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General systems research: quo vadis?

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Introduction

This yearbook in its new format is an extensional definition of progress in general systems research. The papers represent a selection of the key material published in 1978-1979 over a wide range of systems topics. This paper complements them as a collective introduction to the topics rather than a set of individual introductions to the papers. It certainly does not encompass them, for it would be a minor masterpiece of rhetoric to subsume all of the papers in this volume into a coherent whole, and the result would be wholly artificial. Rather, what is presented is an intensional definition of progress in systems research—one person's viewpoint of where we are and where we are going. Such a viewpoint complements the other papers in this volume by giving some rationale for why they are here and of what particular universe of discourse they are a significant cross-section.

The next section is concerned with what we mean by a system and gives a definition of the term which itself demonstrates some aspects of systems thinking. In the following two sections, I consider the role of social concern and ideology in the systems approach and give a systems model for it. The final sections show the significance of presuppositions in systems thinking and give some indication of recent progress.

What are systems?

In a previous paper (Gaines, 1978) on progress in general systems research (hereafter denoted PGSR) I avoided the issue of defining a system. I noted that no definitions are satisfactory, and it seemed to me the essence of the subject area that none can be so. I went on to say that it is the *systems approach*—emphasizing lack of disciplinary boundaries, the freedom to apply knowledge, and techniques gathered in one field to problems in another, or to suggest that two distinct fields are in fact one, the disciplined freedom of the unconstrained intellect—that has been the source of dynamism and progress. I noted that perhaps the most telling progress of all is that we can so confidently speak of a common field of interest knowing that we could not, and would not wish to, agree on a definition of what a system is.

These remarks continue to apply and the papers in this volume of *General Systems* above all exemplify the systems approach with its far-reaching viewpoint ranging across methodologies and disciplines. However, I have also argued the importance of *dialectical* methodologies and of *pluralism* in modern systems theory: that we proceed creatively through contradiction (the opposite of a statement worth making is also a statement worth making); and that we should never be satisfied with one explanation (the universe is generally over-determined and admits many different, and possibly contradictory, bases of explanation). In the interests of fairness and balance it seems reasonable to commence this second paper with a contradiction of the first and an alternative viewpoint. Hence, I shall first define what we all mean by a system, and second argue that there can be no progress in general systems research, only changes in viewpoint.

***Definition*: A system is what is distinguished as a system**. At first sight this looks to be a non-statement. Systems are whatever we like to distinguish as systems. Has anything been said? Is there any possible foundation here for a systems science? I want to answer both these questions affirmatively and show that this definition is full of content and rich in its interpretation.

Let me first answer one obvious objection to the definition above and turn it to my advantage. You may ask, “What is peculiarly systemic about this definition”? “Could I not equally well apply it to all other objects I might wish to define?” e.g.,

A *rabbit* is what is distinguished as a rabbit.

“Ah, but,” I shall reply, “my definition is adequate to define a system but yours is not adequate to define a rabbit.” In this lies the essence of systems theory: that to distinguish some entity as being a system is a necessary and sufficient criterion for its being a system, and this is uniquely true for systems. Whereas to distinguish some entity as being anything else is a necessary criterion to its being that something but not a sufficient one.

More poetically we may say that the concept of a system stands at the supremum of the hierarchy of being. That sounds like a very important place to be. Perhaps it is. But when we realize that getting there is achieved through the rather negative virtue of not having any further distinguishing characteristics, then it is not so impressive a qualification. I believe this definition of a system as being that which uniquely is defined by making a distinction explains many of the virtues, and the vices, of systems theory. The power of the concept is its sheer generality; and we emphasize this naked lack of qualification in the term *general systems theory*, rather than attempt to obfuscate the matter by giving it some respectable covering term such as *mathematical* systems theory. The weakness, and paradoxically the prime strength, of the concept is in its failure to require further distinctions. It is a weakness when we fail to recognize the significance of those further distinctions to the subject matter in hand. It is a strength when those further distinctions are themselves unnecessary to the argument and only serve to obscure a general truth through a covering of specialist jargon. No wonder general systems theory is subject to extremes of vilification and praise. Who is to decide in a particular case whether the distinction between the baby and the bath water is relevant to the debate?

What then of some of the characteristics that we do associate with the notion of a system, some form of coherence and some degree of complexity? The *Oxford English Dictionary* states that a system is “a group, set or aggregate of things, natural or artificial, forming a connected or complex whole.” I would argue that any other such characteristics arise out of the process of which making a distinction is often a part, and are some form of post hoc rationalization of the distinction we have made. One set of things is treated as distinct from another and it is that which gives them their coherence; it is that also which increases their complexity by giving them one more characteristic than they had before—that they have now been distinguished. Distinguish the words on this page that contain an “e” from those which do not. You now have a “system” and you can study it and rationalize why you made that distinction, how you can explain it, why it is a useful one. However, none of your post-distinction rationalizations and studies of the “coherency” and “complexity” of the system you have distinguished is intrinsically necessary to it being a “system.” They are just activities that naturally follow on from making a distinction when we take note that we have done it and want to “explain” to ourselves, or others, why.

Having argued for the appositeness of the definition above, I shall now argue for its content being somewhat greater than appears at first sight. The key to its richness lies in the beautifully ambiguous form of the passive tense in English which allows one to make statements about agency without denoting the agent. The term “is distinguished” omits all qualification about the source of this distinction, and this enables much discussion to be generated by speculation as to the source and the legitimation of distinctions. "A *system* is what is distinguished *in nature* as a system" gives us a Platonic definition of what we might call a *real system* whereby we look to the natural world of reality for the distinctions which give us systems. That is how we used to believe the system of Euclidean geometry originated—it was there and Euclid found it; we only gained non-Euclidean geometries when we started treating mathematics as a game with man-made rules that could be altered rather than with natural laws that were transparently true. Fortunately the shift from essentialism to conventionalism in mathematics took place prior to 1979 so that I do not have to make the difficult choice of whether to report our loss of faith in reality as "progress" or not.

We may make other qualifying remarks: "A *system* is what is distinguished by *scientific procedures* as a system" shifts the weight of the definition upon a methodology of making distinctions that then has itself to be defined (although we often forget to do so, many of us were brain-washed at an early stage to know that science is a unitary body of knowledge based on a unified, and correct, approach). If we take Ayer's criterion of verifiability (Bak, 1970) as a definition of a scientific procedure suitable for defining non-metaphysical systems, then there are very few such distinctions that can be made and systems that can be legitimately defined. However, what systems are left, if any, determine an ultra-positive science. Of course, we do have to define what we mean by verifiable.

If we study the distinguishing agent as a person, then we are defining *psychological systems*, i.e., systems which are defined in relation to the distinctions made by a particular person. This is very different from "psychological systems" as persons under the scrutiny of a decision-making experimenter, and it is only in the *personal construct psychology* of George Kelly (1955) that the two converge. He suggests that we base our notions of psychology on the distinction-making methodology of the person whose psychology we are studying, not that of the experimenter doing the studying. If we study the distinguishing agent as a group of persons, then we are defining *sociological systems*, i.e., systems which are defined in relation to the distinctions made by a group of persons. Systems defined through a group have interrelationships that reflect the structure of the group as being *consensual*, *idiosyncratic*, and so on.

If these be systems then what is progress? In PGSR I again avoided the issue by concentrating only on change. I suggested that perhaps there are only cycles, one of which is a change of emphasis from specialization to generalization. The craft guilds grow and enforce professionalism and consolidation; but, by their very rigidity, they bring into being the fluid forces that will disrupt them. The very concept of progress involves value judgments and hence must be relative to those value judgments. It is the value distinctions that we make that provide the backcloth against which progress might be measured. I cannot envision a value systems for general systems research that is not itself mutable and subject to debate. If we disagree about values and if our own value systems change, then so may the direction of "progress." The papers in this volume of *General Systems* define by extension what we value in terms of current achievement. That in itself is valuable and perhaps all we can ask.

In the next section I shall pursue this question of values in general systems research further and then attempt to model it systemically.

General systems theory and social concern

I suggested in the previous section that the very generality of the concept of a system made the use of such a concept controversial. Workers in a particular field have come to make distinctions that are peculiar to that field and define it for them. To learn to make such distinctions according to the consensus of those already established in the field requires a long and arduous apprenticeship until one finally graduates as a professional in that field, i.e., one who is able to make individual distinctions that completely represent the consensus. To be asked to give up any of those distinctions is both personally and professionally abhorrent. For example, in PGSR I quoted Jacques Monod as one who for me exemplifies the systems approach in his work and yet is critical of general systems theory.

> *What I consider completely sterile is the attitude, for instance, of Bertalanffy who is going around and jumping around for years saying that all the analytical science and molecular biology doesn't really get to interesting results; let's talk in terms of general systems theory ... there cannot be anything such as general systems theory, it's impossible. Or, if it existed, it would be meaningless (Monod, 1974).*

I wonder, somewhat cynically, whether the increasing emphasis on the social value of general systems research that is part of the pattern, if not the progress, in recent years does not stem from such criticism. From the start general systems research has been applications oriented. It is also foundational in the extreme and hence philosophical but differs from philosophy in that philosophers are not expected to make things work. General systems theory is a form of philosophical engineering. On these grounds I would certainly claim Plato's *Republic* as an outstanding example of early work on general systems theory as distinct from philosophy!

When criticism becomes rife, and it has been in recent years, then we are tempted to stress the applied aspects of general systems research. Two major meetings of the Society for General Systems Research in recent years have been dominated, at least in their intent, by active sociological themes: "Avoiding Social Catastrophes and Maximizing Social Opportunities: The General Systems Challenge" (Annual Meeting, Washington, D.C., January 1978); and "Improving the Human Condition: Quality and Stability in Social Systems" (Silver Jubilee Meeting, London, August 1979). In his Presidential Address at the Annual Meeting in Houston last year (1979) Richard Ericson called for an "action research agenda" for the Society for General Systems Research where "action research is that which results from application of transforming concepts and techniques in an ongoing real world organizational context." In his Conference Chairman's Preface to the Silver Anniversary Meeting he reiterates this call and states that:

> *I deeply believe that this society has now thrust upon it a kind of moral imperative to focus efforts on the utilization of general systems concepts and conceptualizations by policy-forming executives, administrators, and managers in all kinds of large-scale organizations.*

At the same time as the Society's meetings are emphasizing social action and the President is calling for action and commitment, there are works appearing such as Lilienfeld's (1978) drawing attention to the implied ideologies of systems theory such as those drawn out in the Habermas & Luhmann (Habermas, 1979) debate on the role of systems theory in sociology. These arguments are reminiscent in their scope and basis of those that have been applied to science in general and its implicit ideologies, such as Tyrrell's (1951), Florman's (1976) and Schumacher's (1977) that the very way we look at things "as a scientist" involves the acceptance of "rules of the game" that must thereafter hide from us certain aspects of "reality" and make us unwittingly accept certain "values" unnecessarily.

I suspect that there will be many who came into general systems research as mathematicians and scientists concerned with cross-disciplinary aspects of their fields of study to whom these ideological and social links are not obvious and to whom talk of "action" and "moral imperative" within this context is very strange. Dare I say that such links and such concepts are indeed strange; that the viewpoint that they do not exist or at least should not exist is an entirely legitimate one; that systems research is the study of abstractions from a range of scientific methodologies with no particular pretence to social worth and promoting no particular ideology? Dare I point to the dangers of becoming "do-gooders" either to humanity or to any particular segment of it in our roles as "general systems theorists" as opposed to any other roles we might personally, or institutionally, wish to play? Perhaps if I put it in terms of seeing nothing in general systems research that makes it constructive rather than destructive, or more likely to avoid social catastrophes than cause them, then there would be little disagreement. However, if I added the carefully excluded middle and said moreover that I saw nothing in it to indicate that it might have any implications whatsoever of a positive or negative nature, not just neutrality but impotence, then this might be more controversial.

The point of this section is that I dare say all of these things—and I dare contradict all of them. There is no simple uniform answer to all the questions raised and there is no real sense in which they can be avoided if we are not to lapse into solipsism. (There goes another value judgment; why should solipsism per se be a "lapse"!) It seems to me that the very questions and issues being raised here are at the heart of science, systems science, and general systems science, and are themselves amenable to discussion within the framework of our own disciplines. The regression involved is unavoidable, the relativity and plurality of the answers obtained is also unavoidable, and some form of commitment is unavoidable if there is to be meaningful discourse even if it is the Pyrrhonian sceptic's commitment to have "no commitment."

Where progress must lie surely is in a systemic analysis of the topics under discussion. I have already indicated a basis for understanding the type of critical comment that Monod makes—*understanding*, not refuting nor substantiating—that of all the sciences general systems theory should be unashamably pluralistic in its models and explanations. Can we understand in systemic terms the role of systems theory in society? Can we model its ideologies, or lack of them? Can we comprehend the different points of view that give different answers as to whether they exist and what they are? In the next section of this paper I shall suggest some synthesis of various systemic approaches that begins to answer these questions.

Surrender and catch

My thesis in this section is that to go beyond being and existence to knowledge and action necessarily involves presuppositions that are ontological (about the nature of being), epistemological (about the process of knowledge acquisition), and axiological (about the values of particular schemata for being, knowledge, and action). Any attempt to "justify" these presuppositions is impossible within the framework for being, knowledge, and action that has been established and, hence, necessarily involves regression into a further framework within which the first is embedded. Such regression can possibly itself continue ad infinitum subject to local circularities in the process.

Such a thesis is probably acceptable in the abstract sense to most readers. However, I feel that few of us are aware how strong the presuppositions actually have to be. We fool ourselves into thinking them far weaker than they are, and are rarely forced to make them explicit. Indeed, by the nature of the problem, we very rarely have an additional framework within which we can make them explicit. Thus those who criticize in terms of "hidden ideologies" are correct in the sense that such "ideologies" necessarily exist, but they are incorrect if they suppose that they are necessarily deliberately hidden or that it is at all easy to bring them out of hiding.

I have taken the title of this section to be that of a book by Wolff (1976) about "experience and enquiry today," which has much in it that is relevant to the theme of this section. In particular his basic model of "surrender" to a situation so that one may "catch" the essence of it—of becoming through surrender someone different in order to catch hold of something different—seems to me to give a valuable insight into the nature of the processes involved in adopting presuppositions in order to achieve knowledge and action. Wolff's book is fragmentary, discursive, literary, even poetic in places, and this is in itself also significant. We shall not catch the essence of *general systems theory* from within its own framework of abstraction.

Wolff s model of surrender is most relevant to the current context in terms of his discussion of the nature of the "mathematician":

> *From the standpoint of the world of everyday life, the mathematician, as we often put it, lives in the "world of mathematics," dealing with "nonreal" elements, notably numbers, whose relation to "real" things, to "reality," is not part of his concern. Analogously for the logician. What makes our subject-object approach to this attitude misleading is the fact that the subject, the student of mathematics or logic—his or her individuality, including motives and attitudes—is irrelevant for our understanding; the only thing that counts is the pursuit, with its results and questions (Wolff, 1976, pp. 162-163).*

He makes the key point that not only does the *real world*, the *object*, disappear to be replaced by the *world of mathematics*, but also that in entering into this world the person *doing mathematic*s, the subject, also disappears to be replaced by a new entity, the *mathematician.*

Wolff was not alone in these insights and there are two further models which develop and complement his. In terms of his "subject," Pask's (1975) concept of "P individuals" as coherent psychological processes capable of engaging in conversations is a useful representation of the results of Wolff's "surrender," particularly when we note that several P individuals may execute within a single processor. Thus "the mathematician" may engage in conversation with "the physicist" or "the statesman," all of whom happen to use the same brain for their processing. In terms of Wolff's "object," Popper's (1968) concept of a "third world" of "statements in themselves" is a useful representation of that which we "catch," particularly when we note its distinct ontological status. He says:

> *I regard the third world as being essentially the product of the human mind. It is we who create third-world objects. That these objects have their own inherent or autonomous laws which create unintended and unforeseeable consequences is only one instance (although a very interesting one) of a more general rule, the rule that all our actions have such consequences. (Schilpp, 1974, p. 148.)*

In her book *On Becoming a Personal Scientist*, Shaw (1980) provides a further insight linking all these into a coherent whole by noting that each P individual may have its own system of "personal constructs" which in Kelly's (1955) terms are "transparent patterns or templets which he creates and then attempts to fit over the realities of which the world is composed" (Kelly, 1955, pp. 8-9). We thus have a model of a *P individual* within a particular person using a particular *construct system* by which to *surrender* himself to a particular *third world* in order to *catch* its essence.

This model of coherent intellectual processes, each with its own individuality, each using its own construct system to enter into an abstract world that has its own reality, provides a very cogent picture of the rationale behind, and the conflicts involved in, the ideologies and commitments entailing, and entailed by, our presuppositions. It goes beyond any simple concepts of "role playing" by providing the basis for formal models of the mechanisms whereby P individuals can enter into an abstract world. The "constructive alternativism" of Kelly provides a model for the extreme relativism of our conception of a world. However, its conventionalism needs counterbalancing by Popper's emphasis on the ontological independence of that world in itself.

Perhaps an obvious question at this stage is as to the necessity of the "schizophrenia" within the person operating a number of distinct P individuals. Even if we accept that this happens, we do not have to accept that it is desirable. Quite the contrary, is not the ideal the "whole man" who integrates together all facets of his existence and is able to avoid conflicts between his being as a "humanitarian" with his being as a "scientist"? I would argue that the basic problem here is one of over-determination. It is an easy one to neglect because we seem to pay far more attention to decisions under uncertainty where we have insufficient determination than to decisions under conflict where we have too much determination. We avoid such conflicts by using construct structures associated with particular world views that filter out all other aspects of the world. To enter in the world of the mathematician is not only to learn to use new constructs that form the key to that world but also to learn to put aside others that would impede entry to that world. This is the way in which the third worlds themselves interact with the person to mould his construct system to make it possible for him to move about within them. In this process constructs may be changed or removed, but this may endanger the capability of the person to operate in other worlds. Hence P individuals are formed within coherent clusters of constructs through which a person enters a particular third world.

Clearly essential conflicts do not somehow magically disappear when a person establishes different P individuals. The overall result is that the conflicts may be avoided within a particular psychological processing unit and isolated to appear at the conversational boundaries between them. Each P individual can then have his own coherence not only in terms of self-consistency but also in terms of mutual consistency with others who ascribe to the same "rules of the game." For example to be a "behavioral psychologist" is to view man in a certain way using a certain methodology and pattern of allowable inferences. Within the framework of psychological science there is an epistemic drive to explore fully all the behaviors of men. As "psychologists" there are experiments which we would wish to perform which involve behavior towards our fellow men which would clearly be unacceptable to us as "humanitarians" (or "law-abiding citizens" or …). How we resolve the conflict between the differing objectives of the P individuals interested in such situations is clearly a significant problem. It has always been a major one for those involved in the science and technology of human life such as clinicians, and books such as *The Social Responsibility of the Scientist* (Brown, 1971) point out that there are undesirable side effects from many scientific and technological activities otherwise regarded as of great value.

The ethical conflicts of medicine and other sciences are real and irresolvable. There are also lesser conflicts which are conveniently treated in the same way. It is conventional for us in scientific papers not to discuss the way in which our family holidays interacted with our experimental program (although it would often be helpful to know which parameters are those of experimental design and which are those of expediency!). If we do attempt to take the "whole man" approach, then our "sciences" assume new dimensions. "$E=MC^2$" might become *true* but *unreported* (due to dangerous consequences). Technically, we can cope with the injection of ethical and other value schemata into science by using many-sorted modal logics to deal with the differing deontic modalities involved. So far we have not done this formally even for epistemic logics, e.g., to treat "unknown" as a logical value of equal status to "true" and "false." In practice, of course, much of the actual argument of scientific debate is based on informal epistemic logic, and in some disciplines such as elementary particle physics and cosmic physics epistemic constraints have come to dominate the interaction between theory and experiment. We are dealing with entities whose time and space scales are intrinsically too small, or too large, for definitive experiments to be feasible.

It is only in the last decade that formal methods for treating many-sorted epistemic and deontic logics have become available (Snyder, 1971). The computational complexity of these treatments is such that the M individual that is a human brain cannot be expected to support P individuals that can cope with them. It seems feasible that computers, or man-computer symbiotic systems, may be able to do so. Indeed this is a natural, and probably necessary, extension of current database systems (Gaines, 1979a); and Belnap (1976) has already suggested that epistemic logics using *told true* and *told false* have a significant role in databases. He also notes that we might wish to go further and tag told true as *told true by X*. Clearly this may be extended to deontic predicates also, such as *felt wrong by Y*, *illegal in France*, and so on.

The development of automated database and inference systems with such capabilities does seem to be of potentially great importance and significance. However, it does not in any way avoid the basic problems already referenced. Any attempt to implement a consensual decision-maker within such a system is doomed to failure in general since it involves mapping many modalities down into a single set of necessity and impossibility. Such a mapping will give rise to contradictions just at those points where there are conflicts: A is *necessary* because A is *desirable* and A is *impossible* because A is *illegal*. The assertion of such a contradiction allows the derivation of any other statement; the decision-maker oscillates or hangs. If we derive consensual subsystems from the database, for example over all mathematicians, then we obtain the "third world" (of "mathematics") within which move particular P individuals (who are "mathematicians").

So what is the world of general systems research? What distinctions do we make in order to surrender ourselves to it and what is it that we hope to catch? I can now refer back to my arguments in the section on "What are Systems?" and note that it is any distinction, as a distinction, that we study as defining a system, i.e., that it is any system of distinctions that forms a phenomenon for us to study, not as "physics," "biology," "religion," and so on, but as a system of distinctions that we are distinguishing as systems theorists. We do not surrender to the world of physics, biology, or religion, but rather to the world of general systems theory which is apart from them, not a part of them; or they are a part of it, but a separate world that gives us a distinct viewpoint of these other worlds. Our “ideology” is to stand apart and say “your universe has these characteristics; by its very nature it will have this feature; through its very presuppositions you can derive this; and by those same presuppositions you can never derive that.” On this basis I would claim Godel’s incompleteness result as a systemic argument applied from *outside* the world of arithmetic to show that the distinctions made in creating that world exclude the existence of certain characteristics for it that, by the value judgments of that world, are not just desirable but *necessary* to it. That is why it is such a shocking result.

Bunge’s (1979) paper on a systems concept of society exemplifies these aspects of the systems approach. He sets up a model of society that abstracts key features of it as a set of relationships between individuals, that is as a system of distinctions. He then uses this to compare and contrast the individualistic and holistic viewpoints of society. The paper has two distinct values to two distinct audiences. To the social theorist it clarifies the relationship between the two approaches to modeling societies; however, only if he accepts the abstraction as having made all those distinctions which he regards as relevant. To the mathematician interested in relational structures it gives a new semantics for certain distinctions in those structures; for him it provides *analogies* with another discipline, another world. However, this paper is neither of the world of sociology, nor of that of mathematics; it mirrors each world in the other and catches the coincidences in the reflections. It is in the world of general systems research that such coincidences themselves take on meaning.

Existence and reality

I would like in this section to be more specific about one type of presupposition that can seem particularly innocuous yet which can be incredibly powerful in its effects. This is the presupposition of *existence*. The great significance of existence presuppositions and existence proofs and the key role they play in mathematics is always something of a surprise to those who meet it for the first time. To go from knowing nothing about A to knowing that A exists may seem a very small step on the path to those who wish to know what A actually is. However, an existence proof is often sufficient in its own right to lead to a derivation of the properties of A and even a construction of A itself. When it is only an existence presupposition then the actual derivation of A is in fact circular although it may well be taken as supporting the original presupposition. We have “caught” A not because it is there but because it was part of our “surrender” to the situation to presuppose that something of the nature of A must be there.

The line of argument involved is of the form:

(i) A exists.

(ii) Any A must P.

(iii) B does P.

(iv) No other entity does P.

(v) Hence B is A.

It is interesting to note that the obvious temptation to put this into symbolic logic in the form of the classical predicate calculus must be resisted. This is because step (ii) is not adequately captured by the statement:

(ii’) $\forall A\ P(A)$,

since we have the standard result:

$$\forall A\ P(A) \supset \exists A\ P(A),$$

that is, (ii’) presupposes (i), whereas (ii) itself is intended to be independent of the truth of (i). We can state that “all unicorns have horns” without having claimed that “a unicorn exists.” It is clearly desirable that this pattern of reasoning be adequately formalized, and Schock (1968) has given a very clear exposition of the problems involved and some of the solutions developed.

Returning to the argument sequence stated above, we can see that its significance lies in the fact that given only that A exists, and that A has the property P, we may find out under some circumstances precisely what A actually is. Somehow the necessity of existence of A has generated a complete ontology of A. The danger is that a false hypothesis of existence can lead through a weak and obvious property to a strong ontological result. The strength of such fallacious reasoning is that the existence hypothesis itself appears to have little content, certainly too little to be responsible for that of the result derived from it.

The classic example of the misapplication of the argument above is:

(I) There exists a largest positive integer.

(II) The square of any integer is greater than or equal to it. The square of the largest integer cannot be greater than it so that it must be equal to it.

(III) 1 squared equals 1.

(IV) No other positive integer squared equals itself.

(V) Hence the largest positive integer is 1.

Only the first step, (I), the existence hypothesis, is false in this line of argument. From the supposition that a largest positive integer exists we have managed to determine precisely what it must be.

I have shown elsewhere (Gaines, 1977) that the presupposition of deterministic causality when modeling phenomena originating from a random process leads to indefinitely complex models, and have suggested that this is a sufficient explanation of the schools of psychopathology originating from Freud’s (1900) original suppositions that there exist causal mechanisms for such phenomena as dreams and mistakes. However, one may reverse this argument and note what a brilliant hypothesis it was that led Freud to treat as causal what others had previously dismissed as random phenomena, and hence to found a new science. Each existence presupposition can bring into being an entire new reality of infinite ramifications in contrast to the original finite event.

These arguments should not be taken as a warning against existence hypotheses *per se*, but only against being unaware of their possible consequences. Each time we “make a distinction,” each time we define a “system,” we are presupposing a difference and having made that distinction we shall find that difference. The worlds we learn to exist in are our own realities, and if we cease to make presuppositions we shall cease to have existence or contact with reality, "the world may seem like shifting sand beneath our feet" (Brown, 1969).

I have traveled a very long way from the discussion of the section on "General Systems Theory and Social Concern" about implied ideologies and calls for action in order to demonstrate that the problems, conflicts, and phenomena involved may be treated within the framework of our own system disciplines. They are "general systems problems." I am quite willing to recurse (or curse again!) by accepting the argument that such treatment may itself involve precisely those presuppositions that one wishes to study or one may wish to disallow. I would not wish to state that general systems research is the *only* vehicle for the study of the features of general systems research, just that we would be foolish to neglect it. Poetry, meditation, and due obeisance to funding agencies may be equally relevant (and their dynamics may also be treated within general systems research!).

Conclusions

In PGSR, in discussing Chiari's (1973) comments on Monod's work, I remarked that his argument is not that Monod's framework is wrong, but that there is room for God within it, and he is right. The proper argument is that the universe *can* be organizationally complete without Teilhard de Chardin, but it also has room for him and his reasoning. It is fascinating to see that in this volume of *General Systems* Kraft (1979) gives an account of the systems approach to energy supply and demand controversies based on Teilhard's work ending with the quotation:

> *Someday, after we have mastered the winds, the waves, the tides and gravity, we shall harness for God the energies of love; and then for the second time in the history of the world, man will have discovered fire.*

This is reminiscent of Henry Adams' (1961) comparison of the hail of dynamos at the Great Exposition in Paris in 1900 where they "became a symbol of infinity…a moral force, much as the early Christians felt the cross," with the Cathedral at Chartres: "All the steam in the world could not, like the Virgin, build Chartres." I have suggested (Gaines, 1979b) that the computer is a source of energy for traversing Popper's "world three" much as was the steam engine for his "world one," and these systemic analogies of worlds of matter, intellect, and religion, and the related sources of energy form a basic theme that recurs in science and literature.

In PGSR I gave a "negative illustration" of progress in systems theory through the problem of time, and made it an open challenge for there to be a session on time at the next conference on applied general systems research. This yearbook contains two fundamental systemic studies of time: Atkin (1978) discusses time as "traffic" on a backcloth of combinatorial relationships; and Winfree (1979) gives a synopsis of key systemic problems arising from his studies of biological clocks. Time is a topic that is fundamental to a wide variety of disciplines and ripe for study in general systems research on a broad front. Hume's demolition of knowledge acquisition as a logical process depends essentially on our notions of time: that we cannot know the future because the knowledge that we know the future is itself knowledge of the future and we would have to prove that we know it in order to be able to prove we know it. That this argument does not also apply to the past depends on our presuppositions about time; that certainty about the past is located in the present but that certainty about the future actually awaits the future. More pragmatically it seems reasonable to devote vast research resources to time travel because even though the probability of success is vanishingly small the rewards for it are sufficiently vast to make the risk/reward ratio highly favorable!

There have been a number of works published in 1979 which do not appear here that mark substantial progress in general systems research. Ralescu (1979) published in the *Society for General Systems Research Silver Jubilee Proceedings* his results on the category-theoretic adjunction between structure and behavior for probabilistic and fuzzy systems which subsumes the earlier results giving such adjunctions for deterministic systems such as finite automata and linear systems. I have used throughout this paper Brown's (1969) notion of "making a distinction" as the fundamental basis of action and knowledge. This notion has been used by Varela (1979) in his book *Principles of Biological Autonomy* to give a systemic account of the living systems on the basis of the intensional definition proposed by Maturana (1975) in terms of *autopoiesis*. Varela's account depends on the extension of Brown's "calculus of distinctions" to legitimize the use of certain self-referential forms and is a beautiful example of the span of systemic thinking across biology, logic, and philosophy.

I have noted elsewhere (Gaines, 1976) that Varela's construction can be extended to give an algebraic model of *degrees of membership* in fuzzy logic, and this is another area where a major breakthrough has been reported in 1979. Before detailing this it is worth recalling Zadeh's motivation for his work on fuzzy logic:

> *It was a biologist—Ludwig von Bertalannfy—who long ago perceived the essential unity of system concepts and techniques in the various fields of science and who in writings and lectures sought to attain recognition for "general systems theory" as a distinct scientific discipline. It is pertinent to note, however, that the work of Bertalannfy and his school, being motivated primarily by problems arising in the study of biological systems, is much more empirical and qualitative in spirit than the work of those system theorists who received their training in exact sciences. In fact, there is a fairly wide gap between what might be regarded as "animate" system theorists and "inanimate" system theorists at the present time, and it is not at all certain that this gap will be narrowed, much less closed, in the near future. There are some who feel this gap reflects the fundamental inadequacy of the conventional mathematics—the mathematics of precisely defined points, functions, sets, probability measures, etc.—for coping with the analysis of biological systems, and that to deal effectively with such systems, we need a radically different kind of mathematics, the mathematics of fuzzy or cloudy quantities which are not describable in terms of probability distributions. Indeed the need for such mathematics is becoming increasingly apparent even in the realms of inanimate systems. (Zadeh, 1962)*

Much of Berlinski's (1976) telling criticism of systems theory is actually of the use of systems of ordinary differential equations to model systems where the presuppositions introduced by such models are very strong and totally inappropriate.

Zadeh (Zadeh & Desoer, 1963) made his own early contributions to the development of mathematical systems theory in the study of the *identification* of such systems and the notion of a system *state*. His introduction of fuzzy set theory in 1965 (Zadeh, 1965) was an attempt to move towards a more appropriate mathematics, and this has been an area of major activity and progress in systems theory during the past 15 years with landmarks such as Goguen's (1968) category-theoretic model of fuzzy sets and a tremendous growth in both theory and applications (Gaines &

Kohout, 1977). Until this year, however, the name fuzzy set theory was not formally justified: classical set theory has a range of results not redeveloped in fuzzy set theory and there was a substantial gap between the two. This gap was dramatically closed in a paper by White (1979) in which he shows that the Lukasiewicz logic on which fuzzy set theory is based gives a set theory in which the axiom of comprehension is consistent in its general form without restriction. This result had been conjectured by Skolem (1957), but White's is the first published proof and, coupled with a previous publication by Maydole (1975) which shows that the unrestricted axiom gives an inconsistent set theory for virtually all other systems of logic, this result is a major event and a remarkable vindication of Zadeh's original proposal. We note that White's result may be restated as: "Any distinction may be assumed to define a system and no paradoxes will arise in reasoning about these systems provide the rules of inference used are those of fuzzy logic."

So general systems theory is alive and progressing, or at least spiraling in an exciting manner. I find it difficult to justify the notion of progress from a systemic point of view (which is the only one available for this paper). Greek philosophy reached a peak which we are as yet only beginning to appreciate, let alone attain. Our acceptance of positive science and axiomatic mathematics as the summa qua non of reasoning lead us to idolize "precision" and reject verbal reasoning. The pendulum is now swinging in the other direction and I wonder if some day we shall not realize that what we have actually achieved is just a clear understanding of what Aristotle actually had to say.

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