A SPECIAL ISSUE
DEVOTED TO POSSIBLE CRISIS IN CYBERNETICS AND GENERAL SYSTEMS THEORY

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EDITORIAL

Is There A Crisis of Identity in Cybernetics?

At a recent Planning Meeting of the American Society for Cybernetics, the question was twice raised: "What is Cybernetics?" Unfortunately, no agreement was reached. Similar situation exists in General Systems Theory. A. Rapoport, after 22 years of editing General System, came to the conclusion that: "General Systems is not a well defined `field'..." If, only a few years after the creation of Cybernetics, our inability to identify the field could be attributed to the "illness of growth," now, over 30 years later, it may indicate more of an "organic illness." Such lack of clear definition is also a handicap to further development. Cyberneticians applying for research funds who offer different interpretations of their field can hardly expect to establish credibility for themselves or their projects. Cybernetics would acquire its own identity if we could show that it represents a unique field of knowledge with rather clear boundaries which separate it from other related fields.

All fields of knowledge can be distinguished by the method of inquiry used and by the subject matter studied. The scientific method in pure sciences is used for understanding the properties of particular systems, while in applied or engineering sciences this method is used for providing information about the design and construction of systems of desired specifications. Although this method is common to all sciences, the way it is applied depends on the type of the system under study: a chemist analyzing a compound by acting upon it, and a political scientist using public opinion poll to predict the outcome of an election are differing in applications of the same scientific method.

The most characteristic feature of Cybernetics and the source of its strength lies in the application of its method—the only method of purposeful activity which is used in any transformation of a controlled system from its initial into its goal state. The decision of the controlling subsystem, selected from its repertory of decisions and executed by the executive subsystem corresponding to its repertory of possible executions, transforms the initial state of the system into a changed state, which, after being compared with the goal state, may require another decision, etc., until the goal state is reached or modified. Control is the heart of the cybernetic method, and information is the blood flowing between the organs—the parts of the controlled system through its arteries—the channels of communication.

Obviously, in order to establish a goal state and to have repertory of decisions and their executions, both the controlling and the executive subsystem should first be aware of the properties of the controlled system. Thus, application of the scientific method proceeds that of the cybernetic method. However, a person, after studying chemistry, cannot design a chemical factory. She or he also needs to learn chemical engineering. In other words, pure science does not supply information in the form needed for effective application of the cybernetic method. This is one reason why social sciences do not have a significant impact on the political process, as shown by R. A. Scott and A. R. Shore in Why Sociology Does Not Apply: A Study of the Use of Sociology in Public Policy.

The scientific and the cybernetic method lead to different kinds of models. After inductive application of the scientific method results in accumulation of data, the scientist suggests a hypothesis explaining newly discovered and previously known relationships between the studied parameters and predicting new relationships in the form in which they can be verified. A hypothesis is a scientific model of a system. Narrowly specialized sciences dealing with a specific, relatively small parts of the natural (geosphere and biosphere) or artificial (societal, technological, and other man-made systems) environment may use a great deal of mathematics in their models. Every complex system is, however, an "insufficiently described system." Therefore a cybernetician, trying to build a model of a brain, learner, or world system, cannot succeed without indicating "degree of insufficiency" ignored in his or her model and the way to control it. Frequently, cyberneticians do not realize the limitations inherent in a mathematical model of even less complex systems. The complex reality cannot fit into the straight-jacket of a few differential equations. An analysis of the cybernetic method by which a societal system actually operates may give more practical information than any model of the system. For example, a war or a revolution can be best explained as a specific breakdown in the application of the cybernetic method by the controlling and executive subsystems of a given social system.

It is not difficult to see that the more abstract the information about a system is, the less effective is the application of the cybernetic method to the system. For example, the goal of education at a
philosophical, i.e. abstract, level was stated by Plato as the “education of a good man.” Different interpretations of “good” at the level of policy making led to five national educational systems which stressed different goals: Education for citizenship (the U.S.A.); Education of elite (France); Education for systematic knowledge (Germany); Education of character (England); and Education of a professional (the U.S.S.R.). Of course, no country totally ignored the other aspects of education. At the practical level, the goals were restated in a concrete form determined by a particular school, curriculum, teacher, students, school budget, etc. Thus, the cyberneticists, operating at a level which is too abstract, run the risk of not finding a path towards an application of their findings to concrete systems. This does not mean that we should not try to understand cybernetic processes at a higher level of abstraction since it may provide us with new insights, but that we should not forget that practice is the supreme judge of the correctness of any theory, and that we as a profession are judged by our verifiable achievements.

Let us turn attention to subject matter of Cybernetics, i.e. to the domain of systems operating by the cybernetic method. The first living cell which appeared on our planet about three billion years ago was the first object to which the concepts of information, communication, and control could be applied. A system of such immense complexity (for example, a single E. coli cell consists of about 100 billion atoms) naturally requires a special controlling subsystem. No geospherical system needs control from outside, because there is no goal state different from the actual one. Planets in their motion around the sun do not deviate from the path specified by the Law of Universal Gravitation. Therefore, we should reject attempts to “reduce” complex, cybernetically controlled biological, psychological, and societal processes to much simpler physical ones. Thus, Cybernetics can easily establish its identity vis-a-vis all those sciences whose systems do not operate by the cybernetic method. Cybernetics should also emancipate itself from the engineering sciences, which deal with relatively simple, deterministic systems. Interchange-
"One begins to appreciate the value of modern education only when one hears an expert speaking on a topic outside the area of his specialization."

—Karl Kraus

I suspect that we are witnessing not so much a "crisis" of cybernetics as rather simply a collapse of the inflated claims that have been made for it. Just as the American world position in general is rapidly collapsing, and reality emerges through the fog of rhetoric, something similar happens in specific areas of endeavor such as cybernetics. The two developments are even related: the American belief in technical, i.e., a-historical, devices and solutions for what are inescapably historical dilemmas, has operated throughout the era of American ascendancy; these devices, whether they be aid programs or servomechanisms, do not deliver what is promised. "Crisis" may be another term for the hangover accompanying a cold grey dawn.

Cybernetics did represent, after all, a genuine though modest advance, and automated machines and factories do operate, more or less effectively. The inflated claims that were made, the sterile pomposities offered by Norbert Wiener and the other founding fathers, their promises to solve the moral, social, and political problems of the world—all of these claims more than a little self-serving—had to lead ultimately to disillusionment. But for a time they rode a very high horse indeed, producing a vast utopian literature.

Countercurrents

Even during the rosy dawn of the entire systems movement beginning in the 1950s, there were dissenting voices. Robert Boguslaw, in his prescient book, The New Utopians, was possibly the first to recognize the authoritarian and utopian temper of the new systems men, and his critique was well thought out and elegantly written. But his book remains subject to an important objection: like the systems men he opposed, his book contained no empirical materials, nor did his criticism even suggest the possibility of any test of the claims of the systems theorists.* It remained for others to approach the matter empirically.

Ida R. Hoos, in Systems Analysis and Public Policy (1962), was the first to address the problem; systems ideas are all very well (or rather, not very well; they are a potpourri of ill-digested materials gathered from a variety of fields), but what happens when they are put into practice? Focusing for the most part on the State of California, the government of which seems to have been especially prone to funding social programs based on the systems/cybernetic approach, Hoos thoroughly documented the failure of the systems men to deliver according to their promises.** Hoos' formidable critique seemed unanswerable and unanswered; about all that could be said by way of comment was that Hoos focused almost exclusively on California, and on the operations research sector of the systems movement, giving only minimal attention to its other branches.

My own approach, in The Rise of Systems Theory (1975), was to survey the origins of systems theory within a variety of separate disciplines: economics, political science, sociology, psychology, communications theory, and the like. Within each of these fields, scholars and thinkers found, or claimed to find, that the systems approach was a fruitful advance over earlier theories. They then proceeded to extend the claims of systems thinking beyond the limits of tightly defined technical problems, offering them as a solution to social, political, and philosophical problems.

I then traced the migration of the systems vocabulary out of these isolated subject-matter fields into the new attempted syntheses, of which the most prominent was the GST of Ludwig von Bertalanffy and Ervin Laszlo. From there, the movement emerged as a popular ideology in such forms as The Limits to Growth, the Club of Rome document, and the urban and world models of Jay Forrester. My entire criticism of the systems/cybernetics movement can be summarized as follows:

I. The claims of GST to be a source of new knowl-

* Not a rare phenomenon, but an interesting one: the opponents of a dominant stream of thought, the "antis", often have more in common with their opponents than they fully realize, or would wish.

**But then a State which elects Jerry Brown to prominent office and fosters other cultural manifestations, might be expected to show unusual forms of gullibility.
edge and insights not available from concrete work on clearly defined problems have not been validated. Aside from declarations of the interrelatedness of all things, nothing new has emerged.

II. The systems approach offers nothing for the advancement of work on philosophical problems.

III. The attempts at the creation of artificial intelligence, or of models of the human brain via computer simulations or network systems have yielded no anthropological insights into the nature of man. Nor have the psychiatric-psychological-clinical therapy branches of the human sciences made any advances on the basis of systems/cybernetic proclamations.

IV. The claims of the systems/cybernetics men to offer the definitive solution to political and social problems such as war, crime, pollution, urban decay and irrational political strife, all of which would now yield to the new approach, have revealed much of the narrow, unhistorical mentality of these technicians, especially their astonishing ignorance of the genuine achievements of many figures in social science. But more than that, their achievements have been as meager (actually, nonexistent) as their claims have been inflated. Scientists from every field are unwilling to admit that human society remains curiously and blessedly intractable to approaches borrowed from the sciences.

V. The very few instances known to me of practical applications of cybernetics/systems theory/operations research to economic and social problems have simply not worked. This is too mild a statement: they have been expensive—and even frivolous—disasters. The systems men for the most part favor general proclamations and programmatic declarations while avoiding specific, concrete problems. The "promising youth" of these fields threatens to become their oldest—and only—tradition.

VI. These approaches exhibit an unhealthy fascination for exercises in vocabulary construction as a substitute for either thought about the world, or observation of it. This, I suggested, was a hallmark of the 20th century, a mostly stagnant interregnum during which technologism has operated as increasingly diffident regent. Its regency was made possible only by the exhaustion and discrediting of previously regnant political ideas: the Divine Right of Kings; the Social Contract; the Invisible Hand of the Marketplace; the Dictatorship of the Proletariat; Government of the People, by the People, for the People. All such images have lost much of their ability to inspire either belief or action; they survive, but mostly in the rhetoric of public speechmaking. In such a vacuum, the cybernetics/systems approach might appear as an answer: an approach based on statistical, acturial, or algorithmic procedures might seem promising for a time.

VII. The stifling authoritarian potential of the cybernetic/systems approach was noted by many writers; it's general effect could only be in the direction of increasing the dead hand of bureaucratic centralization on previously functioning business organizations, government agencies, and economic and urban systems. I would only add that the crisis of confidence in the cybernetics/systems approach does not stop its practitioners (who may no longer even be believers) from pressing its authoritarian potentials wherever they can. Its emphasis on the inter-relatedness of all things within the framework of a whole directed and controlled from a brain center is, of course, the very old metaphor that likens societies, organizations, and the like, to organisms, of which we are the directing brain center, while others are the bones and muscles obeying our commands and sending us appropriate feedback.

VIII. I concluded, then, that the cybernetics/systems approach functioned only on the ideological level, as the mystique of a rising class of technocrats who saw themselves as philosopher-kings, or perhaps as scientist-kings, who would usher in a new era if only the present holders of power would turn over the reins to them and their methods. Thus, I saw the cybernetics/systems approach as a glorification by themselves of the work habits and modes of thought of a narrowly rational, more-or-less educated, group of technicians.

David Berlinski's On Systems Analysis (1976) took a somewhat different approach. His caustic and sometimes witty critique of systems theory focuses mostly on the internal logic of the various attempts to formalize a systems approach in, perhaps, a set of differential equations, or in some other mathematical or pseudo-mathematical notation that decorates so many pages of the systems literature.

He is merciless towards the major documents of the cybernetics/systems literature; his survey contains descriptions like the following: "... a flabby and pretentious book..." (Ashby's Introduction to Cybernetics), "... pages and pages of soggy philosophy" (Norbert Wiener's Cybernetics), "... a collection of coughs in the night" (Walter Buckley's Modern Systems Research for the Behavioral Scientist); "... there is much in his style of prose composition that calls cotton wool to mind... he is not an author of passionate scrupulousness" (David Easton's writings on political science); "... pile up an impossibly complex system of equations and then subject them to an analysis of ineffable innocence. It is a natural prescription for theorists ignorant of differential theory" (Jay Forrester's World Dynamics, and The Limits to Growth); "... there is no suggestion whatsoever that beyond the loose, informal, and disorganized bevarede there stands a set of precise mathematical concepts" (Morton Kaplan's System and Process in International Politics).

I hasten to add here that I have quoted only a few of Professor Berlinski's judgments, most of which
have been preceded by very detailed and scrupulous analyses of a number of systems formalisms, which he groups under three major headings: General Systems Theory, Dynamical Systems, and Mathematical Systems Theory. But the general drift is clear: the mathematics is often ceremonial or downright erroneous; where the models make sense, their applicability is doubtful.

Professor Berlinski concludes his survey with the following rueful paragraph:

There may be a moral in this record of grand efforts brought low by insufficient means, some cagey bit of philosophical wisdom. But aside from the obvious counsel that in great things great ambitions without great theories are insufficient, I do not know what it is.

* * * * * *

I should like now to offer a few hesitant words that might suggest, for Professor Berlinski and the cyberneticists, just what the moral is. But before doing so, there is a small bone to pick.

First, Professor Berlinski aims a barbed shaft at the social sciences; speaking of cybernetics and information theory, he says:

Both subjects are shaped about real theories, but their applications have taken place in the paraplegic disciplines—sociology, psychology, political science, and management science—a sure sign of debility. Certainly the positive results have not been impressive.

Speaking as a sociologist who is often irritated with the state of the social sciences, I would suggest to Professor Berlinski that the true paraplegics are the mathematicians and engineers who think to become dilettantes of the social sciences on the basis of some new technical or formal advances in their fields; along with them go the social scientists who think to become dilettantes of mathematics and logic, and who take Science (with a capital S) more seriously than it deserves.

Second, Professor Berlinski often has his bit of fun by quoting sentences from the systems theorists which might better have not been written; thus, he quotes such gems of wisdom as the following:

"Ours is a complex world" (Ervin Laszlo.)

"Carthage was destroyed by the Romans. This is called system destruction or dissolution." (Morton A. Kaplan.)

"An automobile which runs out of fuel will not function" (Oran R. Young).

"The analogies between a living organism and a machine hold true to a remarkable extent at all levels at which it is investigated" (Jean Pierre Changeux).

Now, I am grateful to Professor Berlinski for displaying these howlers drawn from the systems literature, but I fear I have found a sentence of his which almost belongs in the same category: "A social scientist studies social systems to get at their laws" (David Berlinski).

This statement is possible only for someone who has never heard of such thinkers as Wilhelm Dilthey, Max Weber, or R.G. Collingwood, to name just a few, or who conceives of social science on a positivist model drawn—I was almost going to say, from Auguste Comte—not so much from Auguste Comte as from J.S. Mill's simplistic version of Comte's philosophy. Both the cyberneticists and, it would appear, some of their critics, would do well to learn something of the philosophical struggles which, since the late 19th century, have attempted to come to terms with historicity and the peculiar problems it generates, which have taught many of us "paraplegics" that the notion of "laws" of society analogous to "laws of nature" is a will-of-the-wisp long since abandoned. We remain aware that the problems of the social sciences are bound up with the unresolved problems of dealing with society in the full amplitude of its concreteness.

* * * * * *

... Only certain truths, which do in fact appear unique and exclusive, possess 'absoluteness'; these are abstract truths. But there is no difficulty here: abstract truths do not refer to reality, but to diagrammatic renderings of reality; i.e. to certain elements or aspects of it (idea means aspect) obtained from a fixed point of view and with deliberate elimination of other points of view—that is precisely what abstraction consists in—and in such a case truth must naturally be univocal and exclusive. What has happened is that... logic and the theory of knowledge have been accustomed for centuries to work, with surprising pertinacity, on abstract truths, and have hardly ever descended to thinking about the concrete, which presents much greater difficulties. And the result has been to consider as a mark of truth something which only applies to a very special and secondary class of truths, and which only has meaning for the consideration of that "de-realized" reality which appears when man devotes himself to a task which is highly specialized and not at all basic in his life... disciplines like logic and the theory of knowledge would assume a very different appearance if they were to state their problems in all their radicality and amplitude, so that the present forms of these disciplines would be reduced to mere chapters corresponding to certain particular cases.*

Our systems thinkers, and unfortunately many philosophers as well, are unable to confront the fundamental error in social science involved in retreating from full concreteness in favor of abstract schemes. The abstract schemes may be any or all of the many "systems," or may be some simpler theories of social causation relating society to "underlying" causes such as technological or economic developments, but all such attempts do involve a retreat from reality in favor of logical or pseudo-scientific schemes which gain manipulability at the
cost of reality.

All such schemes are involved with “causal explanations;” but causal explanation has to be schematic and universal, leaving out the concrete individuality which is “the most real part of the thing” explained;** we are faced then with a whole new region of problems. Is it possible, then, that “explanation” is a form of knowledge that is both secondary and defective, and that another, and neglected, form of knowledge must precede it? This neglected form of knowledge would be something called description, something which phenomenologists, historians, and even social scientists, have been attempting.

In their own groping way, I think some cyberneticists have stumbled across these problems; some of their abstract schemes appear to be attempts to capture in formal terms the parameters of (comparatively) specific configurations of factors in social situations, but a basic confusion of thought leads them to generalize these in more schematic directions away from the concrete. They have not had much help from either their allies or their critics, many of whom appear to share the same errors.

But it appears to me that the fundamental error of the systems approach is this basic philosophical confusion of attempting either to retreat from the concrete in favor of manipulability, or to generalize specific configurations. The quantifiers have for a long time assaulted the social and historical sciences; the latest attempt, that of the systems/cyberneticists, has ended in the same sort of failure. The failure will not be reversed by bigger and better computers, by more variables, by better and newer mathematics, but by a somewhat chastened awareness of the historicity of all forms of knowledge, and of the need to confront social reality in its full concreteness. When this awareness emerges, the cyberneticists may even discover that some disciplines have been there ahead of them: these are the humanities.

New York City; February, 1980


** Ibid., p. 145.
Introductory Notes

Theorizing is an important part of the cognitive contents of science. Every scientist is an observer, and as an observer he is continuously busy attempting to organize and order his observations, regardless of the nature of the phenomenon observed, be it parts, facts, numbers, data, etc. Our theories convey the interpretations formed in our minds to other members of our society. We try from time to time to publish our theories in articles, essays and textbooks partly to boost our egos, and partly because by reporting our observations we invite support, review, and criticism.

It is with this framework that my evaluation of Lilienfeld’s theory deals. Dr. Lilienfeld(1) observed a phenomenon; organizing his findings he published a theory: The rise of system theory—an ideological analysis. To attempt to enumerate the value and the damage that this theory contains, we must analyse it in terms of contents, explanatory value, predictive power and consistency.

The well known philosopher of science Karl Popper(2) habitually attempts to explain the growth of knowledge, by the use of a tetradic ‘schema’:

\[ P_1 \rightarrow TT_1 \rightarrow EE_1 \rightarrow P_2 \]

The existence of a research-problem (P₁) will cause the emergence of tentative theories (TT₁), attempting to provide an explanation to the problem. The scientific process of review by error elimination (EE₁) may eliminate most of the theories, or may fail to refute them, but will contribute to the emergence of a new research problem (P₂), which may be the solution to the original problem, but could be also a reformulation of the problem. And of course the cycle starts anew . . .

This is the model that I will use to evaluate Lilienfeld’s theory.

In his preface (starting in page 2) Lilienfeld states that his theory is based on his interpretation of books and articles, in which system technicians (an ambiguous term) make philosophical and societal claims on the basis of their technical work. In an arbitrary manner, I will consider this to be the problem that Lilienfeld addresses. (Though, it is possible that Lilienfeld considers his theory to be part of the error-reduction mechanism, the societal claims to be the tentative theories, and system theory to be the problem requiring explanation.) By assigning to Lilienfeld’s theory the role of a problem (instead of a refutation technique) we are approaching it from the point-of-view that is habitually called “second-order-cybernetics”: our interest is not in the observed systems (system theory and its refuted claims) but in the observing system (Lilienfeld and his interpretations). Thus, my intention in these notes is to contribute to the growth of knowledge, as defined by Popper, and to transform, by a review, Lilienfeld’s theory from a valid verification tool to a more proper and appropriate position of problem.

As stated; Truth, like contact lenses—is in the eye of the beholder.

Lilienfeld’s theory (and I shall refer to it from now on as the theory) appears to be constructed on the basis of the sociological model, described by Piet Thoenes as “the role of the elite in the welfare-state.” Since I am not familiar with the original model, I will summarize it based on Lilienfeld’s description (p. 271) as a three stage evolutionary process: “The announcement of the message, the car-
rying out of the plan set forth in the message, and the conservation of order achieved by carrying out the plan."

The similarity between Thoenes's model and Lilienfeld's theory is presented below in a table form:

<table>
<thead>
<tr>
<th>Thoenes's model stages</th>
<th>Lilienfeld's theory</th>
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<tr>
<td>stage # name</td>
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<tr>
<td>1. The announcement of the message</td>
<td>I The origins of systems theory</td>
</tr>
<tr>
<td>2. The carrying out of the plan set forth in the message</td>
<td>II Societal claims of system thinkers</td>
</tr>
<tr>
<td>3. The conservation of order achieved by carrying out the plan</td>
<td>III System theory as an ideology</td>
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Table 1: Isomorphism between Thoenes and Lilienfeld

(As a personal note, I found in the construction of the table above that it (the description) offered me an aesthetic delight that is its own justification, just as Lilienfeld said (p. 191).)

The existence of a correspondence between Thoenes and Lilienfeld may provide us with a speculative clue, that the strength of the author's attack on systems theory can be explained by the threat that the emergence of a technocratic elite poses to Lilienfeld's world. The selection of isomorphism as a model for the theory will be discussed later on, when I shall attempt to parry Lilienfeld's attack on model selection within the discipline.

The main thrust of my attack on Lilienfeld's work should be directed towards the concept of "carrying out the plan, set forth in the message." If we can refute an intent to use, or rather abuse knowingly systems concepts to advance the discipline, while contributing to the detriment of society, we then can disconnect the link between systems theory, and its perverted usage by a technocratic elite, to pursue its own aims.

This approach will also enable us to avoid the circular arguments about elites, their aims and the consequences to society, a topic that lies beyond the horizon and the scope of this paper and could be regarded as highly speculative, to say the least.

The first part of the theory, organizes the work that was done in the early stages of the discipline. The author is reviewing the "announcement of the message" by presenting concise summaries of early works in General System Theory and the other disciplines that emerged from it or converged to it: Cybernetics, Information Theory, and Artificial Intelligence to mention only a few. These summaries enable a layman as well as practitioner to grasp the main themes and models, while avoiding the rhetoric of most of the seminal works. The clarity expressed in the summarization of these historical documents becomes very dangerous in the other parts, since it establishes a sense of belief in the author, without differentiating between his summary-ability and his original arguments.

Unfortunately, the components of the theory of general-systems that were selected by the author present a very restrictive picture of the origins of the field. The concentration is on the earlier works of Von-Bertallanfy, Laszlo, Klir and their contemporary, It seems that all development stops abruptly and that more current contributors to the field were omitted. I found the omission of Warren McCullough worrisome, since it implied lack of recognition of his ideas, in the development of the field, and ignorance about the influence of McCullough on more current development, such as Experimental Biology (Maturana and Varela), the Biological Computer Lab (Heinz Von-Foerester) and development abroad (Gordon Pask and Ilya Prigogine). The rationale for such omission is that the author concentrated on textbooks and other published works directed to the general public, while avoiding the more technical sources, based on their lack of missionary-content.

The selection of works to be criticized based on the ratio between their technical contents and their missionary-contents, with greater appreciation to missionary works, is biased. It is biased because of lack of criteria to separate technical work made-simple, to increase the number of people that develop appreciation to the problem, from work made complex due to its author's desire to achieve a position in areas other than those of his competence, by masquerading his personal opinions as his expert opinion. A scientific article in the Scientific American is an appropriate example of the first kind; an attempt to write an article that will call for the election of a cybernetician as a president, due to the capabilities implied by cybernetics, will be considered an appropriate example of the latter, if written by a politically ambitious cybernetician, using technical terms to obscure his frustrations or lack of knowledge in political affairs. Since the author elected to selectively define theoretical "cornerstones" he consciously decided to bypass the criterion of meaning, and instead of filtering the "work-made-simple," from "work-made-complex" he grouped them together. This lack of interpretation of his subject matter, or his biased interpretation may have contributed to the selection of his model.

By clearing my impressions about the first part of the theory out of the way, I am ready now to approach the subject of societal claims that were originated by systems thinkers, and were found to be worthless by the author.

Is system theory a theory?

In sumning up the societal claims of system thinkers (another ambiguous term), Lilienfeld states
“System thinkers exhibit a fascination for definitions, conceptualizations, and programmatic statements of a vaguely benevolent, vaguely moralizing nature without concrete, or specific references to historical, social, or even scientific substance...” (p. 191)

According to this argument, the definitions, conceptualizations and the like which should be considered as the “product” of system-thinkers cannot be anchored into any concrete substance. My counter argument is: why should they? Scientific development, emergence, and evolution (pick your own term) is a dynamic and therefore unpredictable process. The foundations of what will be discovered tomorrow are not anchored in any concrete substance today, as a matter of fact they cannot be anchored into a substance a priori, otherwise what is there to be discovered? Stafford Beer’s adage: “absolatum-obsoletem” (if it works it is obsolete) can be verified by viewing all the famous cases of “concrete anchoring,” like the position of Galileo’s detractors, in the question of his telescope: their objections were anchored into substantive evidence, but were they right? Does the physics textbook, which is anchored in “reality” present the frontier of physics? I don’t think so. The determination of “substantive” or “lacking substance” is subjective judgment, anchored in an individual’s perception of his world, and nothing more.

We can view any discipline as containing two non-intersecting domains: that of what was proved already and verified by repetitive experiments, and that which contains unproven hypotheses, unsubstantiated claims, conceptualizations, and often conflicting theories. We will view the former as the enclosure of the domesticated problems, and the latter as the enclosure of the “wild” problems. Furthermore, unless our interest is in the history of the discipline, we will strive to become associated with the set of unsolved problems, and conceptualize some hypotheses that may reduce the uncertainty in a particular field. But can we criticize the preparatory work that precedes discovery as lacking in substance?

What is unsubstantive and lacks references today, may be verified tomorrow, and once accepted by the majority of the practitioners, will become the core of some future textbook, where it will be considered both well-anchored, and removed from the repertoire of current research problems.

System theory is relatively a very young discipline, and as such it lacks consensus on definitions and conceptualizations. Its immaturity, is the prime contributing factor to the fact that most of the work done until now, is within the domain of the “wild” problems, while very little can be found that can be considered as “verified.” But if early statements, which in this case are the basis for the argument, are mistakenly considered as an ultimate result, a poor assessment of any field is inevitable.

A theory is never a complete and faithful description of “reality;” it is little more than a model or an analogy. Theories imply that out there, there are relations, there are logical operations that are similar with what the theory purports to explain, but the “similarity” is of such vagueness that theory can not be fully identified with the problem it explains.

A theorist does not attempt to prove, nor assert the verisimilitude of his theory, nor does he attempt to anchor it in referential or concrete substance, this will be determined, later, by further research and experimentation, and most likely by others.

It is within this framework that “system thinkers” and their product should be validated, as M. Polany(3) states:

“These powers enable us to evoke our conception of a complex inaffable subject matter with which we are familiar by even the roughest sketch of any of its specifiable features. A scientist can accept therefore, the most inadequate and misleading formulation of his own scientific principles without ever realizing what is being said, because he automatically supplements it by his tacit knowledge of what science really is, and this makes the formulation ring true.”

Is the search for isomorphic models reliable? Do these models contribute to scientific development?

A second argument is being pursued by the theory, namely:

“They (system thinkers) collect analogies between phenomena of one field and those of another (preferring to call them isomorphism, though the difference is not discernible), the description of which seems to offer them an aesthetic delight that is its own justification.”

(4)

It is amusing to discover, in retrospect, that Lillenfeld’s theory seems to be similar (my way of avoiding “isomorphisms”) to another theory (see table 1); could it be that the author, in formulating his own theory, conveys a message: “do what I say, not what I do?”

My counter argument to the theory’s suspicion about the intent and the usage of analogies in system theory is: Why not?

Let me present side by side two approaches towards the solution of a hypothetical problem; one will call for a selection of an analogy in different fields and its usage to formulate a solution, or at least tentative theories that will explain the problem at hand; the other will call for formalization of the problem by axiomatization (which is nothing more than borrowing a mathematical analogy). What can
we assert about these approaches?
When one is to “borrow” an existing model and transfer it to another discipline, one must always remember that the model being drawn from another field of knowledge than that to which it is to be applied, carries a certain amount of pre-existing understanding of its properties. An attempt to evade the consequences of such understanding could be fatal. If this ignoring of pre-existing understanding is Lilienfeld’s main complaint, we have no argument at this point. But this is a case of “bad” science, and we must remember that system theory (like any other discipline) includes both good and bad science. If this is not the major reason for Lilienfeld’s objection, we will proceed with our initial example. If the trap of incommensurability between the model and the problem that it was brought to solve was avoided, then I found that the knowledge conveyed in this transfer goes further than the assembly of techniques used to solve mathematical problems, because the model contains elements of both intuitive and experimental understanding. To exclude such understanding, by axiomatization of the model prior to its application, would eliminate some insights about the subject for which the hypothesis was originally proposed.

Analogies enable us to gain insight without sacrificing details and variety, and the act of collecting analogies is generally accepted not only by system thinkers, but among scientists in other disciplines too.

All of our knowledge is stored in an archive, where all information pertaining to a particular field of knowledge can be found in a specific niche within this archive; there is a niche for every specific discipline. All members within this discipline share this knowledge and cooperate in its verification and validation as a prerequisite for increase of the knowledge content of the field. Transfer across discipline boundary is not common—neither in our model of knowledge nor in the “real-world.” Thus, disciplines adopt characteristics similar to those of “closed systems,” minimizing interactions and communication with adjacent disciplines. With the passage of time, this closure results in a state where the experts in a field become so indoctrinated in the current set of paradigms that their critical and imaginative progress comes to a halt. It happened to physics prior to the discovery of relativity, it happened to geology before the advancement of the continental-drift theory. No discipline is immune to such disease. This static state of affairs will be reversed when intellectual intruders appear through the interdisciplinary boundaries and look at the field without pre-conceptions. The facility that separates these intruders from ordinary scientists is not innate nor is it genetic mutation. It is the result of being trained to look for models in other niches, and the skills acquired while transforming solutions across boundaries. The skills acquired and the training contribute, according to Ziman(6), to the task of keeping all scientific disciplines in touch with “reality.”

Models should not be equated to mathematical techniques, though in some cases similarity or even identity relationships may exist. Models are the basis for any system research programme. We use them to formulate tentative theories, and as our critical analysis tools. We even incorporate into them any knowledge gained and any increment in understanding that was identified during our observations, hypothesis formulation, and verification.

As far as the aesthetic delight associated with analogies, I always consider it as the reward of “discovery” to its discoverer, or the sense of elation that usually succeeds the tension and frustration of research. The rewards of a discovery are common to all human activities, to science as well as art. There is no reason to expect system thinkers to be different from society, or to behave differently. It is the fortune of system theory that the implicit use of models enables its practitioners to enjoy the aesthetic delight in their discovery with greater frequency than in other areas of human activity. Unfortunately Lilienfeld’s theory misses completely in this point.

The universality and abstractness of system theory
Another of the theory’s anti-system arguments is that:

“System theory achieves its self-encompassing ‘Universality’ only by its very abstractness: All things are systems by virtue of ignoring the specific, the concrete and substantive”(4)

My counter-argument is: so is Topology, group theory, and the physical theory of ‘black-holes.’

A theory can convey a significant amount of information, without being metrically accurate. This is known as Qualitative knowledge. Is such knowledge unsound because it is not quantitative?

Of more significance is the question: Does a theory correctly represent the significant relationships between identifiable entities in the subject matter? Relationships often can not be resolved nor described by ‘counting’ or ‘measuring.’ Consider the following argument(7):

To the scientist there is no confusion in treating an atom as a tiny sphere, in the kinetic theory of gases, yet knowing that its spectroscopic properties require that it should be thought of as a horribly complex cloud of interpenetrating electron orbitals buzzing around a central nucleus. The real question for the scientist . . . is whether the level of generality is appropriate to the problem at hand, and whether the details are then sufficiently accurate to solve it."
‘Systems description’ is nothing more than the abstraction of the ‘real’ problem, the intention is to describe some phenomena (as components) and the relations among the components. This is not a deliberate attempt to avoid the specific, the concrete and the substantive, but an approach that guarantees that the specific will not lead us “to lose the forest on account of the trees.”

The universality of the term “system” should be evaluated within the context of science per-se. System theory was derived from what is generally acknowledged as modern, or western science. Modern science is monolithic and monopolistic, having successfully eliminated all competition in the 17th century. It is in this frame that we consider science as universal. The universality of science can be demonstrated by the facts that all physicists share common paradigms and communicate among themselves by the use of their consensual language. It will be correct to conclude that scientific universality was forced on us by the structure of science, and the system theory only adhered to what was expected from it to pass from parascience into scientific status.

Let me elaborate on the statement that universality is forced by the structure of science. When we become involved in scientific activity, be it learning the rules of a specific scientific discipline, or experimenting and verifying the current paradigms, we must use a consensual language to communicate. A communication of knowledge contains both ambiguity and variety, since it is being handled as a subset of a natural language. An investigation into the existing cultural differences in the structure of natural languages will lead us to view what was referred to before as ‘universality’ as nothing more than the result of a ‘schooling’ (to differentiate from learning) process that forces all written languages to conform to universal standards of content and structure. It is this form of language-writing, deliberately taught in our schools, that develops ‘abstract’ thought as characteristic of scientific standards. Abstraction played a significant role in the development of mathematics, and the significance is the fact that abstractness is related to the more advanced stages of evolution of mathematical concepts: first there were the quantities to be measured and counted; in its evolution, the discipline discovered and introduced numbers—a more advanced (and abstract) form of the basic quantities; only at maturation did the concept of “number” emerge—more abstract and more universal.

I maintain that “universality” and “abstractness” are complementary relations, through which complex problems can be analyzed and, hopefully, solved. The inability to appreciate the power of abstractness is unfortunately a problematic deficiency of those who did not acquire the mathematical foundation necessary to operate on an abstract level.

System philosophy: Subservient to science? Passive? Should it be?

According to Lilienfeld’s argument:

“If we review the history of philosophy we must conclude that philosophy was subservient to religion during most of the epochs. During the middle ages, the Catholic church labored under philosophical (theological) difficulties that remained unsolved for a long period of time, while philosophy remained passive and subservient to the church.

Did the theological difficulties impair scientific development, or did they serve as what Kuhn will refer to as “the essential tension” between innovativeness and secretarianism that opposed innovation, using dogma as its most powerful and restricting tool? Did any attempt by a religious or other bureaucratic organization ever succeed in stifling radical changes in the way that scientists perceived ‘reality’? Is it not true to state that the scientific progress that we enjoy today, either directly as scientists or indirectly as members of society, is the result of this “subservience”?

Using the same analogy, to present the case for system theory, we can state that due to the changing picture of society at, or around the time that systems-theory was conceived, the themata selected by the pioneers as starting point was not religion but the physical sciences, at that time the most successful enterprise of western science.

System theory remains subservient to epistemological problems posited upon science, by virtue of its dependency on science as its themata. To define this dependency as “passive” becomes therefore a matter of style, not content.

Using the Popperian model I can present the argument as follows:

Can it be that we consider the unknown solution to the problems as “system theory,” where the solution is shift in the original problem based on changes in the scientific thinking that is ‘subservient’ to philosophy? And even if system philosophy could transform itself from ‘passive’ to ‘active’ role, how could it free itself from ‘subservience’ to the sciences?
Failure of system theory or failure of theory applicators?

Repeatedly through the theory, the author builds a case that is based on what can be considered as a major ‘weakness’ of system theory or the minimal success that the theory achieved in the solution of problems in science, as well as in social arena:

"Where it failed (system theory) in application, we are told that the theory is fine but the application was poorly carried out. This of course draws the rebuke that if a technique is not judged by its application how can it be judged?..." (p. 192)

Let’s start the response to this argument by constructing an iconic model of the relation between theory and its application; let’s consider the theory (T) as the model of a particular problem, let’s consider the application (APP) as the programme carried, based on the theory, to an end in the ‘real world.’ It is clear that between the theory and its application we must introduce a critical component—the applicator (A) as the person, group or machine that uses the theory as a map to “cook” an application (technique). Graphically our iconic model will look like:

![Diagram]

The interlocking role played by the applicator in the model can point us to the starting point of an analysis, the application is considered either as a ‘failure’ or as a ‘success.’ Therefore we can divide the model into two components: The relation between theory and the applicator, and the relation between the applicator and the application.

In general, Lilienfeld has some value to his criticism; it is not the ultimate truth of the system picture that matters, but the answer to particular questions posed by ‘system thinkers’ that count. But I will try to be more specific in discussing both the questions and the answers.

When we have a theory, the most accepted manner by which we can either test it or use it is through experimentation, verification and validation of the results. In the world of the physical sciences this can be performed in a laboratory, using elaborate instrumentation, in a controlled environment. Since the subject matter slowed considerably its evolution, experiments can be carried out with relative ease, and with the ability to predict (because of the cessation of evolution). In the social sciences, on the other hand, test and experimentation are restricted due to non-controlled environment, lack of instrumentation and constant dynamics of evolution: “You can not step twice into the same river.” The appeal of system theory to social scientists was in the ease by which its usage can bypass some of the restrictions mentioned above.

The implicit usage of metaphors within the frame of system theory provided possibilities to social scientists to formulate problems and research programmes by transforming models that were proven in other disciplines.

Therefore the lack-of-success that the theory pointedly emphasizes, must be considered within the possibilities of mismatch between the model and the problem, or mismatch between the model and the current paradigms of the field where the experiment took place.

Any theory that is general, this is to say abstract in the sense that the more you have to agree on it, the less content there is, can not and should not attempt to verify itself (see my early comments about the role of theory). It should be used as a model, map, or better album of maps to be studied not as an ultimate truth but as way-points that the applicator can use, provided that he verified the model by his intuition, imagination and the conditions of the problem at hand.

The relative youth of system theory affected the process by which a practitioner became a full-fledged member of the discipline: there is no institutional standard of education, and no agreed-upon learning processes by which paradigms can be verified and validated via communal consensus. This freedom to declare oneself as a system thinker has contributed to the field by drawing numerous brilliant individuals who gained formal training in different disciplines, as well as charlatans who felt that the lack of formal procedures and consensus will enable them to establish themselves as members of the new discipline. All that is required from an individual to declare himself as a system thinker is his ability to use system terms, in proper or improper situations.

It is the interpretations of the theory into operational terms that is wholly applicator-dependent and can not be checked, nor verified by committee of his peers. The interdisciplinary nature of the models used leaves the verification of a model to the applicator; if the applicator abuses this opportunity to farther his own ends and aims—it is the applicator’s failure and not the theory’s.

The relation between the applicator and his application is based again on the applicator’s ethical level and his education as to what is considered ‘bad’ science and what is considered ‘good’ science. If one camouflages his own narrow opinions by attempting to force a model on a problem—it is a case of ‘bad’ science, whether in genetic research or in system theory; however, if one attempts to study a model in order to better understand the magnitude of the problem (like Stafford Beer did in Chile and Canada) this is ‘good’ science, though it does not guarantee the results in terms of desirability or validity. We can learn from our mistakes more than from our successes.
The theory is confusing the term 'system analysis' with system theory repeatedly. This categorical mistake of assuming that any application of system theory in the social arena must adhere to the usage of large scale digital-simulation, contributes more than any other factor to Lilienfeld's confusion. The attempt to utilize system models in social problems is relatively young and maybe could be considered a posteriori—immature, but at least it provided us with important lessons about the limitations of system theory, and with evidence that more basic research is needed in system theory before it could consider itself as capable of solving social problems.

It is these instances where theory fails to provide explanation, that signify the beginning of "paradigmatic shift," which may or may not provide better theories and explanations of 'reality.' If this is the case, then in spite of lack of understanding by applicators, system theory may prevail, and only time will judge whether these 'failures' are indicative of the inapplicability of the system theory (Lilienfeld's position) or just a single episode in the area of scientific development (the position that I take on this issue).

Can we judge mathematics as a falling theory, and geologists as poor applicators, only because of the fact (now, only historical episode) that during the debate about the theory of continental-drift the theory was poorly used? Was this a case of poor theory or poor application? Of course we can not categorically maintain that it is the fault of the theory. Why, then, should we single out system theory?

In Summary: System Theory—parascience or superscience?

The consistency of the theory is grossly impaired by the author's attempt to oscillate between two contradicting opinions about system theory. One, that prevails in the first two parts of the theory, is that system theory is nothing more than a parascience. The other is that by its emergence as ideology system theory (in the last part) is superscience (scientist-king, due to the system ideology). Both opinions are the result of two-valued logic, I maintain that system theory is somewhere in between.

Parascience can be viewed as a fringe of ideas, surrounding the boundaries of institutional science, waiting and seeking formal recognition. Its propagators in many cases are asking for more than being heard, they want to be accepted without passing through any verification attempt. According to the theory, this is the position of the system thinkers, who attempt to predict and promise social solutions due to their systemic insights, but fail to deliver.

The second position, that of superscience, is not grounded in any serious system theory work, it is a combination of interpretation of early seminal work, outside of context, coupled to speculation that system theory may be put to work to the detriment of mankind, by a technocratic elite. This speculative part evades the thought that no scientific theory is immune to misuse of inappropriate interpretation. According to this vision it is the fault of the medium, if it is used for antisocial purposes.

In both versions of the value of system theory, it is considered by the theory as dangerous to organized knowledge and free society. Social scientists are singled out as prime candidates for coercion, since by adopting system terminology they lower their skeptical guard and neglect the consequences of the theory.

It seems to me that para- and superscience are two non-intersecting domains, therefore system theory can not reside in both at the same time. Any attempt to create an intersection of those two separate domains will result in contradictions.

The scenario that seems more feasible to me is that system theory started as a parascience, like any other new, immature and dynamic discipline, but gained recognition during the last decades and was incorporated into the archives of science knowledge (maybe to Lilienfeld's distress). I doubt whether system theory, because of 'newness,' could provide a sound ideology to any power-hungry elite group, whether it is scientific, social, political, or bureaucratic. This is precisely because of its dynamic evolutionary nature. Assume for example that an elite group adopted system theory as an ideology during the peak of the closed-system paradigm, will not the shift from closed- to open-system paradigm affect the political ramifications of such ideology faster than the ability of the elite group to adjust its position to reflect the change?

Any attempt to associate ideology to scientific theory raises some questions about the motive that lies underneath. Lilienfeld appears to play the role of proxy for mankind, and guardian of scientific reliability and social freedom. This is quite a demanding commitment on his side. One may suspect that such commitment is limited to the influx of system ideas as long as they will conform to his paradigms of verification, validation, and prediction. This, carrying the "Galileo mantle," is a dangerous position for any scientist to assume.

If his intention was to make rational assessment of our ignorance on the particular application of system theory in the social sciences, by identifying enigmas and raising consensual questions, his model could initiate a healthy discussion and dissent in the field. But the methodology used in the construction of his theory affected its explanatory powers, its prediction potential and its consistency.

I have some criticism about the current state of affairs in the discipline: the disappearance of real system thinkers, our recurring attempts to concentrate on professional societies instead of society and its problems, and the universal misuse of terms
and models by applicators, all cause me discomfort. But I view these deficiencies as applicator-oriented phenomena, and not as weaknesses of the theory. We are, after all, human. My last observation is: I have used in this paper definition, conceptualization, analysis, isomorphic models, abstract, universal and some philosophical terms, in short all the arguments that Lilienfeld raised against system theory and the people associated with it. If all is wrong and misleading the fault is mine. If all is right and appropriate, all credit is mine also. Systems theory did not create these notes, this writer did.

REFERENCES
4. Lilienfeld, p. 191.
Review* of Berlinski's
"On System Analysis"**

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The dustjacket describes this book as follows: "Here is a book of uncompromising negativism. Systems analysis, David Berlinski observes, is largely a sham, such content as it has involves nothing more than the purely ornamental uses of mathematics." (Emphasis in original.) I regard this as a fair assessment of the book. It is indeed a vigorous attack on Systems Analysis in its many manifestations (such as General Systems Theory and World Dynamics), prosecuted by an author who is both a Professor of Philosophy and a competent mathematician.

To say that Systems Analysis is largely a sham is strong language. Many would reject the book at the outset on that basis alone. However, the author's qualifications, as evidenced by both his credentials and his writing in the book itself, are such that his arguments cannot be rejected lightly. It is therefore worthwhile to see what he says, why he says it, and how much truth there might be in what he says.

In this review I will first synopsize the book, since it is impossible to comment on Berlinski's arguments without first summarizing those arguments. Since comments on one portion of the book might also be relevant to other portions of the book as well, I will save almost all my comments until after the synopsis. In that way the book can be treated as a whole.

The book contains three chapters, representing three basically different approaches to the demolition of Systems Analysis. Each could stand on its own as an independent essay, but the three together provide mutual reinforcement for Berlinski's basis thesis that Systems Analysis is a fraud and is in a state of intellectual bankruptcy.

The approach in the first chapter is to quote passages from several authors who have written on General Systems Theory (GST), then to demonstrate deficiencies in the quoted work. In each case, Berlinski selects quotations which deal with the issues of what are systems, what is GST, and why should GST be studied as a separate discipline. In most of Berlinski's quotations, it is clear the writers are trying to generalize from physical systems to biological, social and political systems. Consciously or unconsciously, they draw analogies with physical systems and hope to emulate the success Systems Analysis has had in dealing with such systems.

Berlinski first takes on von Bertalanffy. He presents several quotations from von Bertalanffy's writings to demonstrate that von Bertalanffy's objective was to derive in a formal way laws true of all systems, whether these systems be physical, biological or social. Berlinski's attack first notes that only the laws of logic are true in all systems, and these laws are all form and no content. If any other laws are to be true, the set of systems to which they apply must be restricted to something less than the set of all systems. That is, if a law is to have any substance at all, it can be true only for some systems, and there will be some systems for which it is not true.

Just in passing, Berlinski quotes von Bertalanffy that "(a certain equation illustrates) a point of interest for the present consideration, namely the fact that certain laws of nature can be arrived at not only on the basis of experience, but also in a purely formal way." Berlinski first demonstrates that the purported law of nature is not true of all systems; that in order to derive the equation von Bertalanffy made certain assumptions which restricted the range of systems for which the law is valid. Then Berlinski closes in for the kill, using the viewpoint of a philosopher. Of the sentence quoted above, Berlinski states that it "suggests somehow that being based in experience and being formally derivable are alternative but symmetrical procedures whereby a sentence may be counted as a law of nature. This collapses the distinction between inductive and deductive experience." (Emphasis in original.) Had Berlinski been a historian of science rather than a philosopher, he might also have noted that major progress in science came only after "natural philosophers" gave up the idea that pure reason could deduce laws which simply must be true in nature, and instead concentrated on experimental discovery.

* This review is reprinted from Technological Forecasting & Social Change, Vol. 14, No. 3, August, 1979, by permission of the editor of the journal.
Berlinski next turns to Ervin Laszlo. He cites a number of quotations dealing with the idea that systems are more than the sum of their parts, that systems somehow have the property that they remain more or less unchanged despite changes in their components or in their environment. Laszlo is quoted as describing these properties with such terms as adaptive self-stabilization, cybernetic stability, adaptive self-organization, and hierarchies. The specific quotations with Berlinski uses are shown to be vague and indefinite, and to have little if any meaningful content. Regarding a particular explanation by Laszlo of the meaning of hierarchy, Berlinski states, "This is not quite a definition of what it is to be a hierarchy, since the definiendum seems to figure prominently in the definitions. Still, one sees what Laszlo has in mind and only the purist, I suppose, will observe that the formalism introduced (in the quotation) is strictly speaking meaningless." Here again, Berlinski focusses on the purported goal of finding general laws true for all systems, and also attacks the use of mathematics for ornamental purposes.

Rapoport is Berlinski's next victim. In this case the attack dwells particularly on Rapoport's misuse of the concept of isomorphism, particularly the attempt to show that if two systems are described by equations of the same form, somehow the laws of the two systems must be analogous. Berlinski concludes that Rapoport's attempt to obtain general laws applicable to all systems is no more successful than those of von Bertalanffy and Laszlo.

Berlinski turns next not to a specific individual but to what he refers to as "affable disciplines." By this he means the collection of disciplines and pseudo-disciplines which General Systems Theorists claim either are related to GST or are part of GST. These affable disciplines include cybernetics (upon which Berlinski heaps a great deal of scorn), mathematical systems theory, graph theory, set theory, automata theory, recursive function theory, and information theory. His basic argument here is that those affable disciplines which, like information theory, have genuine content, are too narrow to support the broad uses to which GST would put them. On the other hand, those "disciplines" which have some breadth, like cybernetics, have little if any genuine content.

The final section of the first chapter presents Berlinski's attack on Urban Dynamics. He examines in detail the equations describing arrival of underemployed males in an urban area. Berlinski's attack can be reduced to two points. First, the form of the equation is purely arbitrary. It consists of the product of a set of variables which represent degrees of preference between the city and the external environment from which the underemployed come. However, no rationale is given as to why the product form is chosen. Other forms of the relationship might be equally plausible, and in any case the proper form is surely a choice to be made on empirical, not theoretical, grounds. Second, certain variables are included in the equation. However, there are other variables in Forrester's model which could, with equal plausibility, be included in the equation. No rationale is given as to why some are included and others excluded. Again, the choice of variables to be included is an empirical question. At no point does Forrester provide any empirical justification for either the form of the equation or the variables included in it. Moreover, in Berlinski's view, Urban Dynamics is dominated by "assumptions that belong quintessentially to General Systems Theory." That is, all the important properties of a city can be explained by describing the processes and structures within the city itself. The environment becomes solely a source of men, or a sink for men leaving the city. Berlinski is saying that there is more to urban dynamics than is captured by Urban Dynamics, and all of GST shares the same faults.

In Chapter 2, Berlinski returns to the attack on Forrester. The chapter opens with a brief digression on systems of differential equations. Berlinski reinforces a point made earlier about the importance of distinguishing between a model and the theory of that model. He quotes Andrei Monin to the effect that a physico-mathematical model of a system is composed not only of a set of dynamic equations describing the behavior of the system, but also the boundary conditions for the system, and the algorithm for numerical solution of the equations.

Berlinski next turns to an examination of Systems Dynamics, as presented in a number of works by Forrester, Meadows and others. While his attack is on System Dynamics generally, his emphasis is on the World III model and most of his examples are drawn from World Dynamics. Berlinski traces one particular set of relations in World Dynamics, those governing population. Population is of course a "level" variable in Forrester's terminology, which means it can not change value instantaneously, but can change value only through the accumulation of "rates" of growth or decline. "Rate" variables, in Forrester's terminology, may change value instantaneously. The form of a rate variable in the World III model is a normal rate multiplied by one or more "multipliers" (whose values may be greater or lesser than 1.0) which represent the effects of various factors on the rate of accumulation or decline of a level variable. This particular form for the rate equations is the same as that which Berlinski criticized in the preceding chapter as being arbitrary and untested empirically. Here he makes another point against this form for the equations. The form implies that the relationship between the variables is independent of time. In fact, Berlinski states, the multipliers are functions of time and should be treated as such. The Birth Rate from Material Multiplier and the Material Standard of Living, for instance, are fixed to have a particular relationship on the basis of the
way they are related in 1970. But, Berlinski points out, they might well have had a different relationship in 1870. To use the 1970 relationship as though it were true for all time is incorrect.

Berlinski next turns to the issue of non-linearity. No one disputes that the real world is nonlinear in its behavior. Mathematically, this means that the relationship between almost any pair of variables is dependent upon the values of other variables. As these other variables change, the relationship between any two given variables will change. But the very form of System Dynamics models, in which rates are affected by multipliers which are allowed to deviate from a “normal” value of unity, is not very nonlinear. Berlinski shows that the general form of the System Dynamics equations can be reduced to the form of differential equations with time-varying coefficients. Moreover, given the assumptions stated by Meadows as being behind the World Dynamics model, Berlinski next shows that it is possible to place bounds on the coefficients for the differential equations. Once these bounds have been imposed, the behavior of the equations is completely defined. The level variables, such as population, must behave in a “grow, overshoot, collapse and repeat” manner. Thus the detailed computer simulations are completely unnecessary. The system behavior can be determined by a qualitative analysis of the equations describing it. This leads him to observe that the distinction between the theory’s assumptions and its cardinal conclusions is very hard to grasp.

Berlinski next addresses the many problems which beset the passage from theory to model. A great deal of work has been done in the past two decades on the procedures which must be used to estimate the coefficients of equations from available data, and the precautions the model-builder must take. Much of this work is exemplified by the procedures of econometrics, although it appears also in other fields. Berlinski’s complaint is that System Dynamics seems totally innocent of this work, and completely ignores the hard-learned lessons about statistical treatment of data which are today put into practice by econometricians, biometricians, etc.

Berlinski’s next major point against World Dynamics, in particular, and one made frequently by other critics as well, is that it is a very highly aggregated model. The variables in World III are not only aggregated, they do not correspond to observable state variables in the real world (with the exception of population). None, not even population, act as do macroscopic variables such as pressure in physics. Interactions among the variables actually occur at the microscopic level of the entities which make up the aggregates. For instance, the effect of pollution on population actually occurs as effects of specific pollutants on specific individuals. Treating the aggregate variables may not be justified. Berlinski offers an example from economics. Individual firms in an industry act to maximize their individual profits. However, there is no justification for projecting this behavior to the industry as an aggregate, and assuming that the industry attempts to maximize industry profits. In fact it probably doesn’t. The actions of individual firms, in maximizing their individual profits, may even cause industry profits as a whole to suffer. There exist well known conditions for the validity of aggregation. World III shows no attempt to demonstrate they are met. Once more, Berlinski complains about the lack of necessary empirical justification for the model.

In Chapter 3, Berlinski addresses Mathematical System Theory (MST) (his term). In one sense, this chapter is Berlinski’s most general attack on Systems Analysis in general and GST in particular. Berlinski argues that MST grew out of roots in engineering and applied physics. He questions, then, the suitability of engineering models for social and biological sciences. Specifically, he argues that systems, broadly conceived, do not share any common properties, and engineering systems are not like social systems. There are two specific reasons for this difference. Most engineering systems are linear or only mildly nonlinear (because they are designed to be that way); political and social systems are highly nonlinear. Engineering systems in general are of low dimensionality, in that only a few variables are needed to describe them; social and political systems involve a high degree of dimensionality. Both these objections bear on the suitability of MST, which deals with Ordinary Differential Equations as classically applied to engineering systems, to the problems of political and social systems. In short, MST is simply not suited to handle highly nonlinear systems, nor is it suited to handle systems of many dimensions. It is therefore inappropriate to try to use MST for political and social systems. Berlinski’s strongest scorn, however, is reserved for those (e.g., Easton, Kaplan) who use the language of MST (e.g., input, output, feedback) in situations where these concepts are empty of content. In short, he objects to those who seriously attempt to apply MST to social and political systems, but he objects even more strongly to those who simply discuss the problems of political and social systems in jargon drawn from MST.

In addition to the dimensionality and linearity issues, Berlinski summarizes his objection that MST is not suitable as a model for political and social systems under the following aspects.

1. Feedback is a meaningful concept in control theory. It refers to measuring the difference between an actual and a desired state and using this “error signal” to move the system toward the desired state. Many systems theorists describe systems as having goal-seeking behavior. They then characterize goal-seeking in purely behavioristic terms (often in terms only of what an outside observer could see), then equate this behavioristic goal-seeking with feed-
back. This, according to Berlinski, is a completely inappropriate use of an engineering concept.

2. Much discussion of political and social systems involves use of the terms "input" and "output." These terms arose in engineering systems, and are meaningful concepts in such devices as filters, amplifiers or petroleum refineries. Their use in discussion of political, social or economic systems is strictly by analogy, and in many cases the analogy may not be valid. Moreover, even where the analogy is valid, use of such terms may not be the most effective way to analyze or describe a system. Inhomogenous differential equations not only exhibit the outputs of a system (the solutions of the equations), but the inputs to the system are contained in the inhomogenous portion of the equations. Thus Systems Analysts often discuss inputs and outputs in situations where discussion of inhomogenous differential equations (even if many-dimensioned and highly nonlinear) would be more appropriate.

3. Control theory is often used as an analogy by those trying to apply MST to political and social systems. But control theory arose in circumstances demanding regulation of one or more variables within a system. There is no proof offered by the Systems Analysts that social and political systems are at all similar to control systems.

To summarize the third chapter, Berlinski argues that MST, and its full mathematical elegance, applies to linear, time-independent systems. Engineering systems are of this type, which explains the power of MST in analyzing them. Social, economic and biological systems are not of this type, and MST shouldn't be used on them. This is not to say, he asserts, that appropriate mathematics cannot be developed for these kinds of systems. It is to say that the appropriate mathematics hasn't been developed, and that those political scientists, biologists, and economists who talk about their subjects in the jargon of MST, without developing the necessary mathematics, are simply using MST as window dressing.

Having said all this, in an attempt to present Berlinski's arguments in a manner which condenses them while not doing them an injustice, what is one then to think of the book? How can a practicing systems analyst respond to Berlinski's onslaught?

The book can be considered on two levels. The first level is as pure entertainment. Berlinski's wit is dazzling. With rapier-like thrusts he punctures the overinflated prose of the System Analysts. He carefully dissects the corpus of Systems Analysis and shows that its true mathematical content is near zero. A few examples will suffice to illustrate the point.

"...Laszlo argues only that the natural systems, as opposed to heaps, are 'nonsummative,' possessing characteristics 'not possessed by (their) parts singly.' But so do heaps since only the whole of a heap of systems theorists, for example, is the whole of that heap." (P. 13)

"Equation (1.4) does have modest usefulness in the description of uniform growth—a continuously copulating clutch of rabbits, for example." (P. 7)

"The world model itself is expressed as a numbered string of finite-difference equations, the notation largely in the cumbersome DYNAMO style, a Stalinist mass of tightly bunched capital letters."

Nevertheless, it must be recognized that this book is a one-sided debate. The Systems Analysts Berlinski attacks have no opportunity for rebuttal. For instance, the debate between Forrester and the economists is not as one-sided as Berlinski implies. Forrester has himself landed some telling blows on the economists. If the book is viewed simply as entertainment, one has to agree it is entertaining. However, one also has to recognize that if Berlinski's targets were present to defend themselves, the book would likely be even more entertaining.

It is necessary to go beyond looking at the book simply as entertainment. A basic issue is whether Berlinski's attack can be considered fair in the sense that he has attacked key points of the writers he has discussed, or that he has analyzed a representative sample of what they wrote. It is clear that Berlinski has overstated his case in at least a few points, possibly in overzealous pursuit of "uncompromising negativism." For instance, he objects to the behavior of the population variable in World III, arguing that the behavior is not "lawlike." One presumes that he would have been more satisfied had it shown behavior such as logistic curve. However, one has to ask, why should it be lawlike? It could behave in that fashion only if it is purely autonomous, or governed by variables endogenous to itself. If it is affected by other variables, one would expect it to reflect that dependence and deviate from a "lawlike" pattern. Berlinski also goes too far in objecting to the use of the concept of "feedback" in social systems. While he rightly condemns those who use the term in at most a figurative sense while pretending they are using it in a mathematical sense, he manages to throw out the baby with the bathwater. Analysis of economic phenomena, in which demand and supply interact to establish levels of production, can be analyzed very fruitfully in terms of feedback, with no compromise of mathematical rigor. Even Adam Smith, in the Wealth of Nations, likened the behavior of economic systems to a regulator on a steam engine. He didn't have the word "feedback" in his vocabulary, but at least he had the idea. Mathematical economists since then have made effective use of the concept.

Despite these errors of over-statement, however, Berlinski has been right on target with a great deal of his criticisms. Clearly, von Bertalanffy did take some ideas ranging from the trivial to the down-
right wrong and dress them up in a lot of unnecessary mathematics. Clearly, Laszlo's writings include some indigestible polysyllabic gibberish. Clearly, Rapoport has written some nonsense about the similarities among systems, however defined. Clearly, systems analysis, particularly those involved in General Systems Theory, have tried to make a great deal more out of a mish-mash of cybernetics, information theory, etc., than these disciplines justify. Clearly, it is not obvious that System Dynamics is suited to describing the kinds of systems present in the social, political and economic world, and its suitability is a matter of empirical verification, not theoretical argument. But is it also clear that none of these things have any merit or content? Ever granting that Berlinski has pointed out serious and possibly important errors, is it the case that he has demolished Systems Analysis completely? This, I believe, is the really crucial issue which must be addressed in examining this book.

One thing is certain. This crucial issue cannot be addressed satisfactorily in even a book the size of Berlinski's, let alone in this review. Nevertheless, I will offer some thoughts on the matter. First, it must be said that it will not be sufficient for systems analysts to reject any criticism which doesn't offer alternatives. One needn't be a cook to tell that the soup is spoiled. One needn't be a tailor to observe that the emperor has no clothes. Thus pointing out errors is important even when one has no better alternatives to offer. In doing this, Berlinski has certainly performed a signal service, for which systems analysts should be grateful, even though the medicine may be hard to swallow.

Secondly, although Berlinski has had a great deal of fun pointing out the absurdities and the dense fog present in many attempts to define the concept "system," there is some meaning in the term. We do experience things which seem to remain themselves despite changes in their surrounding or even changes in their constituents. This reality of systems in the world can't be made to go away simply by pointing out the pompous nonsense in the writings of systems theorists. Thus there is a need to be able to address systematic and system-like behavior, including both analysis and design. Even if there are no general laws of systems, that doesn't mean there can be no formal means of dealing with systems.

Third, we have to face up to the level of presumption, bordering on arrogance, which permeated and still permeates much of Systems Analysis. The early writers on General Systems Theory, for instance, produced some trivial and even incorrect work regarding the similarities among systems considered as systems, and dressed this work up in the inappropriate mathematics with which Berlinski had so much fun. They made references to information theory, to cybernetics and feedback, to automata theory, as though these proved something about the capability of their theories to produce important and practical results. It must be said, however, that they did this in all sincerity. They believed that the body of mathematics contained in such fields as information theory, control theory, etc., would soon prove capable of handling the problems they proposed to deal with, if it were not already capable. They believed they were on the verge of having mathematics adequate to the task of describing political and social systems. If looked at objectively in retrospect, their writings seem filled with hubris. In comparison, one thinks of Newton, who had the presumption to think he could describe the motions of the entire solar system. He believed that the calculus was adequate to the task, and moreover he believed it sufficient to approximate the entire solar system as a collection of point masses in an inverse square force field. The degree of presumption involved in such heroic assumptions borders on the rash. And yet, Newton got away with it. Changes in the description of the solar system, following Newton, are but minor refinements. Even the changes introduced by the Theory of Relativity represent only higher-order corrections. Newton's presumption turned out to be justified. In the twenty-five years since I first studied control theory and read Cybernetics (with enthusiasm and awe), it has become clear that the systems analysts are not going to share Newton's luck. The mathematics available to them was not adequate to the task of describing social and political systems. It still is not adequate, and shows no signs of being adequate in the foreseeable future. However, they are still doggedly pursuing the same course, heading down the same deadend trail, refusing to go back to the beginning and start over in a new direction.

Fourth, someone has remarked that the greatest obstacle to solving a problem is the illusion that you already have a solution. Recognition of the true state of Systems Analysis is the necessary first step toward removing the illusion that we have a body of tools capable of analyzing and describing political and social systems. Removal of that illusion will then allow us to start searching for the solutions that, twenty-five years ago, appeared to be already in hand. Since no one within the Systems Analysis field has taken on the task of dispelling the illusion that we had the solutions we needed, we may be thankful to Berlinski for undertaking the task.

Fifth, there are problems abroad in the land. These problems aren't going to wait until we develop the full array of tools we need to handle them. To pick just one example, the problem of simultaneous inflation and unemployment isn't going to go away or let us alone until (at some date in the far future) we manage to develop an understanding of a complex economy, and develop the mathematics necessary to express our understanding. In the meantime, it is necessary to search for palliatives and partial solutions. One thing which Systems Ana-
lysts can contribute to that search is the notion that “everything is connected to everything else,” and that therefore “you can’t do only one thing.” With some humility about how much we really know about the interconnectedness of the world, and some more humility about our ability to state laws governing the behavior of specific systems in the world, we may then begin to give tentative but useful advice regarding actions to be taken in the short term, not to optimize things, but to keep them from getting any worse. And with a view to the long term, we can begin developing mathematics that is suited to the kinds of systems we find in the political, social and economic world.

In summary, then, I believe that Berlinski’s attack, although overstated in places, has by and large hit the mark. Taking it seriously may be unpleasant, but doing so will prove to be of significant benefit to Systems Analysis and Systems Analysis.
Dear Sir,

I am honored that you asked me to respond to criticisms of my writings on systems philosophy contained in Berlinski’s On Systems Analysis and Lilienfeld’s The Rise of Systems Theory. However, for reasons I shall detail below, I shall not provide a systematic response to these authors, but will offer a few thoughts of my own, which you may share with your readers if you wish.

First, the above named books do not as yet merit a systematic response from the viewpoint of my work in systems philosophy; they operate out of an entirely different intellectual tradition and their conceptual spectacles distort the very nature and intent of systems philosophy beyond the point where reasoned argument could be of help. Berlinski—like Ida Hoos who seems to have started this particular tack of systems-criticism—conceives of systems philosophy as some kind of systems analysis and makes his critique on the basis of the subtitle of his book “... Concerning the Limitations of Some Mathematical Methods in the Social, Political, and Biological Sciences.” But systems philosophy is not a method, mathematical or otherwise, nor is it a science. As its name indicates, it is a philosophy, although one that uses the concepts and theories of some new trends in contemporary sciences to answer perennial philosophical questions.

Lilienfeld’s book is much more aptly conceived. As his main title notes, systems theory is indeed on the rise. It also has aspects, or at least implications, for a socially and politically active worldview, and may in consequence be analyzed, in part, as an ideology. The attempt in itself merits attention and would merit a systematic response as well. Unfortunately, he too, operates out of an entirely different paradigm, which makes dialogue next to impossible and probably fruitless (I use ‘paradigm’ here in the sense of Kuhn, and of Hansen in Patterns of Scientific Discovery, although Hansen did not use that term himself). While giving fine overviews of the ideas of systems thinkers, as soon as he voices his own views he appears to apply some entirely external conceptual scheme which prevents him from seeing even the most fundamental tenets of systems thinking (when he says, for example, that “the basic forms of systems theory remain classical positivism and behaviorism” and asserts that “the system, if it is to be a system, must not only have boundaries; it must also be a closed system...”),

It may—and probably will—be argued that critiques of one conceptual paradigm can and should be made from the standpoint of another. This disregards the fact that any paradigm, if it claims to refer to events ‘out there’ in reality, represents some mode of organization of ideas, sensations, and probably intuitive and emotional elements as well which cannot be tested by reference to a simple inspection of the reality it refers to—since we only ‘see’ such reality through one or another of our conceptual constructions. There is no ‘final’ test for the verity of any basic conceptual mode. Relative tests do apply, however. These operate on the meta-paradigmatic level, where the basic conceptual modes (or paradigms) are compared for such things as power of explanation, practical guidance-value, elegance and simplicity, testability, internal consistency, etc. This is an interesting and valuable exercise, but it is not the one that Lilienfeld performs, although at times he appears to be on the verge of it. For in such exercise the investigator must remain on the metaparadigmatic plane and use conceptual tools which are as neutral as possible. He must not, as Lilienfeld, take the concepts and theories of one paradigm to ‘disprove’ those of the other. That makes the attempt biased and reintroduces all the problems of a cross-paradigm dialogue.

My second reason for not offering a systematic response to these books is a simpler one. It is that they, and others of their ilk, perform a most useful function—which is not diminished by the fact that their authors have never intended to perform it. It is an educational function different from the one they envisage: they ‘teach’ the theories they criticize even if they intend to ‘bury’ them. The beneficiaries are those readers who have not been thoroughly enculturated into either the ‘home’ or the ‘target’ paradigms of the writers (there are some such people, though they seldom occur beyond the undergraduate level).
Young and as yet relatively virginal intellects are suddenly given a choice: you may think this way or that. This is particularly significant when the new way is really new for the reader, and when he or she has been accustomed, as most high school students and college undergraduates, to hear one kind of theory or mode of thinking presented as the gospel truth.

The educational function is brilliantly performed by Lilienfeld, who gives detailed overviews of the systems theories before he gets down to criticizing them. The same function is of keener caliber in Berlinski (as also in Hoos) who keep hammering at their targets even if they are largely made of straw. Such practices seldom, if ever, truly convince anyone of the falseness or insignificance of a theory who is not already so convinced: the history of philosophy, and even that of science, testifies to that fact. But those who are not already convinced of the truth of their pet theory but have a still open mind and some measure of intellectual curiosity, will note with interest that there exists a kind of theory, or mode of thinking, which—though practically without merit in the eyes of its critics—seems nevertheless to deserve writing entire books about. Few people with more than a modicum of intelligence would fail to note that there is more to such exercise than target practice. Just what there is to them they will see very clearly from books such as Berlinski's (and Hoos') but will see rather well from writings such as Lilienfeld's.

On reading Lilienfeld the reader gets a fair overview of the systems field, and such overview, unless one is already a thoroughly indoctrinated positivist, behaviorist, Weberian, existentialist, Marxist, or whatever, cannot help but be impressive. To this writer at least, the overviews are of an entirely different quality than the evaluations. The former show imagination, breadth, penetration, some brilliant insights and offer much food for thought, while the latter often appear petty and small-minded, saying in effect that 'if anyone thinks anything differently from me he is either talking nonsense or just saying what I am saying but through a lot of useless verbiage.'

Publications such as those discussed here may represent points along a curve that marks the trajectory of a theory innovation. When a new theory or conceptual mode of thought appears, it is almost totally ignored by the adherents of the established paradigm (or paradigms). As it wins adherents here and there, more than eyebrows are raised in the circles of the establishment: some definitions or findings attributed to the challenger become known (falsely, as often as not). There are occasional references to it in books and journals. In time, it becomes acceptable to include a criticism of it in one's writings, and may even become fashionable to do so. The day is not far away when it becomes good academic politics to show one's sophistication by producing a full-fledged critique of the challenger.

At first, these are somewhat condescending—after all, one does not want to commit the mistake of taking the new theory too seriously, lest one be misunderstood by one's colleagues. The purpose is to expose the sins and crudities of the challenger, making fun of it in an erudite manner. Whether the critic has truly understood and properly represented the criticized theory is of small moment, since most of his colleagues are certain neither to know nor to care.

If the challenger continues to gain ground in the intellectual community (or in society as a whole), the critics tend to become more expert. Now they read up a little more on the challenger and try to understand it before shooting at it. The shots, however, still come (for a time) from the home base of another mode of thought which, for the critic, grasps the real truth and uses the proper logic.

This is how far we appear to have come today, with Berlinski-Hoos representing the penultimate, and Lilienfeld the (so far) ultimate phase. That we did come this far is a remarkable achievement of systems thinking. It has become an innovation that is legitimate to criticize, and indeed good business to do so. Books on it sell, and are used even by one's establishment colleagues.

There could—and in this case I believe will—be more advanced points along the trajectory aptly described as the 'rise of systems theory.' A logical next stage indicating this rise would be the appearance of books and studies which undertake a consistent meta-paradigmatic exploration of the merits and faults of the new theory vis-à-vis the older schools, without using some of the latter as an axiomatic basis for criticizing it. Subsequent to this we shall witness the publication of an increasing number of critical essays which already move within the conceptual universe of the new field. These constitute internal critiques, exploring inconsistencies, correcting biases, and suggesting further applications and developments. For systems theory this stage is yet to come, but its coming is prepared by the previous stages, including those that have just been attained.

It would be fruitless, and indeed counterproductive from the viewpoint of the new theory, to engage in polemics with writers who trace out the above suggested curve in their books. Rather, one should welcome them for their educational function, and as signposts that the field continues its rise.

In conclusion I shall add two further thoughts. Consider first the nature of systems theory. It is not a specialty falling within the confines of an academic discipline, but is a multitude of loosely connected concepts, theories and assumptions which inform a whole array of fields from the natural sciences to theology. What they articulate is not a simple and immediately testable connection between a few isolable 'facts' or data, but an as yet hazy insight into the nature of growth and evolution in all spheres of experience. It is easy but vacuous to
criticize such a mind-boggling paradigm for being fuzzy or inconsistent. Think only what it would be were it not fuzzy and inconsistent! Systems theory would eclipse Einstein's wildest dreams for a unified field theory, for while a unified field theory would explain the phenomena of the physical universe in reference to a single basic field or depth-structure, systems theory would explain the emergence and the behavior of physical, chemical, biological, social as well as cultural entities and their interaction within a shared time and space matrix. No wonder we are not there yet. But great wonder, indeed, that we could already get going in this direction.

Lastly, let me merely register a polite protest to repeated claims that "systems theories are of no use." Lilienfeld asks, "What substantive theoretical or even "applied" sociological, political, economic, problems have been resolved by the elaboration of systems theory," and adds, "We await the answer." (In another place he repeats: "To what new insights and substantive results does [systems theory] lead? So far there appears to be none.") If Lilienfeld is still awaiting the answer, could it be that he is awaiting it in the wrong place? Could he perhaps wait for it in a bookstore or a library? Or in laboratories, think-tanks, in corporate planning offices, in governmental policy organs or international organizations? He would not have to wait any longer. Take merely my own experience as example. After finishing Introduction to Systems Philosophy (1972, revised edition 1973) and its popularizing overview The Systems View of the World (1972)—books that Lilienfeld has read (at least in part, as he says)—I went on to write A Strategy for the Future (1974), a book that is subtitled, "The systems approach to world order." This, however, Lilienfeld has not read or even heard of, it seems. Yet it is an application of my systems philosophy to problems of world order, coming out of a series of seminars at Princeton's Center of International Affairs. After this came a Report to the Club of Rome, called Goals for Mankind (1977), a short summary of the main findings in The Inner Limits of Mankind (1978), and several dozen studies and articles in a wide variety of journals and newspapers. Almost without exception, they apply theories and concepts which are elaborated on in my work on systems philosophy to international and world affairs. They are not unread (except by the critics of systems theory): Goals for Mankind, for example, has been published in New York (by Dutton and later by the New American Library), London, Tokyo, Milano, Amsterdam, Mexico, and Helsinki, with condensed versions in Moscow, Warsaw and Budapest. My current work, as Special Fel-

low and Project Director at the United Nations Institute for Training and Research, is resulting in a series of 17 volumes in collaboration with some 98 research institutes and teams in all parts of the world.(4) In creating, organizing and bringing to fruition this international research effort, I have made constant recourse to systems thinking. Indeed, it is not something one can merely leave behind, like a worn vest. It is a mode of thinking, a way of looking at the world, and even a set of values and priorities which become deeply embedded in one's mind and personality.

The critics of systems theory accuse it of being "nothing but" a fashionable jargon. In reality, systems terminology has the same function as any other intellectual or scientific language: to state ideas, concepts and relationships as concretely, precisely and economically as possible. The ideas, concepts and relationships are "systemic" in character even in the absence of the systems terminology. The reader will find hardly any such terminology in my writings after 1977, when I became more concerned with communicating the results of my research than fighting for systems theory in the spirit of paradigm chauvinism. Yet, as systems theory becomes more known and accepted, use of its terminology will not place excessive burden on the communication of its message. I plan to begin using the more concise and precise systems terms in the coming years, expecting that more people will understand it, and fewer will need convincing that it is nothing but an empty shell. ln the meanwhile, I, and my fellow systems thinkers, could ask for few things better than to be read at length—and to be criticized extensively, even by scholars coming from and subscribing to different traditions.

REFERENCES

2. Lilienfeld, pp. 249 and 248.
3. Lilienfeld, pp. 172 and 264.

Cybernetics is a curious discipline. For those who practice it, it is in danger of dying out. For at least some people outside the field, it threatens to take over the world. If nothing else, the recent books by David Berlinski and Robert Lilienfeld prove that someone out there is following our work. But if these books are an accurate reflection, we are not making ourselves very clear, and indeed we may have something to learn. Berlinski's book has received more attention, but Lilienfeld's is the more useful. Both books fall short of the ideal—a readable, carefully analytic, constructive contribution to the field. On Systems Analysis is notable for sarcasm more strident than I have ever seen before in an academic treatise. The Rise of Systems Theory uses so many long quotations set in type so similar to the main text that one spends a fair amount of time figuring out whether the ideas being expressed are Lilienfeld's or someone else's.

Shortcomings: Under the Aspect of Berlinski

I do agree with some of Berlinski's criticisms. There are fundamental difficulties with Von Bertalanffy's explanation of his empirical approach. Berlinski notes that if one looks at a variety of systems to discover their common properties, what is common to all systems are the logical truths. Is systems theory then a branch of mathematics? I do not believe that this question can be resolved, as the Binghamton group is inclined to do, by saying that general systems theory lies between philosophy and mathematics on the one hand and political science, sociology, and engineering on the other hand. Such a formulation is not an explanation but a compromise. It may calm a dispute but has little persuasive power.

Cybernetics has dealt with this issue by focusing on cognition. Since all systems are described by observers, an understanding of the observer will tell us something about all systems we shall encounter. For theory development empirical research is therefore directed at the nervous system. Of course models can be constructed of any reference system. But in cybernetics this work is usually considered to lie in the area of application rather than theory development.

Anatol Rapoport's emphasis on searching for mathematical isomorphisms guided the articles included in the General Systems Yearbook for twenty years. Although these articles contain many thought-provoking analogies, I know of no widely referred to scientific success that has resulted from this strategy. Indeed the strategy has led to one important misunderstanding—the confusion over the relationship between information and entropy and the notion that as thermodynamic entropy increases, information or organization must necessarily increase as well. Numerous people, including Laszlo, have been misled by this notion. However, the relationship does not hold. If organization on Earth increases as the sun expends itself, then why do species become extinct and civilizations rise and fall? Industrialized countries consume more energy per capita than developing countries, but some nations at the same level of development are more wasteful than others. A corporation cannot guarantee high profits by consuming more energy. Information and thermodynamic entropy are different concepts. Only the equations are similar.

Berlinski's statements that general systems theory is lacking in content deserve some reply. The view that a general theory of systems would be trivial or would lack a foundation has been expressed more than once. Several replies have been made to this criticism. Bateson has noted that cybernetics is a science of form and pattern rather than a science of substance. Ashby has dealt with the issue in the first chapter of An Introduction to Cybernetics.

Cybernetics treats, not things but ways of behaving. It does not ask, "what is this thing?" but "what does it do?" Thus it is very interested in such a statement as "this variable is undergoing a simple harmonic oscillation," and is much less concerned with whether the variable is the position of a point on a wheel or a potential in an electric circuit. It is thus essentially functional and behavioural.
Cybernetics started by being closely associated in many ways with physics, but it depends in no essential way on the laws of physics or on the properties of matter. Cybernetics deals with all forms of behaviour in so far as they are regular, or determinate, or reproducible. The materiality is irrelevant. . . . The truths of cybernetics are not conditional on their being derived from some other branch of science. Cybernetics has its own foundations.(4)

Berlinski apparently found Ashby's arguments unpersuasive. It would be useful to know why. Any other way of dealing with the criticism that systems theory is too general is to go back to first principles. It is frequently said that scientific concepts are neither true nor false, only more or less useful. Theories are judged by their usefulness in explaining and predicting phenomena. Furthermore, an objective of science is to explain the largest number of phenomena with the smallest number of propositions. From this line of argument we can conclude that if a proposition is useful, it cannot be too general. If it explains a larger number of phenomena, it is more useful even though it is also more general. Consider the concept of mass. An insect has mass, and an elephant has mass. An atom has mass, and a star has mass. Mass is therefore a very general concept, encompassing both living and nonliving things of vastly different size. Does this great generality render the concept meaningless? Quite the contrary. And likewise the concepts of variety and the tendency to equilibrium which lie at the heart of systems theory are useful even though they are very general.

When dealing with mathematical models, Berlinski does not ask why the model was constructed in the first place, or what model was being used prior to the mathematical model. Nor does he suggest how a better model might be constructed. Consequently, although the reader can agree with many of Berlinski's technical points, the book provides little that is helpful.

Berlinski's criticisms of mathematical models reflect a lack of understanding of several ideas basic to cybernetics. He notes that social systems are many dimensional and implies that models are not useful if they include only a few variables.(5) Berlinski does not seem to understand that any act of regulation requires selecting a set of variables to pay attention to.(6) Even when dealing with inanimate objects, the number of variables one could look at is virtually unlimited. The trick lies in deciding which are the important variables to take note of. As Ashby put it, "Man adapts by conquering the reducible, the irreducible is impregnable."(7) In any particular situation a useful criticism would be to suggest a different set of variables and argue why they are more important than the set that was chosen. Or one could argue that a few additional variables should be included. But merely calling attention to dimensional complexity will not help someone who must make a decision.

Furthermore, Conant and Ashby have constructed a proof that every regulator of a system must contain a model of that system.(8) Before people tried to construct mathematical models of systems, they were using mental models. For example the domino theory was a conception used to justify United States intervention in Viet Nam. Few people worried about whether the relationships in the domino theory were "linear" or not. The theory was accepted or rejected on other grounds. Consider a second example. The Club of Rome models used as key variables population, natural resources, food, and pollution. The principal mental model used prior to that time dealt with nations, alliances, and the military balance of power. Berlinski does not examine the "mathematical properties" of the earlier model, nor does he argue that the natural resources model is less appropriate than the nation-state model. Indeed he seems to be unaware that two models are competing for public acceptance. Berlinski brings to mind a ball player who has mastered the locker room repartee but has forgotten, if he ever knew, what happens out on the playing field.

Ideology in the Eyes of Lilienfeld

Whereas Berlinski's arguments are purely academic, Lilienfeld is principally concerned with the social consequences of cybernetics. Lilienfeld views systems theory as the ideology of the welfare state. By "welfare state" he means a society run by a centralized bureaucracy, a society in which the key issues are decided by a technical elite rather than through a democratic process. He maintains that the models constructed by systems theorists assume and help to create such a decision-making apparatus.

One can make many small criticisms of Lilienfeld's case. The summaries of the theories of Von Bertalanffy, Ashby, Wiener, Shannon, Turing and others shed new light on their work. Frequently he selects quotations which help to make his case regardless of whether they are a good reflection of a person's overall point of view. This is standard debating practice, but I am disturbed by his tendency to attribute anti-democratic motives to people whose goals were quite the reverse. Cybernetics may be naive about social systems, and some are ambitious. But I know of none who would advocate "the welfare state" that Lilienfeld describes. However, these are merely qualifying remarks. There is something important in what Lilienfeld is saying, and I hope that we shall not be so defensive or proud that we miss it.

I would make the case as follows. In a social system there are basically two ways to make a deci-
cision. One can examine the system of interest, study its past, construct a model, project the future, decide what variable or variables one wants to try to maximize, and then select the best alternative. Or one can consult the people concerned, let everyone have their say, and then make the most political decision—the one that pleases the most people or the most powerful people. In a democratic society the second alternative has an important advantage—people learn best by making their own mistakes. Societies learn in much the same way that individuals learn. If an elite makes decisions that the general population does not understand, political and ethical learning will not take place.

Cybernetics has tended to emphasize the first decision-making method. This may have been a necessary conceptual stage for the field to go through. Most of the theoretical work in the first few decades dealt with cognition in a single individual. The widespread assumption was that the same principles would hold at the societal level. Some people, such as Russell Ackoff, have argued that there is an important difference between a brain and a society. Individuals have their own goals but neurons do not. Others, such as Stafford Beer, acknowledge the difference but claim that it is not theoretically significant. Littenfeld’s book has helped me to arrive at the conclusion that the difference is quite important, not only theoretically but even more in how we act. The emphasis in cybernetics on the first style of decision-making—building a model and then using it to select an alternative—is at the root of the feeling that cybernetics is at least potentially anti-democratic.

A science of the second decision-making method would be a science of how groups make decisions—not an individual acting for a group but a group of autonomous, opinionated people. Such a science would have to deal with how a consensus is produced, how motivation is increased or maintained, and why people work together despite disagreements. As cyberneticians move into this new domain of inquiry, there is much that we can learn from social scientists. Since the second method of decision-making has advantages in certain circumstances, can an expanded cybernetics tell us when the second method is preferable or what combination of the two methods to use in a particular situation? Or is this not a theoretical question but rather a decision for the group?

Second order cybernetics, as it was developed by people associated with the Biological Computer Laboratory (BCL) during the 1970’s, has laid a firm foundation for this new arena of cybernetics inquiry. By investigating the nature of the observer, the BCL group developed a theory of autonomous systems. A next step will be a theory of how groups of autonomous systems make joint decisions and act together.

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Cybernetics:
Search For A Paradigm

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"The concern for man and his destiny must always be the chief interest of all technical effort. Never forget it among your diagrams and equations."—Albert Einstein

Is there a crisis in cybernetics? If so, I am surprised to learn it, for it has seemed to me that the ideas and techniques of this field have had as powerful and pervasive an influence as any development in recent scientific history. Perhaps the crisis has emerged because cybernetics has succeeded too well!

Looking back at Wiener's book (1948), one finds three main themes depicted there as central to his definition of cybernetics as the science of "communication and control in the animal and in the machine." These are: (1) control systems; (2) information theory; and (3) the brain-computer analogy. Each of these has had a profound influence on the fields affected; each has encountered difficulties; each has aroused controversy. Let us briefly survey the advances and problems each of these has had.

Prior to cybernetics, there was virtually no theory to account for the behavior of biological control systems. True, the concept of homeostasis had been advanced to describe the relative constancy of many physiological quantities (like body temperature and the pH of body fluids); but this was little more than an empirical generalization. Cybernetic control theory provided a powerful and rapidly evolving source of ideas, techniques of analysis, and mathematical models which soon produced a flourishing and still growing literature. There is no reason to doubt that research in this area will continue to be fruitful.

The concept of the servomechanism also effectively resolved a problem in biology that had previously been difficult to explain: the apparent purposiveness of many physiological phenomena. Biologists generally rejected the Aristotelian concept of a final cause, since it was difficult to see how a future state could affect the present course of events. "Teleological thinking" was regarded as "unscientific." Yet many physiological processes seem to be goal-seeking in character; and purposes guide our conscious lives. A servo-mechanism is basically a teleological mechanism, whose input may be regarded as a goal signal representing a future state, and whose activity, guided by feedback, proceeds until the output matches the input—the goal is achieved. On this basis the apparent purposiveness of physiological phenomena can in principle be scientifically explained. Even the differences between biological and engineering servomechanisms are not beyond the cybernetic modes of thinking to explain; thus, the fact that biological systems have the capability to generate their own goals requires only a relatively straightforward generalization of cybernetic control theory (Coulter, 1976).

The chief problems cybernetic control theory has encountered arise from the complexity of physiological processes and the difficulty of isolating physiological control systems from the matrix of interactions in which they are embedded. This problem is formidable but it is not insurmountable. It is a challenge for future research.

The application of information theory has been less successful. True, it has been a source of some useful concepts in neurophysiology and genetics; but efforts to apply the theory to biological phenomena have not been very fruitful to date. Leibovic (1969) points out that the statistical concept of information is "too restrictive" from a biological standpoint. When information is defined as "negative entropy," we are given a precise mathematical expression of limited biological utility. As a result, the general practice has been to regard information as synonymous with "pattern," and in this sense it has been useful, especially in focussing attention on an aspect of neurophysiological phenomena that is sometimes disregarded. Information, in this sense, is as real as mass and energy, and as important in understanding biological phenomena. The song of a radio singer exists as a pattern imposed on sound waves, then as a pattern of electric voltages in the transmitter, then as a modulation of an electromagnetic carrier, etc. The pattern is invariant under transformation from carrier to carrier; and it is related, by some as yet not understood coding process, to nerve impulse patterns in the brains of the singer and, ultimately, the people who hear the song. Physical laws govern the transformations of...
matter and energy involved in all these processes; but they do not explain the song. Yet the song can be described mathematically and there is no need to evoke some unknowable vital force to account for the coding and decoding processes of brains.

The brain-computer analogy has probably been the most controversial of the three main cybernetic themes. Wiener considered the computer to be "almost an ideal model of the problems arising in the nervous system. The all-or-none character of the discharge of the neurons is precisely analogous to the single choice made in determining a digit on the binary scale . . . The synapse is nothing but a mechanism for determining whether a certain combination of output from other selected elements will or will not act as an adequate stimulus for the discharge of the next element and must have its precise analogue in the computing machine." This picture of the brain and its working is highly oversimplified and, to neurophysiologists, naive. Nevertheless, the analogy is highly suggestive, and, to many, far better than earlier analogies (like telephone switchboard, energy transformer, the "hydraulic" model of Freud, etc.). And it has led to the emergence of an exciting, though itself controversial (Dreyfuss, 1972, Weisenbaum, 1976), area of computer science—the field of artificial intelligence (Boden, 1977, gives an excellent overview of this field.)

These basis ideas, and others, of cybernetics have thus had a pervasive influence; so the "crisis" evidently has other sources than the scientific content involved. What are these sources, and what, if anything, should be done to resolve the crisis?

The General Problem of Interdisciplinary Programs

Cybernetics (and General Systems Theory as well) originated basically as an interdisciplinary endeavor. Wiener was convinced that "the most fruitful areas for the growth of the sciences were those which had been neglected as a no-man's land between the various established fields." He was concerned by the fact that "science has been increasingly the task of specialists, in fields which show a tendency to grow progressively narrower . . . These specialized fields are continually growing and invading new territory. The result is . . . an inextricable tangle of exploration, nomenclature, and laws. There are fields of scientific work . . . which have been explored from the different sides of pure mathematics, statistics, electrical engineering, and neurophysiology; in which every single notion receives a separate name from each group, and in which important work has been triplicated and quadruplicated; while still other important work is delayed by the unavailability in one field of results that may have already become classical in the next field."

Specialization has been and continues to be a vital necessity in the advance of science. But it has the inevitable side-effect of creating more and more special fields, each separated from its neighbors by walls of ignorance that constitute barriers to understanding—the modern equivalent of the biblical Tower of Babel. This problem has long been recognized but a fully effective solution has yet to be found.

One approach is to foster the development of interdisciplinary educational programs and research institutes which focus on the interfaces between related or relatable special fields. Such programs can and do help. But they themselves encounter problems, of which two in particular seem important.

The first is that it is difficult enough to master one special field, let alone two (or more). One runs the risk of "falling between two stools." This can result in mediocre work. It must, I think, be acknowledged that this has happened sometimes in cybernetics.

But it must also be noted that specialists in a given field tend to judge work related to that field in terms of their own backgrounds, knowledge, and interests. Thus, a physiologist may judge a cybernetic paper purely from a physiological perspective, ignoring or belittling its non-physiological content. On the other hand, a theoretical physicist may ignore or belittle its physiological relevance, and find little new or interesting from the standpoint of theoretical physics. As Small (1960) points out, "It is a sad reality, but a reality all the same, that whenever one does something new and different, one has to expect to be shot down." It must be acknowledged that this, too, has happened.

A partial solution to this problem is to design special new courses and curricula which are selective in content and methodology. There are many details of biological science which are unnecessary for a cybernetician to know. Similarly, many aspects of engineering control theory have little biological relevance, and examples chosen from industrial and military applications do not illustrate how these methods can be used to elucidate biological phenomena. Meanwhile, as the cybernetic literature grows, such courses and curricula may progressively be weakened from dependence on their parent sources. Such a selective approach, reinforced by a critical emphasis on quality, can filter out mediocrity.

The second problem is socioeconomic in nature. Scientific research requires both institutional homes for scientists and financial support for their work. The established departments of academia tend sometimes to be jealous of their territory and jealous in defense of what they perceive as their "intellectual property rights." Where is a cybernetician to find an academic home, with reasonable freedom to conduct research in cybernetics? How many cyberneticians obtain funding without prostituting themselves to military and commercial interests—especi-
ally when more appropriate granting agencies are dominated by the established disciplines? The progress of science in general, and in particular areas, is not determined by scientists alone; it depends on the socioeconomic environment as well.

Unlike the first problem, this second problem does not have a clearly definable solution, or one that can be resolved primarily by cyberneticists. It depends on the politics of academia and the fickle winds of scientific funding. This does not mean, however, that nothing can be done. There are several lines of action that can be and are being pursued.

The first is the establishment and development of scientific organizations dedicated to cybernetics, and concomitantly the publication of scientific journals in this field. Although long delayed, this fortunately is now being done; and although the effects are not spectacular, they are bound to secure the survival and flourishing of cybernetics in the long run.

A second line of action, suggested by our president, is to “provide leadership . . . for the humanization of systems.” This involves investing a major effort in addressing the problems of society that are the source of so much human misery, and whose complexity has thus far defied the best efforts of the established sciences traditionally concerned with their elucidation. One of these, for example, is the problem of war, and the steadily increasing threat of nuclear holocaust. It does no good to rail at the politicians; they are prisoners of their roles, and all too often simply the end-products of systems which reward those motivated by the lust for power. What needs to be done is to analyze these complex systems in cybernetic terms, and to find ways to catalyze and accelerate a cultural evolution which will ultimately make war as unthinkable as cannibalism, human sacrifice and slavery have become, almost everywhere. Effective measures to achieve this are unlikely to be found by directly addressing the issues and crises that recurrently seize public attention. These are symptoms of cybernetic disorders, unstable states of extremely complex systems whose parameters have shifted in subtle and unknown ways. Rather, it is possible that cybernetic analyses can reveal the hidden relationships among these parameters that govern the global behavior of these systems, and suggest changes we and others can implement which will ameliorate that behavior. There is no assurance that cyberneticists can do this, and we should make no promises. But we can at least try.

If we are able to make effective contributions to the resolution of these problems of society, then a favorable socioeconomic climate for cybernetics will naturally emerge. Thus, efforts to facilitate the humanization of social systems are clearly in our own self-interest. They are also very much in tune with the social outlook of Norbert Wiener, whose advocacy of organizational changes promoting “the human use of human beings” is well known.

A third line of action is to clarify and develop a cybernetic paradigm. Kuhn (1970) characterizes scientific paradigms as achievements that “serve for a time implicitly to define the legitimate problems and methods for succeeding generations of practitioners. They were able to do so because they shared two essential characteristics. Their achievement was sufficiently unprecedented to attract an enduring group of adherents away from competing modes of scientific activity. Simultaneously, it was sufficiently open-ended to leave all sorts of problems for the redefined group of practitioners to resolve.” He discusses various examples, such as Copernican astronomy, Newtonian physics, Lavoisier’s Chemistry, relativity, and quantum mechanics.

Does cybernetics have a paradigm? I believe that it does but that it is needs clarification and development. Wiener’s book was the catalyst, but it is far too sketchy to provide a paradigm that is sufficiently powerful to solve the problems it addresses or to fulfill the expectations it arouses. But it does define a field of research—a general approach to communication, control, and “computation” whose elements are clearly related and whose range of application is quite broad. And there has been considerable progress since Wiener’s book was published.

If and when a cybernetic paradigm clearly emerges, it will inevitably attract an enduring group of adherents and provide an enduring basis for defining problems that challenge young minds to use the tools it provides to solve them. Unfortunately, a paradigm cannot be designed to order; it can only emerge as a result—indeed, as a byproduct—of an ongoing process of scientific endeavor.

Some Fundamental Problems

What, then, are the failures of substance and method that may have led to the crisis in cybernetics and that block the full emergence of a cybernetic paradigm?

One such failure, I believe, resulted from the practice of drawing attention to analogies between biological phenomena and their engineering counterparts. The assumption was then made that the engineering theory could fully explain the biological phenomenon. The similarity between engineering communication networks and those of the central nervous system is an example. It was assumed that information theory could be directly applied to the analysis of neural signals. The results have not been impressive.

Since the biologist rarely understood the mathematics of information theory, he had to rely on the engineer or physical scientist to select the appropriate mathematics. The engineer, on the other hand, rarely had an adequate understanding of the complexity of the biological phenomenon. As McCulloch once aptly noted, “One has to have a reasonable
knowledge of both engineering and biology in one head, and there is no use having in one room what should be in one head."

But this failure need not be occasion for despair. The analogy remains. A model or theory is no good if it is not falsifiable; and such failures can lead to major advances if appropriately used. In this case, it led to a better appreciation of the differences between neural and engineering communication channels. In an engineering system, a design is required to transmit a signal that faithfully represents the message originated at the source, and to do this in the most efficient manner. The entropy of the source, a quantity based on the ensemble of possible messages, is significant for this task. In a neural communication system, however, what is significant is the relevance of the patterns characterizing the source to the behavior of the organism concerned. The classical paper of Lettin, Maturana, McCulloch, and Pitts—"What the Frog's Eye Tells the Frog's Brain" (1959)—illustrates the difference. A faithful representation of the visual field of the frog is not required. What is important is the detection of small, moving patterns like those cast by a bug on a frog retina—or the global dimming of light intensity caused by the shadow of a large predator that eats frogs. Appreciation of these findings then enabled Moreno-Diaz (1968) to develop a model of the "bug detector network" of the frog retina which accounted for the experimental data.

Another example is the analogy between an engineering servomechanism and the neuromuscular network controlling movement of a limb. A preliminary application of this analogy would lead one to identify the motor born cells of the spinal cord as the comparator, the outputs from these cells to the muscle as the controller, the muscle itself as the controlled system, and the signals from the muscle spindle receptors as the feedback. In actuality, the system is far more complex, and the simplest scheme to characterize it includes not just a given muscle, but its synergists and antagonists as well, which together produce partial rotation about a joint. In addition, some of the motoneurons send outputs not to the muscle itself, but to the muscle spindle receptors, changing the "bias" of this "transducer." The system includes nonlinear components (notably the muscle itself), analog-to-digital converters, time delays, and other complexities. In this example, however, the cybernetic paradigm has stimulated a lot of research and has considerably advanced our understandings of the system; and the rapid development of engineering control theory has provided powerful tools for analysis.

The computer-brain analogy has been intermediate in usefulness, and has evoked the most controversy. Unfortunately, some early workers in artificial intelligence were excessively optimistic in their predictions of what it could do, and overly simplistic in claims made for some of its results. This has resulted in severe criticisms which went to the opposite extreme. In my opinion, this still remains one of the most potentially useful parts of the cybernetic paradigm—not so much for the results that it may ultimately achieve, as it is for two by-products of the endeavor. First, programming a computer to simulate neural (or mental) activity imposes a tough discipline, a requirement for clear thinking, that has all too often been lacking in the psychological and social sciences. And second, the failures and inadequacies of artificial intelligence research have advanced our understanding of, and appreciation for, the complexity and sophistication of "neurocomputers."

The comparative study of brains and computers is as scientifically legitimate an enterprise as the accepted practice of studying the brains of lower animals for clues to the performance of the nervous system. Ultimately, brains and computers may be seen as special cases of a still more general kind of system, the theory of which will explain the behavior of both. The development of such a theory may yet emerge from artificial intelligence research.

Conclusion

The foregoing is admittedly inadequate and incomplete, and reflects the interests and biases of the author. I have taken Wiener's book as a basis for defining cybernetics and describing its content, which unfairly ignores the contributions of others. And even here, I have not considered other themes Wiener discusses, such as cybernetics and psychopathology. I hope the reader will pardon these flaws and remedy these deficiencies.

Nevertheless, the survey leaves me more confident than ever that cybernetics will survive the "crisis" and prosper, as one of the more significant scientific developments of the twentieth century.

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The Quality Of Life and How To Improve It

The emphasis for the 1980 Acapulco Congress will include: applications of systems research in social and natural sciences, advances in development of systems research methodologies, and applications of cybernetics and systems concepts to ethical management of human systems. A special Symposium will be arranged to focus on the question of how systems research and cybernetics can be practically utilized to help us improve the quality of human life in our society and what scientists from different fields can do to more effectively mobilize their resources toward this goal.

Any topic from the area of Applied Systems Research and Cybernetics can be presented at the Congress. The submitted papers may cover: system modeling and simulation, research on social, political, economic and ecological systems, methodology for analysis of systems behavior, systems research in education, health care systems, biological systems, information-processing and communication systems, ethical and philosophical aspects of human systems management, and others.

All inquiries should be sent to:

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