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Progress in General Systems Research

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I. Introduction

Reviewing progress within one's own era and area is a dangerous game. Perhaps a gathering such as this is the last place to expect any reasonable appraisal. We are all engrossed in our own problems within our own subfields of systems studies. Progress for any part need not be progress for the whole. And against what backcloth should overall progress itself be measured?

I shall resist the temptation to define a system—there are so many definitions of greater or lesser abstraction and generality—none are satisfactory—and it seems to me the essence of the subject area that none can be so. 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—the approach, rather than the subject area, that has been the source of dynamism and progress. 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.

In this disillusioned and fast-changing age it would be even less appropriate to attempt to define "progress". 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, bring into being the fluid forces that will disrupt them. I shall duck the issues by concentrating on change. What is left of the thought patterns and objectives of the pioneers? What other disciplines have been infiltrated and how have they reacted? What other disciplines have been incorporated and what role do they play? Where are the frontiers now?

This conference itself provides the clearest picture of progress in general systems theory and its applications *per se*: the technical advances in theoretical foundations based on considerations of systems complexity, identification and optimization; the use of new mathematical tools, such as those of category theory, to express these concepts formally, yet with generality, ease of manipulation, and ease of application; the application of general systems theory itself to foundational problems in biology and sociology as well as to more immediately practical aspects of these sciences.

We shall very soon be reveling in this level of detail. Let us first take some time off to ask what it is that gives the various sessions a common theme. I shall argue that it is the "systems approach" rather than any "general systems theory," real or imagined, attained or sought, that is the coherent theme. I will go on to argue that in this we are as much followers as leaders—the systems approach permeates modern scientific thought, even that which specifically denies the relevance of general systems theory—the pioneers predicted and recognized a trend as much as they created and motivated it.

II. The systems approach—progress and the role of mathematics

Probably the real definition of the systems approach must always be extensional in terms of the recognized key works in which it is adopted. We each of us have been influenced by the writings of the pioneers, sometimes by their explicit statements but more usually by their implicit methodologies. For myself Norbert Wiener's (1956) *I am a Mathematician*, followed by Ross Ashby's (1952) *An Introduction to Cybernetics*, and particularly his brilliant derivation in *Design for a Brain* (Ashby, 1952) of habituation as a universal property of systems with many states of equilibria, were the initial impetus, followed by contact with works on automata and games by John von Neumann (1966), Ludwig von Bertalanffy's (1950) seminal paper, Robert Rosen's (1958) treatment of biological systems in terms of category theory (in 1958 well ahead of its time), the 1963 Case Symposium on *Views on General Systems Theory* (Mesarovic, 1964) which included Lotfi Zadeh's (1964) stimulating treatment of an abstract notion of "state", Rudolf Kalman's (1960) elegant results at IFAC 1960 on controllability/observability, Michael Arbib's (1966) exciting presentation of the links between control and automata theory, and surveys such as McNaughton's (1961) "The Theory of Automata", Gibson's (1965) "From Control Engineering To Control Science", and Zadeh's (1962) "From Circuit Theory to System Theory."

I made the list above by browsing through my notes of the early sixties and books and reprints collected at that time. Rereading them now, I am struck by the immediate relevance of the literature some 15 to 25 years ago to the problems and approaches of today—in terms of recognizing the significance of systems theory, its potential for far-reaching impact on all aspects of science and technology, the progress that had been made some twenty years ago. The key indicator of timescale is not the freshness or precision of concept but the weakness and obvious inappropriateness of the mathematical tools employed.

This area of weakness in the mathematics available was also recognized by the pioneers—Wiener and von Neumann's contributions to providing more powerful mathematical techniques are well known—Ashby (1967) put a tremendous effort in later years into applying and expounding the algebraic techniques of the Bourbaki school—Zadeh had clearly in mind, even in 1962, the development of fuzzy set theory:

> "*.... it was a biologist—Ludwig von Bertalanffy—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 Bertalanffy 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 realm of inanimate systems.*" (Zadeh, 1962)

At much the same time as I personally became aware of the systems literature, I was, in a very different context, working through the psychological models of Clark Hull (1943) and Kurt Lewin (1935). Having transferred directly from mathematics to psychology, the total inappropriateness of Hull's use of multiplication of "habit strength," etc., and Lewin's of the vector calculus to represent "psychological fields," struck me very forcibly, and much of my own later work derives directly from the impact of this. How many of us have echoed these feelings of a need for more appropriate mathematical tools in our own work? In a recent survey (Gaines and Kohout, 1977), Ladislav Kohout quoted distinguished researchers from differing fields:

–Vachek (1966), in the context of diachronistic structures in linguistics—*"And it is commonly admitted that mathematical science has not yet developed a formal apparatus capable of expressing what is happening within a changing structure. There can be no doubt, however, that one day such an apparatus will be available;"*

–Hartmanis and Stearns (1966), in the context of automata theory—*"Many of its results show considerable similarity with results in universal algebra, and some can clearly be derived from such considerations. Nevertheless, the engineering motivation demands that this theory go its own way and raises many problems which require new mathematical techniques to be invented that have no counterpart in the development of algebra;"*

–and Aizermann (1975), in the context of control theory—*"unsolved problems…of structural stability, absolute stability, etc. In such areas we do not have answers... a mathematics which should be based on a different set of axioms, a different set of rules of inference, and above all a different concept of precision."*

The difference in the mathematical requirements of general systems theory, expressed by these workers in widely varying disciplines, who have in common only the "systems approach," and "applied mathematics" itself, may be seen very clearly in surveys written within a mathematical rather than system-theoretic framework. Van der Vaart (1961) in his paper on "The Role of Mathematical Models in Biological Research" is concerned with the application of different equations and statistics as they exist—how to use them—how well do they apply. Garrett Birkhoff's (1969) review of "Mathematics and Psychology" is wide-ranging and stimulating, but fragmentary—"this is relevant to this and this to this." Neither survey offers an integrative, underlying theoretical foundation or calls for new developments in mathematics to provide one. Birkhoff emphasizes the importance of application in stimulating new developments in mathematics but exemplifies this in terms of specific techniques not general theories.

In contrasting the approaches taken in these surveys with those, for example, in Klir's (1972) *Trends in General Systems Theory*, Zeigler's (1976) *Theory of Modelling and Simulation*, or in these conference proceedings, one becomes acutely aware of the tremendous ontological commitment of the systems approach—we are expecting and seeking theoretical foundations of great generality in terms of their range of application. The local success of a particular mathematical technique is pleasing to mathematicians and system theorists alike, but it pales in significance for general systems theory unless it can be seen as exemplifying particular aspects of a global theory.

It is clear that key questions we must ask ourselves in judging progress are: "Are the mathematical tools appropriate to applied general systems theory now available?", and "Are we using them?" I would answer the first question with a qualified "Yes"—developments in category theory (Manes, 1974), fuzzy system theory (Gaines, 1976; Gaines and Kohout, 1977), and multivalued (MVL, 1976) and modal logics (Snyder, 1971), have given us new tools of great power and generality—the qualification is that we can never be sure that there are not better ones to be discovered. The second question gets a qualified "No"—we have far to go before these tools are fully and skillfully used—the qualification here is to avoid offence to the many participants at this conference who have been pioneering the use of these tools for many years.

It is interesting to look back to Jean Dieudonne's (1964) survey of "Recent Developments in Mathematics" and see what developments were foreshadowed. Category theory gets the major space but largely in the context of homology. Automata theory is not mentioned (von Neumann gets mentioned for his work on infinite-dimensional spaces!). This typifies the neglect of automata theory in pure mathematics and throws into sharp relief the substantial achievements of Goguen (1973), Arbib and Manes (1974), and Ehrig (1974), in welding automata theory and category theory so closely together in recent years.

There are prophetic remarks in other surveys cited above:

–McNaughton (1961)—*"The domain of application will probably be different: the theory of automata will probably be useful for constructing new machines and new systems, whereas cybernetics will probably be most useful in the analysis of the animal nervous system and very large machines which are already built. There seems to be no reason, however, why these two theories should not coalesce to become one theory, except perhaps there are few scientists who have sufficient command over both logic and statistics to be a force for bringing these two theories together. The same fact explains why probabilistic automaton theory has not developed very far."*

–Gibson (1965)—*"Will control engineering for the next five years follow only those paths that are already well-defined? My feeling is that if we restrict our attention to those problems that are "well defined" from the point of view of the mathematician, we are engaging in a process of self-sterilization. We should emphasize the newer areas, such as the study of self-organizing or learning systems, and move into the grey areas, such as biological control. We should attempt to generalize our field into general system studies from an engineering point of view, not merely from a mathematical point of view…is it possible to formulate our foreign aid policy as a feedback control basis?"*

In the session devoted to Varela, Maturana and Uribe's (1974) theory of autopoietic system, we can see McNaughton's first prophecy coming true: the automata-theoretic construction and analysis of artificial and natural autopoietic *structures*; the cybernetic analysis in terms of behavioral equivalence of teleological aspects of autopoietic *behaviour*; and, most significantly for general system theory itself, the integration of these two viewpoints into a coherent whole.

McNaughton's second remark about logic, statistics and probabilistic automata points to what has remained a major area of weakness until very recently. For example, Goguen (1974) remarks—"stochastic or for non-deterministic machines…seem to be poorly endowed with pleasing theoretical results." This is a situation which has changed greatly since then with the application of computational complexity theory to problems of general system identification. In retrospect I feel that we had become trapped by the statistician's view of probability theory (which is that taught to engineers) and that the very much more fruitful logical and algebraic foundations were insufficiently known, being neglected both in theory and practice. Terrence

Fine’s (1973) book on *Theories of Probability* does much to bring these foundations together and a wide range of technical and philosophical results may be welded into a coherent system (Gaines, 1977). Recent progress in these areas will be apparent in the sessions on systems modelling and complexity.

On a personal note I feel that the theory of *stochastic computation* (Gaines, 1969) which Ted Poppelbaum and I were developing in the mid-sixties has still to find its place in general systems theory. Whereas the engineering developments arising from it have been significant for high-speed analog computation with digital elements (Poppelbaum, 1974), the biological implications for modelling *large-scale* computation at fairly slow speed with simple elements have not yet been fully investigated. Papers on neural mechanisms, even as recent as 1976, seem to have to re-invent stochastic computation (Srinivasan and Bernard, 1976). Hopefully a realistic target for the next decade will be to bring non-deterministic, probabilistic and fuzzy systems, into the domain of general systems theory with results of comparable clarity, power and application to those for deterministic automata and continuous linear systems today. There has been substantial progress towards uncertain systems theory but it now needs consolidation and exposition.

Finally, returning to Gibson’s remarks, they have been truly prophetic of the following decade—artificial intelligence has itself become a major new discipline—biological application of control theory has been fruitful both in modelling and clinical applications—departments of control engineering have broadened out into systems engineering and made substantial contributions to economics and management sciences.

III. The systems approach—an intensional definition

It is probably not difficult to agree on what work clearly exhibits the systems approach and what does not—an extensional definition is reasonably straightforward. However, I not only doubt that agreement could be reached on an intensional definition of the “systems approach,” but would wish to argue that this is itself an intrinsic and important feature of general systems theory.

There have been attempts at the intensional definition of a general systems theory and I would like to develop my theme in terms of a particularly lucid one given by George Klir (1969) in the foreword to *An Approach to General Systems Theory*. What I hope to do is to substantiate the thesis that general systems theory is essentially in a state of dynamic tension, a dialectical method rather than a discipline, whose main characteristic is its freedom to assimilate change at all levels in its own framework.

Klir gives six requirements for general systems theory-

1. *The theory must be based on precisely defined concepts. Vague concepts are not accepted.*

   Given the impact of Zadeh’s (1965) pioneering attack on over-precisiation in systems science and the rigorous formulation of fuzzy reasoning since, this statement now stands in need of clarification. Klir would have in mind the metaphysical and linguistic excesses of some parts of, for example, the cybernetics literature. Now we can see that it is not adequate to attribute these essentially to lack of precision and vagueness. Indeed there are equal excesses in the unjustified over-precisiation of much of classical science—a phenomenon perhaps worse in its impact because the problems created are not so obvious and the approach itself is seen as scientifically impeccable.

Here we see the Hegelian dialectic at work—THESIS (Klir) imprecision leads to sloppy thinking: be precise! ANTITHESIS (Zadeh) over-precision leads to complex, content-less theories: be appropriate in your level of precision! Note that the synthesis is not, as has been suggested in the literature, fuzzy reasoning as a *precise exposition* of vagueness. This is the first step of a regression, not a synthesis—we are faced with artificial precision at the meta-level and the problems remain. No, the synthesis is rather to match allowable patterns of reasoning to the problem in hand. In particular, not to force all problem areas into the Procrustean bed of the classical predicate calculus!

This is the essence of the systems approach, that we can accept Klir's first precept above in the spirit that it was intended without making a dogma of it, and at the same time develop a tool for dealing with imprecision and vagueness. What could have been dogmatic becomes instead a source of dynamic tension: in any particular study are we creating artifacts of overprecisiation on the one hand, or being intellectually lazy in not clarifying our conceptual framework on the other? Worse, are we doing both simultaneously?

2. *There must be no limitations on the application of the theory with the exception of systems with an infinite number of quantities or infinite number of elements. More specifically, the theory must be applicable in experimental science, in engineering, in formal disciplines, and also in such special branches as history.*

This has been the theme of systems theory and cybernetics as expressed by all the key developers of these fields. *No limitation*—von Neumann was equally at home with the values of ENIAC, the mathematics of continuous geometry, the biology of cellular reproduction, the economics of the free market, etc. Wiener emphasized the single domain of man and machine in his definition of cybernetics. Von Bertalanffy's over-riding concern was with coherent underlying analogies between the foundations of all sciences. The sheer breadth of application of general systems theory is not subject to demarcation. Neither is the depth—without artifice, and with a single work, the studies of these pioneers and later workers moves from levels of philosophical abstraction to levels of specific physical and anatomical detail.

There are dangers in this sheer freedom of intellectual movement, dangers of superficiality, of obfuscation, of pushing analogies too far—and the actuality of these dangers may be fully illustrated within the literature! However, it is perhaps again a marker of progress that the knowledge of a general systems approach, and its application, are now so widespread despite these obvious dangers.

There is an aside in Klir's second precept that is interesting in its own right. It marks the distinction yet again between mathematics in its own right, even applied mathematics, and general systems theory. It is an ontological distinction, that general systems theory provides not just techniques but *models*. No "real" observations or constructions can involve an infinite number of elements. Hence "systems" with an infinite number of elements are not necessarily to be included. The lack of limitations is to extend over the domain of physical reality but *not* into the domain of mathematical imagination.

One can see the significance of this argument to workers in general systems theory—applications and reality orientation are what makes it a distinct discipline from mathematics. There has been a marked emphasis on discrete systems theory, e.g. automata, rather than continuous systems. However, again there is a source of dynamic tension—what is reality?—how can we distinguish the imagined from the real?—might not the notion of infinity arise naturally from simple and unexceptional axioms?—certainly the notion of "potentially infinite," i.e. indefinitely extensible, arises in this way—and so on.

Again, there is no dogma—the precept follows from the approach, but it is accepted as a guide to the underlying philosophy. One can begin to see a methodology that permeates the systems approach—"an opposite to any significant statement is itself significant—the synthesis of the statement and its opposite produces a new statement of greater significance than either alone." This is illustrated by Klir's third requirement:

3. *The basic theory should be common for both continuous and discrete systems even if particular procedures must be elaborated separately for each class of systems.*

This in itself has been a fruitful source of systems developments, e.g. in the work of Wymore (1967, 1976), but the fundamental synthesis between discrete and continuous has not yet been achieved. The key progress so far has been in mathematics rather than general systems theory, e.g. Robinson's (1966) non-standard analysis. Perhaps by the next conference we shall have a related non-standard automata theory that achieves a true synthesis. Despite the difference in philosophy and objectives, progress in general systems theory is highly dependent on progress in mathematics.

4. *The theory must be applicable for both the description of system properties and the solution of system problems, i.e. both the descriptional and operational views must be applicable.*

This is the description-prescription axis, the interplay between identification and control, between science and engineering, that has long been a source of dynamic tension in its own right. Again, it is a desideratum that an adequate systems theory account for both requirements. However, it is also a systems problem that, in reality, neither is achievable completely and each involves the other—we have to identify to control and we have to control to fully identify, but in either case, one interferes with the other.

5. *It should be possible to formulate all the fundamental problems dealing with systems with the help of general systems theory. Either the problems must be solvable or it must be possible to prove that they are not solvable.*

Two requirements of the approach are again expressed here. Firstly, that general systems theory must provide its own meta-language—there can be no other discipline not included in the theory that says more about the theory than it does itself—there is no section of the theory that is not itself subject to analysis, question and study—it is inherently recursive—I have already amply illustrated that this is itself a major source of continuing progress.

Secondly, the theory is to be formulated so as to be decidable or decidedly undecidable. This may perhaps be regarded as more appropriate formulation of precept one relating to vagueness. With the vast potential extent of systems theory we need criteria of meaningfulness— decidability, or rather the amenability of a particular theory to considerations of decidability, to provide us with an appropriate criterion—one again itself subject to system-theoretic study.

6. *No a priori classification of system quantities to input and output are needed. The classification must generally follow (if it is at all possible) from the other traits of the system.*

> This is another expression of the requirement for generality and lack of preconception that permeates the systems approach. A significant one, although it is specific, because it exemplifies the way in which systems thinking tends to break down not just artificial barriers between subject areas, but also what, for many practitioners, are fundamental distinctions. However, this breaking down is not the destructive analysis of the Humean sceptic, but rather the constructive development of mathematical reasoning in which what were constants themselves become variables. We do not destroy a distinction but rather seek to characterize it. We do not decree a distinction but rather seek to analyse what it would imply to do so.

I hope these variations on a theme of George Klir (taken out of context with apologies!) will serve to remind us of the nature of the systems approach and of its inherent scope for progress. The approach itself is not new—one can see examples through recorded history. It is apparent in the works of Aristotle (if not those of his later disciples!), of Leonardo, of Eddington, and so on. What characterizes our own era is not the intellectual factors involved, but rather the technology available. We have computers-those before us did not. The tremendous involvement of the pioneers in general systems and cybernetics with computers is no coincidence. What is operational, usable and hence, in general systems terms, *useful*, in mathematical theories has expanded to new horizons, albeit yet ill-defined, since 1945. We, through our intellectual symbiosis with the computer, have a scope for applying, testing, and hence developing system theory, that would otherwise be totally impossible.

IV. Systems theory and the life sciences

It is not realistic in a single paper, or for one person, to give an overall survey of progress in general systems research, let alone an exhaustive one. I shall focus briefly on applications in the life sciences both because these have been regarded as of paramount importance by the pioneers and because it is here that progress has become so significant for our own era, in coming to understand ourselves and our social institutions.

I shall take the rather strange approach of not dwelling upon the contribution of those central to general systems itself. Such examples as the contributions of Bertalanffy to psychiatry are already well-documented (Grinker, 1976), and the current state of the art in biological and social applications will be detailed by others. Instead I wish to exemplify the extent to which the systems approach has contributed to advances in the life sciences on a broad front. It is now only of historical interest as to who, or what, was "responsible" for this permeation. The general systems movement itself has both influenced and been driven by events—the pioneers were leaders but also followers, sensitive to the spirit and possibilities of the era. We can rarely be gods changing the shape of the world. The skill is in prophesy, in being the first to foresee and understand what is to come.

A. Genetics, Chance and Necessity

Developments in molecular biology and their impact on genetics provide a key modern example of the interaction of the systems approach within a specialist scientific discipline. The original discovery of the genetic mechanism and code cannot be seen as peculiarly system-theoretic. However, Jacques Monod's (1972) arguments in *Le Hasard et la Nécessité* exemplify the characteristic role of systems thinking in bridging the gap between advances in a specific discipline and the implications of those advances for the many worlds outside that discipline.

However, Monod himself would *not* wish to be associated with system theory, certainly not *general* system 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)

Given that statement, what justification have I for claiming a system-theoretic approach in Monod's work? One essential dimension is the precise chain which he delineates from precise atomic mechanism through the evolution of life to the nature of consciousness. This is a dimension of depth—of being prepared to derive explanations for explanations, and to build structures of structures. The systems approach uses mathematical interpolation in going from axioms to theorems, but it goes beyond mathematics in asking whence the axioms and whither the theorems.

The second dimension in Monod's work is its completeness, that it has room for both physics and the nature of man. This is a dimension of breadth—of being prepared to find a natural niche for dimensionally, physically, epistemologically, etc., disparate phenomena. It is, of course, for this speculative transition from molecular biology to a philosophy of life that Monod has been widely criticized (Lewis, 1974). It is interesting that much of the non-technical criticism is concerned with the apparently restrictive aspects of the work. Chiari's (1973) argument is essentially, 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!

Monod's approach is not reductionist—the whole *is* far greater than the sum of the parts—but neither is it vitalist—we have a logical and coherent explanation of the whole which involves no extraneous "magic"—"purpose" is neither discarded as a fiction, nor taken as a causative agency—it has its natural place and role. This is not to argue that the systems approach must be all-embracing—Monod's work would not become a better illustration if he had also slipped in God. One can have organizational completeness, a reasonable degree of closure, without being forced to enfold all possible worlds.

To what then does Monod object in general systems theory? A quotation from Bertalanffy throws some light:

> *"General System Theory would be an exact doctrine of wholeness as a 'pure natural science'..., that is, it is a hypothetico-deductive system of those principles which follow from the definition of system and by the introduction of more or less special conditions. In this sense, system theory is a priori and independent of its interpretation in terms of empirical phenomena, but it is applicable to all empirical realms concerned with systems. Its position is similar to that, for example, of probability theory, which is itself a formal mathematical doctrine but which can be applied, by way of empirical interpretation of its terms, to different fields ..."* (Bertalanffy, 1951)

It is the concept of there being "a General System Theory" which is surely the cause of the objection. Many of the biologically oriented papers at this conference and in the general systems literature take positions and approaches closely resembling Monod's. But there is no underlying

General System Theory on which this work is based—it is the *approach* which they have in common. Was Bertalanffy wrong then, are we all wrong, to talk of a General System Theory? I think not—as an analogy, Arthur's Knights of the Round Table did many goodly and wondrous deeds in their search for the Holy Grail, though they never found it! When one feels a common bond, an underlying methodology, a general approach, one looks for a common pattern of thought supported by an underlying language of general application—that is what a General System Theory would be. It need never exist, yet the goal of looking for it may be very fruitful.

I have chosen Monod's work as an illustration because of his very antipathy to general system theory. My thesis is that the systems approach is widely pervasive, not only because of general systems studies, but often despite them. Monod's work exemplifies the systems approach at its best and uses arguments previously developed in the context of "biocybernetics" yet he wishes to deny the relevance of systems theory. I feel his line of argument is a valid and important one—that his theory develops organically out of the specialist field of genetics and is not the result of an external methodology applied from without. Conversely, I feel that a close reading of Bertalanffy would show that he does not criticize this line of argument, quite the contrary—Monod sees only the *normative* role of G.S.T. and resents it—thou shalt make more progress with this methodology. Much of what Bertalanffy is saying is *predictive*—that this type of approach is leading to rapid progress; let us develop and exploit it further.

The normative approach in intellectual endeavour is rarely fruitful, as both Socrates and Galileo discovered. Newton did not bother with it and continued to teach the old Copernican system long after he had developed his own theory of universal gravitation. My quotation above from Bertalanffy, and the extended one earlier from Klir, show them both discussing what a general system theory *might* be-if we are looking, how shall we know what we are looking for?—if we arrive, how shall we recognize that we are there? Once one begins to answer these questions, of course, the answers themselves become subject to study, to examples, and criticism. The answers may be wrong—they will certainly become so—but the *proof* of this is itself a source of progress.

B. Positivism and General System Theory

There is a sense in which general systems theory can be seen as a natural heir to logical positivism. It is clearly possible to approach system theory in a positivist way as a "science of sciences." This is both attractive and will have been the starting point for many of us in generating our initial interest in system theory. The emphasis on strong mathematical foundations, operationalism, equal applicability to the physical and life sciences, the role as both a pure theory and as a practical tool-all these are important and attractive, and are a clear legacy of positivism (Achinstein and Barker, 1969).

Clearly as a "science of sciences," general systems theory is a natural development of positivism. In this respect, in its application to the life sciences in particular, general systems theory must itself be open to the criticism of those who have attacked positivism. I would wish to argue, however, that general systems theory, whilst an heir to positivism that includes it, also *transcends* it in a way that is a reaction to, and an answer to, this very criticism.

Giedymin (1975), in his essay on "Antipositivism in Contemporary Philosophy of Social Science and Humanities", distinguishes six independent aspects of positivism:

(a) identification of knowledge with science (natural and social) and mathematics, to the exclusion of other areas, e.g. ethics;

(b) empiricism in the extreme form of either phenomenalism or physicalism, i.e. the reduction of science to statements about directly observable facts and the elimination as meaningless of any sentence that is neither analytic nor empirical, e.g. of metaphysics;

(c) the reduction of philosophy to the "logic of science" (philosophy of science) and of mathematics;

(d) methodological naturalism (naturalistic methodological monism), i.e. the view that the social sciences and even humanities have basically the same aims and methods as the natural sciences;

(e) sociological relativism with respect to norms, in particular ethical ones;

(f) the emphasis on the social value of science and on its practical applications.

Reading through each item in turn, is it not clear that general systems transcends positivism in accepting *all* of these as significant and meaningful statements but attaching a *truth-value* to none? We can develop a system, a possible world, which is that of physical science and which is complete, such that the notion of ethics has no meaning—such a system is significant and important. It is an *elemental system*, purified from the raw material of actual science and its progress. However, we can add to that system an ethical one, e.g. of value judgements about directions of research, and study its compatibility, dynamics of the resultant system, etc. Conversely, we can develop theories of ethical systems, elemental in themselves. However, we can also take an ethics expressed in some form, and seek, without changing that form, to imbed it in a system that represents its accurately and concisely—in so doing we may well generate new mathematical tools and new system theories.

The very exclusion of the non-operational as metaphysical is itself a system-theoretic construct. System theory can be used to analyse, logically, precisely and completely, the implications of this philosophical position. On the other hand, a completely viable system-theoretic account of metaphysics that do not involve adopting this position is possible.

Ultimately this transcendence must itself be seen as stemming from increased generality—what was once a tenet, or a dogma, itself becomes a variable element subject to study. It was its very status, as tenet or dogma, worth holding as such, that later makes it worth variation. However, generalization is often thought of as a passive activity, a spread of definition or a weakening of boundaries. I have argued strongly that it is instead dialectical in nature, involving thesis, antithesis, and synthesis—generalization in this sense transcends the forces originating it.

Giedymin's specification above is itself an illustration of the system-theoretic approach: six independent binary decisions lead to sixty three brands of positivism—studying each in itself, and the relationships between them, clearly transcends both positivism and anti-positivism.

A clear case history of such transcendence at work is the Adorno/Popper debate and its continuation, published as *The Positivist Dispute in German Sociology* (Adorno *et al*, 1976). What was intended to be a confrontation between positivism and critical rationalism in fact comes too late and the participants are disappointed to find that disagreement on points of substance is so difficult to create. There is a tendency in the exposition to blame incompatibility of terminology, lack of clarity, and an agreed common language, for the fuzziness of the supposed dispute. To the outsider, at this historic distance, the positions taken up appear to be just that-the emphasis on differing aspects of the same coherent whole.

I mention this particular debate for I feel it has great significance to the general systems movement. It is a precursor to the more recent Luhmann-Habermas debate in German sociology about the role systems theory plays in sociology (Habermas and Luhmann, 1971). It is clear, however, that what is criticized in systems theory is, as was the case with the critique of positivism, a very narrow interpretation of the methodology. The critical rationalism of the Frankfurt School (Jay, 1973) has itself a significant role to play in systems theory, one which many of us have come to realize implicitly through our own studies in particular areas. For example, the contrast between a society in which one tries to enforce policies by the manipulation of behaviour, stimulus-response and reward-punishment networks, and a society in which one generates the same behaviour by presenting the logic of the situation to its members in such a way that they act in the required manner of their own volition, has always been clearly present in the experiments and writings of Gordon Pask (1975).

Pask has studied and demonstrated, in automated instructional systems, both the technical cognitive (using the terminology of Jurgen Habermas, 1972) approach to instruction in which the learner is a behavioural object to be manipulated, and the *emancipatory cognitive* approach in which the learner is an equal partner to whom structures may be exhibited. He has stressed the multi-level nature of learning and training, and demonstrated the significance of the higher levels. Many of those who have been involved in general systems theory and cybernetics have developed applications in education. Over the last twenty years we have seen the swing from "programmed learning" and "teaching machines" to "computer-based learning" and "learner-controlled instruction." This swing from control to participation has its technical foundations in systems theory, but its psycho-social foundations are most clearly expressed in the work of the Frankfurt school.

Recently Brian Melville has argued that one significant feature of Ron Atkin's (1974) "q-analysis" of system connectivity, is that it presents not a technical solution to a problem but rather a cognitive map allowing this "emancipation of cognition" (Melville, 1976). Another striking example recently is in the psycho-therapeutic work of David Mulhall (1977) where patients with problems of interpersonal relationships are presented with graphs (essentially state-transition diagrams) showing the expected patterns of behaviour based on their own, and the other person's reactions to various situations. Without any other "therapy," this presentation of information is itself sufficient in many cases to effect the re-shaping of behaviour necessary to improve the situation.

V. A negative illustration—time

Highlights are often created by contrast—awareness of the real progress in many areas can be heightened by comparison with the stark lack of progress in others. A class of problems of fundamental importance that have so far been inadequately treated in systems theory is that of time. There are many books on the physics, philosophy, psychology, etc. of time (Fraser, 1968; Gale, 1971; Zeman, 1971; Gold, 1967), but they present fragmentary and isolated arguments. The systems approach is lacking, although the very range of subject domains which pretend to contribute to the study of time is a clear indicator that a systems approach is required.

Whereas the cosmological models of Ryle and Hoyle exemplify systems thinking at its best, comparable work on time falters at a level of specific detail that is too low for solutions to be achieved. The remarkable collection of papers and discussion edited by Gold (1967) is fascinating reading in its own right, and as a case study of science in progress—the detailed reporting of the discussion sessions is particularly illuminating. However, it is very clearly a statement of problems, not solutions—indeed it is clear that lack of a framework for the clear presentation of many of the problems is a key factor in the lack of solutions. The depth of these foundational problems is apparent also in philosophical, rather than physical, studies of the problem of time (Freeman and Sellars, 1971).

What then are the problems of time? Let me state just one: *why do clocks keep the same time?* The variety of time-measuring devices concocted throughout the years is fantastic—what do the burning candle, the swinging pendulum and the disintegrating atom have in common? The answer is nothing—except time!

If you tackle this one straight on, you will find yourself in a realm of tautologies (pendulums keep time because they are in a stationary gravitational field, i.e. one exerting constant force on inertial mass, i.e. one that pendulums keep time in), of disconnected sciences (chemical reactions will take place at the same rate regardless of gravitational forces—what have oscillations under nucleonic forces got to do with oscillations under gravitational forces?), and among the greatest thinkers throughout the ages.

If you find a system-theoretic answer that keeps everything time-locked, then account for the fact that real systems do not keep exactly in step. If that is exactly what you predict, then account for the exact relationship between gravitational and radiational time. And so on.

I hope the illustration makes the point that in some areas (of fundamental importance to systems theory), there has been no progress. Apart from that, let me offer the problem of time as an open challenge for the *next* Applied General Systems Research conference. If there is then no session on the problems of time, I hope there will also be no paper on “progress” in general systems research!

VI. The frontiers

Looking back over this paper, I am pleased to see that it leaves everything rather more confused and muddled than it might have appeared to be when I started. General systems theory is an uncomfortable area in which to work, and if you do not feel discomfort then you are probably working somewhere else. Philosophy is similar in this respect, but differs in that philosophers are not expected to make things—certainly not things that work-general systems theory is a form of philosophical engineering.

One source of discomfort that we should have is the state of the physical sciences. The foundations of particle physics, cosmology, gravitation, electromagnetism, and so on, are at least as shaky now as they were twenty years ago (Korner, 1957). Do not be fooled into thinking that quantum mechanics is played out just because so much has been said about it—these are fundamental system-theoretic problems in physics and many frontiers are still to be explored (Bastin, 1971; Audi, 1973; Nierlich, 1976). I sometimes wonder whether our new focus of attention on the life sciences is not partly because we find it easier to accept that we do not understand people—we have been brainwashed into believing that we actually do understand things, and find it too disconcerting to admit that we do not!

The advances made in the mechanization of both deductive and inductive reasoning have also created an important new frontier. Theorem provers for both the classical predicate calculus (Chang and Lee, 1973), and for non-standard calculi (Morgan, 1976), like the Lewis-Langford modal logics (Snyder, 1971), have opened up new possibilities of automated deductive inference. Again, deductive reasoning is often felt to be barren territory, but in fact automation of non-standard deductive systems is an essential step in the automation of induction. This is particularly clear in the great advances made by Hajek and his school in the computer-implementation of practical inductive systems (Hajek, 1975; Hajek and Havranek, 1977; Havranek, 1975). The associated studies of non-standard logics and set theories are an integral and necessary part of this work (Vopenka and Hajek, 1972).

Automata theory and formal languages were seen as major components of the theoretical foundations of systems theory by most of the pioneers. Despite the great effort devoted to derivation of mathematical results in these fields, however, the practical contributions have been disappointing until recent years. However, the successful studies and applications of Lindenmayer systems (Rosenberg and Salomaa, 1974) in biology would now justify all the previous theoretical efforts even if there were no other applications. The basic concepts hark back directly to those of von Neumann's cellular automata but we have acquired in the intervening years an impressive armoury of structures, results, and computational techniques, that have suddenly come together to give spectacular progress (Herman and Rosenberg, 1975).

I feel safe in predicting similar progress in the application of systems theory to ethological studies of animal behaviour. A number of recent conferences have shown workers in that field finding immediate applications of automata, language and hierarchy theory to experimental data (Aronson *et al*, 1970; Bateson and Klopfer, 1976). So far, unlike the development of Lindenmayer systems, this does not seem to have called for extensions of systems theory itself. If so, what has changed? I believe it is the psychology of experimentation itself. Theoretical developments that are not subject to experimental test are of no great interest. The new availability of low-cost laboratory computers gives the ethologist the ability to carry out data analysis on a scale not previously possible. What were previously interesting speculations are now becoming operationally testable theories.

Moving up from cell biology through animal behaviour one comes naturally to linguistics! Here the Chomskyan revolution has promoted an interest in formal language theory that has been very fruitful in understanding syntax (Fodor and Katz, 1964). In recent years this has been extended to semantics (Fillmore and Langendoen, 1971; Jackendoff, 1972) and, most recently of all, to the acquisition of language (McNeill, 1970; Slobin, 1971; Derwing, 1973). There is still a gulf between the theory and natural language, but the advances of the past twenty years have been very great and show no signs of slowing down.

Computer science is a subject area that did not exist twenty years ago—some doubt that it exists now! However, in recent years the development of theoretical foundations for interacting processes, virtual machine hierarchies, data-base structures, and so on, have seen the integration of systems theory with practical experience to produce at least the beginnings of a science. This has enabled many of the concepts employed to be integrated into a category-theoretic unity.

Kohout and I have emphasized the scope for system-theoretic studies of dynamic protection systems and their relationship to human action (Kohout and Gaines, 1976). Database theory and practice is already heavily involved with classical logic (Date, 1976; Sundgren, 1975). I see this as one of the most immediately fruitful areas for the application of non-classical logics, both fuzzy and inductive. When sending out a call for updates to a bibliography of fuzzy system studies recently, it was interesting to note how many requests for copies came from those in the operating system and database areas.

If there is one area of activity that combines aspects of all these frontiers, then it is "artificial intelligence." Here deductive and inductive logics, linguistics, psychology, and computer science come together to develop artifacts that are—us! In the early days of systems theory and cybernetics, artificial intelligence seemed as integral component of this field. In recent years there has been a strong tendency for an "artificial intelligence clique" to develop who deny these origins, or their present relevance. I see the key reason for this as the switch from "learning" systems to "performance" systems some ten years ago. The argument that, "you cannot expect to build a machine to learn a skill if you cannot at least build a machine to perform that skill," has substantial validity—it certainly helps to know that the goal is attainable! The performance-approach has proved very much more difficult than expected but has had the encouraging success of the work of Terry Winograd (1972) in recent years, and much artificial intelligence research continues to expand and enhance that line of development (Schank and Colby, 1973; Simon and Siklossy, 1972; Bobrow and Collins, 1975; Norman, *et al*, 1975). However, we are also seeing a return to the original question in terms of, "how could the performance built into Winograd's system be acquired through learning?" (Sussman, 1975; Harris, 1977). Artificial intelligence research has also made significant contributions to psycholinguistics (Miller and Johnson-Laird, 1976; Fodor, 1976).

The gap between systems theory and artificial intelligence is far more apparent than real. If it persists, then it is due to misunderstanding on both sides. There is no such gap, for example, in what is commonly regarded as another major achievement of artificial intelligence research, the MYCIN project. Shortliffe's (1976) book on MYCIN brings together results in artificial intelligence, systems theory and philosophy, and demonstrates their practical application in the subject domain of *Computer-Based Medical Consultations*.

There are many frontiers I have not mentioned, world models in economics, man-machine symbiosis in data-analysis, architecture and engineering, the system-theoretic analysis of major legal systems, and so on. There is evidence of progress in the number of journals which now incorporate the word "system" in their titles, many of which have done so as a change in recent years reflects the actual change in their contents over many years. For the past three years we have had our own *International Journal of General Systems*, setting new standards for system studies.

Finally, if all my emphasis on tension, change, and the general lack of definition of this field appears to cast doubt upon, and undermine, the efforts of the many of us who are developing "general systems theories," who are consolidating, defining, and formalizing the notion of a system, then the point of this essay has been missed. Those who put up the scaffolding to enable a building to be constructed make an essential contribution to the building itself. Some buildings are so immense that the scaffolding itself needs scaffolding to enable its construction! We are far away from seeing the actual shape of the final building. The scaffolding we erect is, however, already adequate for many purposes. Because we know that we shall dismantle tomorrow what we have erected today, it does not mean that it did not serve its purpose. Progress in general systems theory will always involve change and destruction as much as it involves application and construction.

We must all be grateful to George Klir for bringing us together in close proximity for this week, and giving us the opportunity to wreak wholesale destruction on one another. I hope the opportunity will be well taken, and we shall remember Kenneth Boulding's (1964) remark, *"the willingness to make a fool of oneself should be a requirement for admission to the Society of General Systems Research, for this willingness is almost a prerequisite to rapid learning."*

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