It is more than 15 years since the writer has first presented, to a larger public, the proposal of a General System Theory. Since then, this conception has been widely discussed and was applied in numerous fields of science. When an early reviewer found himself "hustled into awed silence" by the idea of a General System Theory, now in spite of obvious limitations, different approaches and legitimate criticism, few would deny the legitimacy and fertility of the interdisciplinary systems approach.

Even more: The systems concept has not remained in the theoretical sphere, but became central in certain fields of applied science. When first proposed, it appeared to be a particularly abstract and daring, theoretical idea. Nowadays "systems engineering," "research," "analysis" and similar terms have become job denominations. Major industrial enterprises and government agencies have departments, committees or at least specialists to the purpose; and many universities offer curricula and courses for training.

Thus the present writer was vindicated when he was among the first to predict that the concept of "system" is to become a fulcrum in modern scientific thought. In the words of a practitioner of the science [R. L. Ackoff]:

In the last two decades we have witnessed the emergence of the "system" as a key concept in scientific research. Systems, of course, have been studied for centuries, but something new has been added... The tendency to study systems as an entity rather than as a conglomerate of parts is consistent with the tendency in contemporary science no longer to isolate phenomena in narrowly confined contexts, but rather to open interactions for examination and to examine larger and larger slices of nature. Under the banner of systems research (and its many synonyms) we have also witnessed a convergence of many more specialized contemporary scientific developments... These research pursuits and many others are being interwoven into a cooperative research effort involving an ever-widening spectrum of scientific and engineering disciplines. We are participating in what is probably the most comprehensive effort to attain a synthesis of scientific knowledge yet made.

This, however, does not preclude but rather implies that obstacles and difficulties are by no means overcome as is only to be expected in a major scientific reorientation. A reassessment of General Systems Theory, its foundations, achievements, criticisms and prospects therefore appears in place. The present study aims at this purpose.

According to the Preface to the 5th volume of General Systems by Meyer, the greatest number of enquiries made asks for "new statements describing the method and significance of the idea." Another central theme is "the organicistic viewpoint." As one of the original proponents of the Society for General Systems Research and founders of the organicistic viewpoint in biology, the author feels obliged to answer this challenge as well as readily admitted limitations of his knowledge and techniques permit.

I. The Rise of Interdisciplinary Theories

The motives leading to the postulate of a general theory of systems can be summarized under a few headings.
1. Up to recent times the field of science as a nomothetic endeavor, i.e., trying to establish an explanatory and predictive system of laws, was practically identical with theoretical physics. Few attempts at a system of laws in non-physical fields gained general recognition; the biologist would first think of genetics. However, in recent times the biological, behavioral and social sciences have come into their own, and so the problem became urgent whether an expansion of conceptual schemes is possible to deal with fields and problems where application of physics is not sufficient or feasible.

2. In the biological, behavioral and sociological fields, there exist predominant problems which were neglected in classical science or rather which did not enter into its considerations. If we look at a living organism, we observe an amazing order, organization, maintenance in continuous change, regulation and apparent teleology. Similarly, in human behavior goal-seeking and purposefulness cannot be overlooked, even if we accept a strictly behavioristic standpoint. However, concepts like organization, directiveness, teleology, etc., just do not appear in the classic system of science. As a matter of fact, in the so-called mechanistic world view based upon classical physics, they were considered as illusory or metaphysical. This means, to the biologist for example, that just the specific problems of living nature appeared to lie beyond the legitimate field of science.

3. This in turn was closely connected with the structure of classical science. The latter was essentially concerned with two-variable problems, linear causal trains, one cause and one effect, or with few variables at the most. The classical example is mechanics. It gives perfect solutions for the attraction between two celestial bodies, a sun and a planet, and hence permits to exactly predict future constellations and even the existence of still undetected planets. However, already the three-body problem of mechanics is unsolvable in principle and can only be approached by approximations. A similar situation exists in the more modern field of atomic physics. Here also two-body problems such as that of one proton and electron are solvable, but trouble arises with the many-body problem. One-way causality, the relation between "cause" and "effect" or of a pair or few variables cover a wide field. Nevertheless, many problems particularly in biology and the behavioral and social sciences, essentially are multivariable problems for which new conceptual tools are needed. Warren Weaver, cofounder of information theory, had expressed this in an often-quoted statement. Classical science, he stated, was concerned either with linear causal trains, that is, two-variable problems; or else with unorganized complexity. The latter can be handled with statistical methods and ultimately stems from the second principle of thermodynamics. However, in modern physics and biology, problems of organized complexity, that is, interaction of a large but not infinite number of variables, are popping up everywhere and demand new conceptual tools.

4. What has been said are not metaphysical or philosophic contentsions. We are not erecting a barrier between inorganic and living nature which obviously would be inappropriate in view of intermediates such as viruses, nucleoproteins and self-duplicating units in general which in some way bridge the gap. Nor do we protest that biology is in principle "irreducible to physics" which also would be out of place in view of the tremendous advances of physical and chemical explanation of life processes. Similarly, no barrier between biology and the behavioral and social sciences is intended. This, however, does not obviate the fact that in the fields mentioned we do not have appropriate conceptual tools serving for explanation and prediction as we have in physics and its various fields of application.

5. It therefore appears that an expansion of science is required to deal with those aspects which are left out in physics and happen to concern just the specific characteristics of biological, behavioral, and social phenomena. This amounts to new conceptual models to be introduced. Every science is a model in the broad sense of the word, that is a conceptual structure intended to reflect certain aspects of reality. One such model is the system of physics—and it is an incredibly successful one. However, physics is but one model dealing with certain aspects of reality. It needs not to have monopoly, nor is it the reality as mechanistic methodology and metaphysics presupposed. It apparently does not cover all aspects and represents, as many specific problems in biology and behavioral science show, a limited aspect. Perhaps it is possible to introduce other models dealing with aspects outside of physics.

These considerations are of a rather abstract nature. So perhaps some personal interest may be introduced by telling how the present author was led into this sort of problem.

When, some 40 years ago, I started my life as a scientist, biology was involved in the mechanism vitalism controversy. The mechanistic procedure essentially was to resolve the living organism into parts and partial processes: the organism was an
aggregate of cells, the cell one of colloids and organic molecules, behavior a sum of unconditional and conditioned reflexes, and so forth. The problems of organization of these parts in the service of maintenance of the organism, of regulation after disturbances and like were either by-passed or, according to the theory known as vitalism, explainable only by the action of soul-like factors, little hobo-goblins as it were, hovering in the cell or the organism—which obviously was nothing less than a declaration of bankruptcy of science. In this situation, I was led to advocate the so-called organicist viewpoint. In one brief sentence, it means that organisms are organized things and, as biologists, we have to find out about it. I tried to implement this organicist program in various studies on metabolism, growth, and biophysics of the organism. One way in this respect was the so-called theory of open systems and steady states which essentially is an expansion of conventional physical chemistry, kinetics and thermodynamics. It appeared, however, that I could not stop on the way once taken and so I was led to a still further generalization which I called “General System Theory.” The idea goes back for some considerable time—I presented it first in 1937 in Charles Morris’ philosophy seminar at the University of Chicago. However, at this time theory was in bad reputation in biology, and I was afraid of what Gauss, the mathematician, called the “clamor of the Boeotians.” So I left my drafts in the drawer, and it was only after the war that my first publications in this respect appeared.

Then, however, something interesting and surprising happened. It turned out that a change in intellectual climate had taken place, making model building and abstract generalizations fashionable. Even more: quite a number of scientists had followed similar lines of thought. So General System Theory, after all, was not isolated or a personal idiosyncrasy as I have believed, but rather was one within a group of parallel developments.

Naturally, the maxims enumerated above can be formulated in different ways and using somewhat different terms. In principle, however, they express the viewpoint of the more advanced thinkers of our time and the common ground of system theorists. The reader may, for example, compare the presentation given by Rapoport and Horwitz which is an excellent and independent statement and therefore shows even better the general agreement.

There is quite a number of novel developments intended to meet the goals indicated above. We may enumerate them in a brief survey:

(1) Cybernetics, based upon the principle of feedback or circular causal trains providing mechanisms for goal-seeking and self-controlling behavior.

(2) Information theory, introducing the concept of information as a quantity measurable by an expression isomorphic to negative entropy in physics, and developing the principles of its transmission.

(3) Game theory, analyzing in a novel mathematical framework, rational competition between two or more antagonists for maximum gain and minimum loss.

(4) Decision theory, similarly analyzing rational choices, within human organizations, based upon examination of a given situation and its possible outcomes.

(5) Topology or relational mathematics, including non-metrical fields such as network and graph theory.

(6) Factor analysis, i.e., isolation by way of mathematical analysis, of factors in multivariable phenomena in psychology and other fields.

(7) General system theory in the narrower sense (G.S.T.), trying to derive from a general definition of “system” as complex of interacting components, concepts characteristic of organized wholes such as interaction, sum, mechanization, centralization, competition, finality, etc., and to apply them to concrete phenomena.

While systems theory in the broad sense has the character of a basic science, it has its correlate in applied science, sometimes subsumed under the general name of Systems Science. This development is closely connected with modern automation. Broadly speaking, the following fields can be distinguished:

Systems Engineering, i.e., scientific planning, design, evaluation, and construction of man-machine systems;

Operations research, i.e., scientific control of existing systems of men, machines, materials, money, etc.

Human Engineering, i.e., scientific adaptation of systems and especially machines in order to obtain maximum efficiency with minimum cost in money and other expenses.

A very simple example for the necessity of study of “man-machine systems” is air travel. Anybody crossing continents by jet with incredible speed and having to spend endless hours waiting, queuing, being herded in airports can easily realize that the physical techniques in air travel are at their best, while “organizational” techniques still are on a most primitive level.
Although there is considerable overlapping, different conceptual tools are predominant in the individual fields. In systems engineering, cybernetics and information theory, also general system theory are used. Operations research uses tools such as linear programming and game theory. Human engineering, concerned with the abilities, physiological limitations and variabilities of human beings, includes biomechanics, engineering psychology, human factors, etc., among its tools.

The present survey is not concerned with applied systems science; the reader is referred to Hall’s book as an excellent textbook of systems engineering. However it is well to keep in mind that the systems approach as a novel concept in science has a close parallel in technology. The systems viewpoint in recent science stands in a similar relation to the so-called “mechanistic” viewpoint, as stands systems engineering to physical technology.

All these theories have certain features in common. Firstly, they agree in the emphasis that something should be done about the problems characteristic of the behavioral and biological sciences, but not dealt with in conventional physical theory. Secondly, these theories introduce concepts and models novel in comparison to physics: for example, a generalized system concept, the concept of information compared to energy in physics. Thirdly, these theories are particularly concerned with multivariable problems, as mentioned before. Fourthly, these models are interdisciplinary and transcend the conventional fields of science. If, for example, you scan the Yearbooks of the Society for General Systems Research, you notice the breadth of application: Considerations similar or even identical in structure are applied to phenomena of different kinds and levels, from networks of chemical reactions in a cell to populations of animals, from electrical engineering to the social sciences. Similarly, the basic concepts of cybernetics stem from certain special fields in modern technology. However, starting with the simplest case of a thermostat which by way of feedback maintains a certain temperature and advancing to servomechanisms and automation in modern technology, it turns out that similar schemes are applicable to many biological phenomena of regulation or behavior. Even more, in many instances there is a formal correspondence or isomorphism of general principles or even of special laws. Similar mathematical formulations may apply to quite different phenomena. This entails that general theories of systems, among other things, are labor-saving devices: A set of principles may be transferred from one field to another, without the need to duplicate the effort as has often happened in science of the past.

Fifthly, and perhaps most important: Concepts like wholeness, organization, teleology and directive-ness appeared in mechanistic science to be un-scientific or metaphysical. Today they are taken seriously and as amenable to scientific analysis. We have conceptual and in some cases even material models which can represent those basic characteristics of life and behavioral phenomena.

An important consideration is that the various approaches enumerated are not, and should not be considered to be monopolistic. One of the important aspects of the modern changes in scientific thought is that there is no unique and all-embracing “world system.” All scientific constructs are models representing certain aspects or perspectives of reality. This even applies to traditional physics: far from being a metaphysical presentation of ultimate reality (as the materialism of the past proclaimed and modern positivism still implies) it is but one of these models and, as recent developments show, neither exhaustive nor unique. The various “systems theories” also are models that mirror different aspects. They are not mutually exclusive and often combined in application. For example, certain phenomena may be amenable to scientific exploration by way of cybernetics, others by way of general system theory; or even in the same phenomenon, certain aspects may be describable in the one or the other way. Cybernetics combine the information and feedback models, models of the nervous system net and information theory, etc. This, of course, does not preclude but rather implies the hope for further synthesis in which the various approaches of the present toward a theory of “wholeness” and “organization” may be integrated and unified. Actually, such further syntheses, e.g., between irreversible thermodynamics and information theory, are slowly developing.

The differences of these theories are in the particular model conceptions and mathematical methods applied. We therefore come to the question in what ways the program of systems research can be implemented.

2. Methods of General Systems Research

Ashby has admirably outlined two possible ways or general methods in systems study:

Two main lines are readily distinguished. One, already well developed in the hands of von Bertalanffy and his
co-workers, takes the world as we find it, examines the various systems that occur in it—zoological, physiological, and so on—and then draws up statements about the regularities that have been observed to hold. This method is essentially empirical. The second method is to start at the other end. Instead of studying first one system, then a second, then a third, and so on, it goes to the other extreme, considers the set of all conceivable systems and then reduces the set to a more reasonable size. This is the method I have recently followed.

It will easily be seen that all systems studies follow one or the other of these methods or a combination of both. Each of these approaches has its advantages as well as shortcomings.

(1) The first method is empirico-intuitive; it has the advantage that it remains rather close to reality and can easily be illustrated and even verified by examples taken from the individual fields of science. On the other hand, the approach lacks mathematical elegance and deductive strength and, to the mathematically minded, will appear naive and unsystematic.

Nevertheless, the merits of this empirico-intuitive procedure should not be minimized.

The present writer has stated a number of “system principles,” partly in the context of biological theory and without explicit reference to G.S.T., partly in what emphatically was entitled an “Outline” of this theory. This was meant in the literal sense: It was intended to call attention to the desirability of such field, and the presentation was in the way of a sketch or blueprint, illustrating the approach by simple examples.

However, it turned out that this intuitive survey appears to be remarkably complete. The main principles offered such as wholeness, sum, centralization, differentiation, leading part, closed and open systems, finality, equifinality, growth in time, relative growth, competition, have been used in manifold ways (e.g., general definition of system; types of growth; systems engineering; social work). Exeptiong minor variations in terminology intended for clarification or due to the subject matter, no principles of similar significance were added—even though this would be highly desirable. It is perhaps even more significant that this also applies to considerations which do not refer to the present writer’s work and hence cannot be said to be unduly influenced by it. Pursuit of studies such as those by Beer and Kremanskiy on principles, Bradley and Calvin on the network of chemical reactions, Haire on growth or organizations, etc., will easily show that they are also using the “principles.”

(2) The way of deductive systems theory was followed by Ashby. A more informal presentation which summarizes Ashby’s reasoning lends itself particularly well to analysis.

Ashby asks about the “fundamental concept of machine” and answers the question by stating “that its internal state, and the state of its surroundings, defines uniquely the next state it will go to.” If the variables are continuous, this definition corresponds to the description of a dynamic system by a set of ordinary differential equations with time as the independent variable. However, such representation by differential equations is too restricted for a theory to include biological systems and calculating machines where discontinuities are ubiquitous. Therefore the modern definition is the “machine with input”: It is defined by a set $S$ of internal states, a set $I$ of input and a mapping $f$ of the product set $I \times S$ into $S$. “Organization,” then, is defined by specifying the machine’s states $S$ and its conditions $I$. If $S$ is a product set $S = nI$, with $i$ as the parts and $T$ is specified by the mapping $f$. A “self-organizing” system, according to Ashby, can have two meanings, namely: (1) The system starts with its parts separate, and these parts then change toward forming connections (example: cells of the embryo, first having little or no effect on one another, join by formation of dendrites and synapses to form the highly interdependent nervous system). This first meaning is “changing from unorganized to organized.” (2) The second meaning is “changing from a bad organization to a good one” (examples: a child whose brain organization makes it fire-seeking at first, while a new brain organization makes him fire-avoiding; an automatic pilot and plane coupled first by deleterious positive feedback and then improved). “There the organization is bad. The system would be ‘self-organizing’ if a change were automatically made” (changing positive into negative feedback). But “no machine can be self-organizing in this sense” (author’s emphasis).

For adaptation (e.g., of the homeostat or in a self-programming computer) means that we start with a set $S$ of states, and that $f$ changes into $g$, so that organization is a variable, e.g., a function of time $t(1) which has first the value $f$ and later the value $g$. However, this change “cannot be ascribed to any cause in the set $S$; so it must come from some outside agent, acting on the system $S$ as input” (our emphasis). In other terms, to be “self-organizing” the machine $S$ must be coupled to another machine.
not only a clumsy but, in principle, inadequate way to deal with many problems of organization. The author was well aware of this emphasizing that a system of simultaneous differential equations is by no means the most general formulation and is chosen only for illustrative purposes.

However, in overcoming this limitation, Ashby introduced another one. His "modern definition" of system as a "machine with input" as reproduced above, supplants the general system model by another rather special one: the cybernetic model, i.e., a system open to information but closed with respect to entropy transfer. This becomes apparent when the definition is applied to "self-organizing systems." Characteristically, the most important kind of these has no place in Ashby's model, namely, systems organizing themselves by way of progressive differentiation, evolving from states of lower to states of higher complexity. This is, of course, the most obvious form of "self-organization," apparent in ontogenesis, probable in phylogenesis, and certainly also valid in many social organizations. We have here not a question of "good" (i.e., useful, adaptive) or "bad" organization which, as Ashby correctly emphasizes, is relative on circumstances; increase in differentiation and complexity—whether useful or not—is a criterion that is objective and at least on principle amenable to measurement (e.g., in terms of decreasing entropy, of information). Ashby's contention that "no machine can be self-organizing." more explicitly, that the "change cannot be ascribed to any cause in the set S" but "must come from some outside agent, an input" amounts to exclusion of self-differentiating systems. The reason that such systems are not permitted as "Ashby machines" is patent. Self-differentiating systems that evolve toward higher complexity (decreasing entropy) are, for thermodynamic reasons, possible only as open systems, i.e., systems importing matter containing free energy to an amount over-compensating the increase in entropy due to irreversible processes within the system ("import of negative entropy"). However, we cannot say that "this change comes from some outside agent, an input"; the differentiation within a developing embryo and organism is due to its internal laws of organization, and the input (e.g., oxygen supply which may vary quantitatively, or nutrition which can vary qualitatively within a broad spectrum) makes it only possibly energetically.

The above is further illustrated by additional examples given by Ashby. Suppose a digital computer is carrying through multiplications at random; then the machine will "evolve" toward showing even numbers (because products even x even as well as even x odd give numbers even), and eventually only zeros will be "surviving." In still another version Ashby quotes Shannon's Tenth Theorem, stating that if a correction channel has capacity H, equivocation of the amount H can be removed, but no more. Both examples illustrate the working of closed systems: The "evolution" of the computer is one toward disappearance of differentiation and establishment of maximum homogeneity (analog to the Second Principle in closed systems); Shannon's Theorem similarly concerns closed systems where no negative entropy is fed in. Compared to the information content (organization) of a living system, the imported matter (nutrition, etc.) carries not information but "noise." Nevertheless, its negative entropy is used to maintain or even to increase the information content of the system. This is a state of affairs apparently not provided for in Shannon's Tenth Theorem, and understandably so as he is not treating information transfer in open systems with transformation of matter.

In both respects, the living organism (and other behavioral and social systems) is not an Ashby machine because it evolves toward increasing differentiation and inhomogeneity, and can correct "noise" to a higher degree than an inanimate communication channel. Both, however, are consequences of the organism's character as an open system.

Incidentally, it is for similar reasons that we cannot replace the concept of "system" by the generalized "machine" concept of Ashby. Even though the latter is more liberal compared to the classic one (machines defined as systems with fixed arrangement of parts and processes), the
objections against a "machine theory" of life remain valid.

These remarks are not intended as adverse criticism of Ashby's or the deductive approach in general; they only emphasize that there is no royal road to General Systems Theory. As every other scientific field, it will have to develop by an interplay of empirical, intuitive and deductive procedures. If the intuitive approach leaves much to be desired in logical rigor and completeness, the deductive approach faces the difficulty of whether the fundamental terms are correctly chosen. This is not a particular fault of the theory or of the workers concerned but a rather common phenomenon in the history of science; one may, for example, remember the long debate as to what magnitude—force or energy—is to be considered as constant in physical transformations until the issue was decided in favor of mv^2/2.

In the present writer's mind, G.S.T. was conceived as a working hypothesis; being a practicing scientist, he sees the main function of theoretical models in the explanation, prediction and control of hitherto unexplored phenomena. Others may, with equal right, emphasize the importance of axiomatic approach and quote to this effect examples like the theory of probability, non-Euclidean geometries, more recently information and game theory, which were first developed as deductive mathematical fields, and later applied in physics or other sciences. There should be no quarrel about this point. The danger, in both approaches, is to consider too early the theoretical model as being closed and definitive—a danger particularly important in a field like general systems which is still groping to find its correct foundations.

3. Homeostasis and Open Systems

Among the models mentioned, cybernetics in its application as homeostasis, and G.S.T. in its application to open systems lend themselves most readily for interpretation of many empirical phenomena. The relation of both theories is not always well understood, and hence a brief discussion is in order.

The simplest feedback scheme can be represented as follows (Fig. 1). Modern servomechanisms and automation, as well as many phenomena in the organism, are based upon feedback arrangements far more complicated than the simple scheme (Fig. 1) but the latter is the elementary prototype.

In application to the living organism, the feedback scheme is represented by the concept of homeostasis.

Homeostasis, according to Cannon, is the ensemble of organic regulations which act to maintain the steady states of the organism and are effectuated by regulating mechanisms in such a way that they do not occur necessarily in the same, and often in opposite, direction to what a corresponding external change would cause according to physical laws. The simplest example is homeothermy. According to Van't Hoff's rule in physical chemistry, a decrease in temperature leads to slowing down of the rate of chemical reactions, as it does in ordinary physico-chemical systems and also in poikilothermic animals. In warm-blooded animals, however, it leads to the opposite effect, namely, to an increase of metabolic rate, with the result that the temperature of the body is maintained constant at approximately 37°C. This is effectuated by a feedback mechanism. Cooling stimulates thermogenic centers in the brain thalamus which "turn on" heat-producing mechanisms in the body. A similar feedback pattern is found in a great variety of physiological regulations.

Regulation of posture and the control of actions in animals and man toward a goal are similarly controlled by feedback mechanisms.

In contradistinction to cybernetics concerned with feedback arrangements, G.S.T. is interested in dynamic interaction within multivariable systems. The case particularly important for the living organism is that of open systems. It amounts to saying that there is a system into which matter is introduced from outside. Within the system, the material undergoes reactions which partly may

![Figure 2](image)

Figure 2—Model of a simple open system. The component A is introduced into the system and transformed, in a reversible reaction, into B; it is catabolized in an irreversible reaction, into C which eventually is excreted. K_1, K_2, K_3 are constants of import and export, respectively; k_1, k_2, k_3 are reaction constants. The model approximately corresponds, for example, to protein turnover in an animal organism, A representing amino acids, B proteins, and C products of excretion.)
yield components of a higher complexity. This is what we call anabolism. On the other hand, the material is catabolized and the end products of catabolism eventually leave the system. A simple model of an open system is indicated in Figure 2.

A few main characteristics of open as compared to closed systems are in the fact that, appropriate system conditions presupposed, an open system will attain a steady state in which its composition remains constant, but in contrast to conventional equilibria, this constancy is maintained in a continuous exchange and flow of component material. The steady state of open systems is characterized by the principle of equifinality; that is, in contrast to equilibrium states in closed systems which are determined by initial conditions, the open system may attain a time-independent state independent of initial conditions and determined only by the system parameters. Furthermore, open systems show thermodynamic characteristics which are apparently paradoxical and contradictory to the second principle. According to the latter, the general course of physical events (in closed systems) is toward increasing entropy, leveling down of differences and states of maximum disorder. In open systems, however, with transfer of matter import of "negative entropy" is possible. Hence, such systems can maintain themselves at a high level, and even evolve toward an increase of order and complexity—as is indeed one of the most important characteristics of life processes.

The open-system model also has a wide application. According to its character, it is particularly applicable to phenomena showing nonstructural, dynamic interaction of processes, such as those of metabolism, growth, metabolic aspects of excitation, etc.

Speaking more generally, living systems can be defined as hierarchically organized open systems, maintaining themselves, or developing toward a steady state. Disease is the life process regulating toward normacy after disturbance, owing to the equifinality of biological systems and with the assistance of the physician. In this way, the *viv medicaevarix naturae* of old is divested of its metaphysical paraphernalia; it is not a vitalistic agent but an expression of the dynamics of living systems, maintaining and reestablishing, so far as possible, the steady state.

In this way, the theory of open systems accounts for basic characteristics of the living organism which have baffled physicists, biologists, and philosophers, and appeared to be violations of the laws of physics, explainable only by vitalistic factors beyond the competence of science and scientific explanation.

Thus "feedback" and "open systems" are two models for biological and possibly behavioral phenomena. It should be made clear that the term "homeostasis" can be used in two ways. It is either taken in the original sense as preferred by Cannon and illustrated by examples like maintenance of body temperature and other physiological variables by feedback mechanisms—or else the term is often used as a synonym for organic regulation and adaptation in general. This is a question of semantics. However, it is a wise rule in the natural sciences to use terms in the sense originally attached to them by their authors. So I propose to use the word homeostasis in its narrower but well-defined sense, and this has important consequences, as it reveals certain limitations which are often forgotten.

As was already emphasized, regulations of the homeostasis or feedback type are abundant in the mature higher organism. However, it is clear from the scheme (Fig. 1) or any other flow diagram that feedback represents a machine-like arrangement, that is, an order of processes based upon fixed arrangements and representing linear, though circular, causal trains. The primary phenomena of organic regulation, e.g., the regulations in early embryonic development, in regeneration, etc., appear to be of a different nature. It seems that the primary regulations in the organism result from dynamic interaction within a unitary open system that reestablishes its steady state. Superimposed upon this by way of progressive mechanism are secondary regulatory mechanisms governed by fixed structures especially of the feedback type.

Although the homeostasis model transcends older mechanistic models by acknowledging directiveness in self-regulating circular processes, it still adheres to the machine theory of the organism. This also applies to a second aspect. An essential component of the mechanistic view is a utilitarian conception which is deeply connected with the economic outlook of the 19th and early 20th centuries. This is well-known, for example, in the history of Darwinism: Struggle for existence and survival of the fittest are a biological version of the economic model of free competition. This utilitarian or economic viewpoint also prevails in the concept of homeostasis: The organism is essentially envisaged as an aggregate mechanism for maintenance of minimum costs. However, there seem to be plenty of non-utilitarian structures and functions in the living world.
The concept of homeostasis also retains a third aspect of the mechanistic view. The organism is essentially considered to be a reactive system. Outside stimuli are answered by proper responses in such a way as to maintain the system. The feedback model (Fig. 1) is essentially the classical stimulus-response scheme, only the feedback loop being added. However, an overwhelming amount of facts shows that primary organic behavior as, for example, the first movements of the fetus, are not reflex responses to external stimuli, but rather spontaneous mass activities of the whole embryo or larger areas. Reflex reactions answering external stimuli and following a structured path appear to be superimposed upon primitive automatism, ontogenetically and phylogenetically, as secondary regulatory mechanisms. These considerations win particular importance in the theory of behavior, as we shall see later on.

In this sense, it appears that in development and evolution dynamic interaction (open system) precedes mechanization (structured arrangements particularly of a feedback nature). In a similar way, G.S.T. can logically be considered the more general theory; it includes systems with feedback constraints as a special case, but this assertion would not be true vice versa. It need not be emphasized that this statement is a program for future systematization and integration of G.S.T. rather than a theory presently achieved.

4. Criticism of General System Theory

A discussion of G.S.T. must take account of the objections raised, both to clarify misunderstanding and to utilize criticism for improvement.

A "devastating" criticism of "General Behavior Systems Theory" by Buck would hardly deserve discussion were it not for the fact that it appeared in the widely read Minnesota Studies in the Philosophy of Science, a leading publication of modern positivism. In passing, it should be noted that the lack of interest in, or even hostility of logical positivists against, G.S.T. is a rather remarkable phenomenon. One would expect that a group whose program is "Unified Science" should be concerned with a novel approach to this problem, however immature it may still be. The opposite is the case; no contribution or even pertinent criticism came forward from these quarters. The reason is not difficult to see. Abandoning the debatable but challenging position of Logical Positivism and replacing it by a rather tame "Empirical Realism," modern positivists have come back to what is generally agreed among modern scientists, avoiding commitments which trespass and would imply an adventure of thought. It needs to be said that modern positivism has been a singularly sterile movement. It is paradoxical that the declared "philosophers of science" have neither contributed any empirical research nor new idea to modern science—while professional or half-time philosophers who were justly censored for their "mysticism," "metaphysics," or "vitalism," indubitably did. Eddington and Jeans in physics, Driesch in biology, Spengler in history are but a few examples.

Buck's critique is not directed against the present author but against J. G. Miller and his Chicago group. Its essence is in the "So what?" argument: Supposing we find an analogy or formal identity in two "systems," it means nothing. Compare, for example, a chessboard and a mixed dinner party; a general statement expressing the alternation of black and white squares on the one hand, and of men and women on the other can be made. "If one is tempted to say 'All right, so they're structurally analogous, so what?' my answer is 'So, nothing.'" In the same vein, Buck pokes fun at some of Miller's more hazardous comparisons, such as of the behavior of slime molds and Londoners during the blitz. He asks, "What are we to conclude from all this? That Londoners are a form of slime mold? That myxamoebae are a sort of city dweller?" Or "if no conclusion, why bother with the analogy at all?"

As proof of the emptiness of analogies Buck offers the example of a scientist, A, who finds a formula for the rate of formation of frost in a refrigerator; of another, B, formulating the rate of carbon deposit in an automobile motor; and a "general systems theorist," C, who notices that both formulas are the same. The similarity of mathematical expressions and models is, according to Buck, "sheer coincidence"—it does not prove that a refrigerator is an automobile or vice versa, but only that both are "systems" of some sort. This, however, is a meaningless statement; for

One is unable to think of anything, or of any combination of things, which could not be regarded as a system. And, of course, a concept that applies to everything is logically empty.

Regardless of the question whether Miller's is a particularly felicitous presentation, Buck has simply missed the issue of a general theory of systems. Its aim is not more or less hazy analogies; it is to establish principles applicable to entities
not covered in conventional science. Buck's criticism is, in principle, the same as if one would criticize Newton's law because it draws a loose "analog" between apples, planets, etc. and tide and many other entities; or if one would declare the theory of probability meaningless because it is concerned with the "analogous" of games of dice, mortality statistics, molecules in a gas, the distribution of hereditary characteristics, and a host of other phenomena.

The basic role of "analog"—or rather of isomorphisms and models in science—has been lucidly discussed by Ashby. Hence a few remarks in answer to Buck will suffice.

The "So what?" question mistakes a method which is fundamental in science although—like every method—it can be misused. Even Buck's first example is not a meaningless pseudoproblem; in the "analog" of chessboard and dinner party topology may find a common structural principle that is well worth stating. Generally speaking, the use of "analog" (isomorphism, logical homology)—or, what amounts to nearly the same, the use of conceptual and material models—is not a half-poetical play but a potent tool in science. Where would physics be without the analogy or model of "wave," applicable to such dissimilar phenomena as water waves, sound waves, light and electromagnetic waves, "waves" (in a rather Pickwickian sense) in atomic physics? "Analogies" may pose fundamental problems, as for example, the analogy (logically not dissimilar from that of chessboard and dinner party) of Newton's and Coulomb's law which raises the question (one of the most basic for "Unified Science") of a general field theory uniting mechanics and electrodynamics. It is commonplace in cybernetics that systems which are different materially; e.g., a mechanical and an electrical system, may be formally identical; far from considering this as a meaningless So what? the researcher has to work out the common structure (flow diagram), and this may be of incomparable value for practical technology.

A similar lack of understanding is manifest in the criticism of the system concept. By the token ("One is unable to think of anything" which would not show the properties in question) mechanics would have to be refused as "logically empty" because every material body shows mass, acceleration, energy, etc. In the following paragraphs of his paper, Buck has some glimpse of this truism, but he soon comes back to ridiculing Miller's use of "analogies."

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fish and to apply system principles) and in considering G.S.T. "a philosophy of modern science." With respect to the first item, the present study is devoted to just this problem. The second point is a misunderstanding. G.S.T. in its present form is one—and still very imperfect—model among others. Were it completely developed, it would indeed incorporate the "organismic" world view of our time, with its emphasis on problems of wholeness, organization, directiveness, etc., in a similar way as when previous philosophies have presented a mathematical world view (philosophies more geometricos), a physicalistic one (the mechanistic philosophy based upon classical physics), etc., corresponding to scientific development. Even then, this "organismic" picture would not claim to be a "Nothing-but" philosophy: It would remain conscious that it only presents certain aspects of reality (richer and more comprehensive than previous ones, as corresponds to the advance of science), but never exhaustive, exclusive or final.

According to the authors, Marxist-Leninist philosophy "formulates a series of most important methodological principles of analysis of complex systems"; Soviet scientists "attempt to give a general definition of the notion of systems and to obtain a classification." Difficulties in international communication make it unfortunately impossible to the present writer to evaluate these claims.

Another criticism backed by the same welan-

khanung is that of Kamaruy. The main arguments are:

(1) Underestimation of the structural and morphologic aspects of organization of the theory of open systems (and implicitly in G.S.T.). The theory of open system does not "solve" the problem of life, its origin and evolution which is successfully attacked in modern biochemistry, submicroscopic morphology, physiological genetics, etc. The reply to this is that the functional and processual aspect has been emphasized in the theory, particularly in contradistinction to structural, homeostatic mechanisms. But neither the importance of the latter is denied, nor of course the specificity of the material basis of life. "Morphology and physiology are different and complementary ways of studying the same integrated object." If one wishes, this may be called a "dialectic unity of structure and function."

(2) Neglect of "qualitative specificity" of biological open system and of the specific "dynamics" of the first. The reply is: Thermo-
dynamic considerations (of machines, chemical reactions, organisms, etc.) permit balance statements regarding the system as a whole, without entering into, or even knowing partial reactions, components, organization, etc., in detail. Hence part of the "theory of open systems" is concerned with such over-all balances of the system as a whole. If, however, the theory is applied to individual processes such as formation of proteins, behavior of tracers in the organism, ionistic steady states, etc., the "specificity" of the respective components enters as a matter of course.

5. Advances of General System Theory

The decisive question is that of the explanatory and predictive value of the "new theories" attacking the host of problems around wholeness, teleology, etc. Of course, the change in intellectual climate which allows us to see new problems which were overlooked previously, or to see problems in a new light, is in a way more important than any single and special application. The "Copernican Revolution" was more than the possibility something to better calculate the movement of the planets; general relativity more than an explanation of a very small number of recalcitrant phenomena in physics; Darwinism more than a hypothetical answer to zoological problems; it was the changes in the general frame of reference that mattered. Nevertheless, the justification of such change ultimately is in specific achievements which would not have obtained without the new theory.

There is no question that new horizons have been opened up but the relations to empirical facts often remain tenuous. Thus, information theory has been hailed as a "major breakthrough" but outside the original technological field contributions have remained relatively scarce. In psychology, they are so far limited to rather trivial applications such as rote learning, etc. When, in biology, DNA is spoken of as "coded information" and of "breaking the code" when the structure of nucleic acids is elucidated, this is more a façon de parler than added insight into the control of protein synthesis. "Information theory, although useful for computer design and network analysis, has so far not found a significant place in biology." Game theory, too, is a novel mathematical development which was considered to be comparable in scope to Newtonian mechanics and the introduction of calculus; again, "the applications are meager and faltering" (the reader is urgently referred to Rapoport's discussions on information and game theory which admirably analyze the problems here mentioned). The same is seen in decision theory from which considerable
gain in applied systems science was expected; but as regards the much-advertised military and business games, "there has been no controlled evaluation of their performance in training, personnel selection, and demonstration."

A danger in recent developments should not remain unmentioned. Science of the past (and partly still the present) was dominated by one-sided empiricism. Only collection of data and experiments were considered as being "scientific" in biology (and psychology); "theory" was equated with "speculation" or "philosophy," forgetting that a mere accumulation of data, although steadily piling up, does not make a "science." Lack of recognition and support for development of the necessary theoretical framework and unfavorable influence on experimental research itself (which largely became an at-random, hit-or-miss endeavor) was the consequence. This has, in certain fields, changed to the contrary in recent years. Enthusiasm for the new mathematical and logical tools available has led to feverish "model building" as a purpose in itself and often without regard to empirical fact. However, conceptual experimentation at random has no greater chances of success than at-random experimentation with biological, psychological, or clinical material. In the words of Ackoff, there is the fundamental misconception in game (and other) theory to mistake for a "problem" what actually is only a mathematical "exercise." One would do well to remember the old Kantian maxim that experience without theory is blind, but theory without experience a mere intellectual play.

The case is somewhat different with cybernetics. The model here applied is not new; although the enormous development in the field dates from the introduction of the name, Cybernetics, application of the feedback principle to physiological processes goes back to R. Wagner's work nearly 40 years ago. The feedback and homeostasis model has since been applied to innumerable biological phenomena and—somewhat less persuasively—in psychology and the social sciences. The reason for the latter fact is, in Rapoport's words that usually, there is a well-marked negative correlation between the scope and the soundness of the writings. . . . The sound work is confined either to engineering or to rather trivial applications; ambitious formulations remain vague.

This, of course, is an ever-present danger in all approaches to general systems theory; doubtless, there is a new compass of thought but it is difficult to steer between the scylla of the trivial and the charybdis of mistaking neologisms for explanation.

The following survey is limited to "classical" general system theory—"classical" not in the sense that it claims any priority or excellence, but that the models used remain in the framework of "classical" mathematics in contradistinction to the "new" mathematics in game, network, information theory, etc. This does not imply that the theory is merely application of conventional mathematics. On the contrary, the system concept poses problems which are partly far from being answered. In the past, system problems have led to important mathematical developments such as Volterra's theory of integro-differential equations, of systems insight that the behavior depends not only on actual conditions but also on previous history. Presently important problems are waiting for further developments, e.g., a general theory of non-linear differential equations, of steady states and rhythmic phenomena, a generalized principle of least action, the thermodynamic definition of steady states, etc.

It is, of course, irrelevant whether or not research was explicitly labeled as "general system theory." No complete or exhaustive review is intended. The aim of this unpretentious survey will be fulfilled if it can serve as a sort of guide to research done in the field, and to areas that are promising for future work.

OPEN SYSTEMS

The theory of open systems is an important generalization of physical theory, kinetics and thermodynamics. It has led to new principles and insight, such as the principle of equipotential, the generalization of the second thermodynamic principle, the possible increase of order in open systems, the occurrence of periodic phenomena of overshoot and false start, etc. The possibility of measuring organization in terms of entropy ("chain entropy" of high molecular compounds showing a certain order of component molecules) deserves further attention.

The extensive work done cannot be reviewed here. . . . It should be briefly mentioned, however, that apart from theoretical developments, the field has two major applications, i.e., in industrial chemistry and in biophysics.

The applications of "open systems" in biochemistry, biophysics, physiology, etc., are too numerous to permit more than brief mentioning in the present study. The impact of the theory follows from the fact that the living organism, the
cell as well as other biological entities essentially are steady states (or evolving toward such states).

This implies the fundamental nature of the theory in the biological realm, and a basic reorientation in many of its specialties. Among others, the theory was developed and applied in such fields as, e.g., the network of reactions in photosynthesis, calculation of turnover rates in isotope experiments, energy requirements for the maintenance of body proteins, transport processes, maintenance of ion concentrations in the blood, radiation biology, excitation and propagation of nerve impulses, and others. The organism is in a steady state not only with respect to its chemical components, but also to its cells; hence the numerous modern investigations on cell turnover and renewal have also to be included here. Beside the work already cited, results and impending problems in biophysics and related fields may be found in Netter (1959).

There are certainly relations between irreversible thermodynamics of open systems, cybernetics, and information theory, but they are still unexplored. First approaches to these problems are those by Foster, Rapoport and Trucco, and by Tribus. Another interesting approach to metabolism systems was made by Rosen (1960) who instead of conventional reaction equations, applied "relational theory" using mapping by way of block diagrams.

Beyond the individual organism, systems principles are also used in population dynamics and ecologic theory. Dynamic ecology, i.e., the succession and climax of plant populations, is a much-cultivated field which, however, shows a tendency to slide into verbalism and terminological debate. The systems approach seems to offer a new viewpoint. Whitaker has described the sequence of plant communities toward a climax formation in terms of open systems and equifinality. According to this author, the fact that similar climax formations may develop from different initial vegetations is a striking example of equifinality, and one where the degree of independence of starting conditions and of the course development has appeared even greater than in the individual organism. A quantitative analysis on the basis of open systems in terms of production of biomass, with climax as steady state attained, was given by Patten.

The open-system concept has also found application in the earth sciences, geomorphology and meteorology, drawing a detailed comparison of modern meteorological concepts and Bertalanffy's organismic concept in biology. It may be remembered that already Prigogine in his classic mentioned meteorology as one possible field of application of open systems.

GROWTH-IN-TIME

The simplest forms of growth which, for this reason, are particularly apt to show the isomorphism of law in different fields, are the exponential and the logistic. Examples are, among many others, the increase of knowledge of number of animal species, publications on drosophila, of manufacturing companies. Boulding and Keiter have emphasized a general theory of growth.

The theory of animal growth after Bertalanffy (and others)—which, in virtue of using overall physiological parameters ("anabolism," "catabolism") may be subsumed under the heading of G.S.T. as well as under that of biophysics—has been surveyed in its various applications.

RELATIVE GROWTH

A principle which is also of great simplicity and generality concerns the relative growth of components within a system. The simple relationship of allometric increase applies to many growth phenomena in biology (morphology, biochemistry, physiology, evolution).

A similar relationship obtains in social phenomena. Social differentiation and division of labor in primitive societies as well as the process of urbanization (i.e., growth of cities in comparison to rural population) follow the allometric equation. Application of the latter offers a quantitative measure of social organization and development, apt to replace the usual, intuitive judgments. The same principle apparently applies to the growth of staff compared to total number of employees in manufacturing companies.

COMPETITION AND RELATED PHENOMENA

The work in population dynamics by Volterra, Lotka, Gause and others belongs to the classics of G.S.T., having first shown that it is possible to develop conceptual models for phenomena such as the "struggle for existence" that can be submitted to empirical test. Population dynamics and related population genetics have since become important fields in biological research.

It is important to note that investigation of this kind belongs not only to basic but also to applied biology. This is true of fishery biology where theoretical models are used to establish optimum conditions for the exploitation of the
sea (survey of the more important models: Watt). The most elaborate dynamic model is by Beverton and Holt developed for fish populations exploited in commercial fishery but certainly of wider application. This model takes into account recruitment (i.e., entering of individuals into the population), growth (assumed to follow the growth equations after Bertalanffy), capture (by exploitation), and natural mortality. The practical value of this model is illustrated by the fact that it has been adopted for routine purposes by the Food and Agriculture Organization of the United Nations, the British Ministry of Agriculture and Fisheries and other official agencies.

Richardson's studies on armaments races, notwithstanding their shortcomings, dramatically show the possible impact of the systems concept upon the most vital concerns of our time. If rational and scientific considerations matter at all, this is one way to refute such catch words as Si vis pacem para bellum.

The expressions used in population dynamics and the biological "struggle for existence," in econometrics, in the study of armament races (and others) all belong to the same family of equations. A systematic comparison and study of these parallelisms would be highly interesting and rewarding. One may, for example, suspect that the laws governing business cycles and those of population fluctuations according to Volterra stem from similar conditions of competition and interaction in the system.

In a non-mathematical way, Boulding has discussed what he calls the "Iron Laws" of social organizations: the Malthusian law, the law of optimum size of organizations, existence of cycles, the law of oligopoly, etc.

SYSTEMS ENGINEERING

The theoretical interest of systems engineering and operations research is in the fact that entities whose components are most heterogeneous—men, machines, buildings, monetary and other values, inflow of raw material, outflow of products and many other items—can successfully be submitted to systems analysis.

As already mentioned, systems engineering employs the methodology of cybernetics, information theory, network analysis, flow and block diagrams, etc. Considerations of G.S.T. also enter. The first approaches are concerned with structured, machine-like aspects (yes-or-no decisions in the case of information theory); one would suspect that G.S.T. aspects will win increased importance with dynamic aspects, flexible organizations, etc.

PERSONALITY THEORY

Although there is an enormous amount of theorizing on neural and psychological function in the cybernetic line based upon the brain–computer comparison, few attempts have been made to apply G.S.T. in the narrower sense to the theory of human behavior. For the present purposes, the latter may be nearly equated with personality theory.

We have to realize at the start that personality theory is at present a battlefield of contrasting and controversial theories. Hall and Lindzey have justly stated: "All theories of behavior are pretty poor theories and all of them leave much to be desired in the way of scientific proof"—this being said in a textbook of nearly 600 pages on "Theories of Personality."

We can therefore not well expect that G.S.T. can present solutions where personality theorists from Freud and Jung to a host of modern writers were unable to do so. The theory will have shown its value if it opens new perspectives and viewpoints capable of experimental and practical application. This appears to be the case. There is quite a group of psychologists who are committed to an organismic theory of personality, Goldstein and Maslow being well-known representatives. Biological considerations may therefore be expected to advance the matter.

There is, of course, the fundamental question whether, first, G.S.T. is not essentially a physicalistic simile, inapplicable to psychic phenomena; and secondly whether such model has explanatory value when the pertinent variables cannot be defined quantitatively as is in general the case with psychological phenomena.

(1) The answer to the first question appears to be that the systems concept is abstract and general enough to permit application to entities of whatever denomination. The notions of "equilibrium," "homeostasis," "feedback," "stress," etc., are no less of technologic or physiologic origin but more or less successfully applied to psychological phenomena. System theorists agree that the concept of "system" is not limited to material entities but can be applied to any "whole" consisting of interacting "components." ... Systems engineering is an example where components are partly not physical and metric.

(2) If quantitation is impossible, and even if the components of a system are ill-defined, it can...
at least be expected that certain principles will qualitatively apply to the whole "qua" system. At least "explanation on principle" (see below) may be possible.

Bearing in mind these limitations, one concept which may prove to be of a key nature is the organismic notion of the organism as a spontaneously active system. In the present author's words, "Even under constant external conditions and in the absence of external stimuli the organism is not a passive but a basically active system. This applies in particular to the function of the nervous system and to behavior. It appears that internal activity rather than reaction to stimuli is fundamental. This can be shown with respect both to evolution in lower animals and to development, for example, in the first movements of embryos and fetuses.

This agrees with what von Holst has called the "new conception" of the nervous system, based upon the fact that primitive locomotor activities are caused by central automatisms that do not need external stimuli. Therefore, such movements persist, for example, even after the connection of motoric to sensory nerves had been severed. Hence the reflex in the classic sense is not the basic unit of behavior but rather a regulatory mechanism superimposed upon primitive, automatic activities. A similar concept is basic in the theory on instinct. According to Lorenz, innate releasing mechanisms (I.R.M.) play a dominant role, which sometimes go off without an external stimulus (in-vacuo or running idle reactions): A bird which has no material to build a nest may perform the movements of nest building in the air. These considerations are in the framework of what Hebb called the "conceptual C.N.S. of 1930-1950." The more recent insight into activating systems of the brain emphasizes differently, and with a wealth of experimental evidence, the same basic concept of the autonomous activity of the C.N.S.

The significance of these concepts becomes apparent when we consider that they are in fundamental contrast to the conventional stimulus-response scheme which assumes that the organism is an essentially reactive system answering, like an automaton, to external stimuli. The dominance of the S-R scheme in contemporary psychology needs no emphasis, and is obviously connected with the zealousist of a highly mechanized society. This principle is basic in psychological theories which, in all other respects are opposite, for example, in behavioristic psychology as well as in psychoanalysis. According to Freud it is the supreme tendency of the organism to get rid of tensions and drives and come to rest in a state of equilibrium governed by the "principle of stability" which Freud borrowed from the German philosopher, Fechner. Neurotic and psychotic behavior, then, is a more of less effective or abortive defense mechanism tending to restore some sort of equilibrium (according to D. Rapaport's analysis of the structure of psychoanalytic theory: "economic" and "adaptive points of view"). Charlotte Buhler, the well-known child psychologist, has aptly epitomized the theoretical situation:

In the fundamental psychoanalytic model, there is only one basic tendency, that is toward need gratification or tension reduction. . . . Present-day biologic theories emphasize the "spontaneity" of the organism's activity which is due to its built-in energy. The organism's autonomous functioning, its "drive to perform certain movements" is emphasized by Beralafany. . . . These concepts represent a complete revision of the original homeostasis principle which emphasized exclusively the tendency toward equilibrium. It is the original homeostasis principle with which psychoanalysis identified its theory of discharge of tensions as the only primary tendency (Emphasis partly ours).

In brief, we may define our viewpoint as "Beyond the Homeostasis Principle":

1. The S-R scheme misses the realms of play, exploratory activities, creativity, self-realization, etc.

2. The economic scheme misses just specific, human achievements—the most of what loosely is termed "human culture"

3. The equilibrium principle misses the fact that psychological and behavioral activities are more than relaxation of tensions; far from establishing an optimal state, the latter may entail psychosis-like disturbances as, e.g., in sensory-deprivation experiments.

It appears that the S-R and psychoanalytic model is a highly unrealistic picture of human nature and, in its consequences, a rather dangerous one. Just what we consider to be specific human achievements can hardly be brought under the utilitarian, homeostasis, and stimulus-response scheme. One may call mountain climbing, composing of sonatas or lyrical poems "psychological homeostasis"—as has been done—but at the risk that this physiologically well-defined concept loses all meaning. Furthermore, if the principle of homeostatic maintenance is taken as a golden rule of behavior, the so-called well-adjusted individual will be the ultimate goal, that is a well-oiled robot maintaining itself in optimal biological, psychological and social homeostasis. This is a Brave
New World—not, for some at least, the ideal state of humanity. Furthermore, that precarious mental equilibrium must not be disturbed. Hence in what somewhat ironically is called progressive education, the anxiety not to overload the child, not to impose constraints and to minimize all directing influences—with the result of a previously unheard-of crop of illiterates and juvenile delinquents.

In contrast to conventional theory, it can safely be maintained that not only stresses and tensions but equally complete release from stimuli and the consequent mental void may be neurogenic or even psychogenic. Experimentally this is verified by the experiments with sensory deprivation when subjects, insulated from all incoming stimuli, after a few hours develop a so-called model psychosis with hallucinations, unbearable anxiety, etc. Clinically it amounts to the same when insulation leads to prisoners' psychosis and to exacerbation of mental disease by isolation of patients in the ward. In contrast, maximal stress need not necessarily produce mental disturbance. If conventional theory were correct, Europe during and after the war, with extreme physiological as well as psychological stresses, should have been a gigantic lunatic asylum. As a matter of fact, there was statistically no increase either in neurotic or psychotic disturbances, apart from easily explained acute disturbances such as combat neurosis.

We so arrive at the conception that a great deal of biological and human behavior is beyond the principles of utility, homeostasis and stimulus-response, and that it is just this which is characteristic of human and cultural activities. Such new look opens new perspectives not only in theory, but in practical implications with respect to mental hygiene, education, and society in general.

What has been said can also be couched in philosophical terms. If existentialists speak of the emptiness and meaninglesslness of life, if they see it in a source not only of anxiety but of actual mental illness, it is essentially the same viewpoint: that behavior is not merely a matter of satisfaction of biological drives and of maintenance in psychological and social equilibrium but that something more is involved. If life becomes unbearably empty in an industrialized society, what can a person do but develop a neurosis? The principle which may loosely be called spontaneous activity of the psychophysical organism, is a more realistic formulation of what the existentialists want to say in their often obscure language. And if personality theorists like Maslow or Gardner Murphy speak of self-realization as human goal, it is again a somewhat pompous expression of the same.

Theoretical History

We eventually come to those highest and ill-defined entities that are called human cultures and civilizations. It is the field often called "philosophy of history." We may perhaps better speak of "theoretical history," admittedly in its very first beginnings. This name expresses the goal to form a connecting link between "science" and the "humanities"; more in particular, between the "social sciences" and "history."

It is understood, of course, that the techniques in sociology and history are entirely different (polls, statistical analysis against archival studies, internal evidence of historic relics, etc.). However, the object of study is essentially the same. Sociology is essentially concerned with a temporal cross-section as human societies are; history with the "longitudinal" study how societies become and develop. The object and techniques of study certainly justify practical differentiation; it is less clear, however, that they justify fundamentally different philosophies.

The last statement already implies the question of constructs in history, as they were presented, in grand form, from Vico to Hegel, Marx, Spengler, and Toynbee. Professional historians regard them at best as poetry, at worst as fantasies pressing, with paranoid obsession, the facts of history into a theoretical bed of Procrustes. It seems history can learn from the system theorists, not ultimate solutions but a sounder methodological outlook. Problems hitherto considered to be philosophical or metaphysical can well be defined in their scientific meaning, with some interesting outlook at recent developments (e.g., game theory) thrown into the bargain.

Empirical criticism is outside the scope of the present study. For example, Geyl and many others have analyzed obvious misrepresentations of historical events in Toynbee's work, and even the non-specialist reader can easily draw a list of fallacies especially in the later, Holy-Ghost inspired volumes of Toynbee's _magnus opus_. The problem, however, is larger than errors in fact or interpretation or even the question of the merits of Marx's, Spengler's or Toynbee's theories; it is whether, in principle, models and laws are admissible in history.

A widely held contention says that they are not. This is the concept of "nomothetic method
in science and "idiographic" method in history. While science to a greater or less extent can establish "laws" for natural events, history, concerned with human events of enormous complexity in causes and outcome and possibly determined by free decisions of individuals can only describe, more or less satisfactorily, what has happened in the past.

Here the methodologist has his first comment. In the attitude just outlined, academic history condemns constructs of history as "intuitive," "contrary to fact," "arbitrary," etc. And, no doubt, the criticism is pungent enough vis-à-vis Spengler or Toynbee. It is, however, somewhat less convincing if we look at the work of conventional historiography. For example, the Dutch historian, Peter Geyl, who made a strong argument against Toynbee from such methodological considerations, also wrote a brilliant book about Napoleon, amounting to the result that there are a dozen or so different interpretations—we may safely say, models—of Napoleon's character and career within academic history, all based upon "fact" (the Napoleonic period happens to be one of the best documented) and all flatly contradicting each other. Roughly speaking, they range from Napoleon as the brutal tyrant and egotistic enemy of human freedom to Napoleon the wise planner of a unified Europe; and if one is a Napoleonic student (as the present writer happens to be in a small way), one can easily produce some original documents refuting misconceptions occurring even in generally accepted, standard histories. You cannot have it both ways. If even a figure like Napoleon, not very remote in time and with the best of historical documentation, can be interpreted contrarily, you cannot well blame the "philosophers of history" for their intuitive procedure, subjective bias, etc., when they deal with the enormous phenomenon of universal history. What you have in both cases is a conceptual model which always will represent certain aspects only, and for this reason will be one-sided or even lopsided. Hence the construction of conceptual models in history is not only permissible but, as a matter of fact, is at the basis of any historical interpretation as distinguished from mere enumeration of data, i.e., chronicle or annals.

If this is granted, the antithesis between idio-
graphic and nomothetic procedure reduces to what psychologists are wont to call the "molecular" and "molar" approach. One can analyze events within a complex whole—individual chemical reactions in an organism, perceptions in the psyche, for example; or one can look for over-all laws covering the whole such as growth and development in the first or personality in the second instance. In terms of history, this means detailed study of individuals, treaties, works of art, singular causes and effects, etc., or else over-all phenomena with the hope of detecting grand laws. There are, of course, all transitions between the first and second considerations; the extremes may be illustrated by Carlyle and his hero worship at one pole and Tolstoy (a far greater "theoretical historian" than commonly admitted) at the other.

The question of "a theoretical history" therefore is essentially that of "molar" models in the field; and this is what the great constructs of history amount to when divested of their philosophical embroidery.

The evaluation of such models must follow the general rules for verification or falsification. First, there is the consideration of empirical bases. In this particular instance it amounts to the question whether or not a limited number of civilizations—some twenty at the best—provide a sufficient and representative sample to establish justified generalizations. This question and that of the value of proposed models will be answered by the general criterion: whether or not the model has explanatory and predictive value, i.e., throws new light upon known facts and correctly foretells facts of the past or future not previously known.

Although elementary, these considerations nevertheless are apt to remove much misunderstanding and philosophical fog which has clouded the issue.

1) As had been emphasized, the evaluation of models should be simply pragmatic in terms of their explanatory and predictive merits (or lack thereof); a priori considerations as to their desirability or moral consequences do not enter.

Here we encounter a somewhat unique situation. There is little objection against so-called "synchronic" laws, i.e., supposed regularities governing societies at a certain point in time; as a matter of fact, beside empirical study this is the aim of sociology. Also certain "diachronic" laws, i.e., regularities of development in time, are undisputed such as, e.g., Grimm's law stating rules for the changes of consonants in the evolution of Indo-Germanic languages. It is commonplace that there is a sort of "life-cycle"—stages of primitivity, maturity, baroque dissolution of form and eventual decay for which no particular external causes can be indicated—in individual fields of culture, such as Greek sculpture, Renaissance painting or German music. Indeed, this even has its counterpart in certain phenomena of biological evolution.
showing, as in ammonites or dinosaurs, a first explosive phase of formation of new types, followed by a phase of speciation and eventually of decadence. Violent criticism comes in when this model is applied to civilization as a whole. It is a legitimate question—Why often rather unrealistic models in the social sciences remain matters of academic discussion, while models of history encounter passionate resistance? Granting all factual criticism raised against Spengler or Toynbee, it seems rather obvious that emotional factors are involved. The highway of science is strewn with corpses of deceased theories which just decay or are preserved as mummies in the museum of history or science. In contrast, historical constructs and especially theories of historical cycles appear to touch a raw nerve, and so opposition is much more than usual criticism of a scientific theory.

(2) This emotional involvement is connected with the question of "Historical Inevitability" and a supposed degradation of human "freedom." Before turning to it, discussion of mathematical and non-mathematical models is in place.

Advantages and shortcomings of mathematical models in the social sciences are well known. Every mathematical model is an oversimplification, and it remains questionable whether it strips actual events to the bones or cuts away vital parts of their anatomy. On the other hand, so far as it goes, it permits necessary deduction with often unexpected results which would not be obtained by ordinary "common sense."

In particular, Rashevsky has shown in several studies how mathematical models of historical processes can be constructed.

On the other hand, the value of purely qualitative models should not be underestimated. For example, the concept of "ecologic equilibrium" was developed long before Volterra and others introduced mathematical models; the theory of selection belongs to the stock-in-trade of biology, but the mathematical theory of the "struggle for existence" is comparatively recent, and far from being verified under wildlife conditions.

In complex phenomena, "explanation on principle" by qualitative models is preferable to no explanation at all. This is by no means limited to the social sciences and history; it applies alike to fields like meteorology or evolution.

(3) "Historical inevitability"—subject of a well-known study by Sir Isaiah Berlin—dreaded as a consequence of "theoretical history," supposedly contradicting our direct experience of having free choices and eliminating all moral judgment and values—is a phantasmagoria based upon a world view which does not exist any more. As, in fact, Berlin emphasizes, it is founded upon the concept of the Laplacean spirit who is able completely to predict the future from the past by means of deterministic laws. This has no resemblance with the modern concept of "laws of nature." All "laws of nature" have a statistical character. They do not predict an inexorably determined future but probabilities which, depending on the nature of events and on the laws available, may approach certainty or else remain far below it. It is nonsensical to ask or fear more "inevitability" in historical theory than is found in sciences with relatively high sophistication like meteorology or economics.

Paradoxically, while the cause of free will rests with the testimony of intuition or rather immediate experience and can never be proved objectively ("Was it Napoleon's free will that led him to the Russian Campaign?"); determinism (in the statistical sense) can be proved, at least in small-scale models. Certainly business depends on personal "initiative," the individual "decision" and "responsibility" of the entrepreneur; the manager's choice whether or not to expand business by employing new appointees, is "free" in precisely the sense as Napoleon's choice whether or not to accept battle at Austerlitz. However, when the growth curve of industrial companies is analyzed, it is found that "arbitrary" deviations are followed by speedy return to the normal curve, as if invisible forces were active. Haire states that "the return to the pattern predicted by earlier growth suggests the operation of inexorable forces operating on the social organism" (our emphasis).

It is characteristic that one of Berlin's points is "the fallacy of historical determinism (appearing) from its utter inconsistency with the common sense and everyday life of looking at human affairs." This characteristic argument is of the same nature as the advice not to adopt the Copernican system because everybody can see that the sun moves from morning to evening.

(4) Recent developments in mathematics even allow to submit "free will"—apparently the philosophical problem most recalcitrant against scientific analysis—to mathematical examination.

In the light of modern systems theory, the alternative between molar and molecular, nomothetic and idiographic approach can be given a precise meaning. For mass behavior, system laws would apply which, if they can be mathematized, would take the form of differential equations of
the sort of those used by Richardson mentioned above. Free choice of the individual would be described by formulations of the nature of game and decision theory.

Axiomatically, game and decision theory are concerned with "rational" choice. This means a choice which "maximizes the individual's utility or satisfaction," that "the individual is free to choose among several possible courses of action and decides among them at the basis of their consequences," that he "selects, being informed of all conceivable consequences of his actions, what stands highest on his list," he "prefers more of a commodity to less, other things being equal," etc. Instead of economical gain, any higher value may be inserted without changing the mathematical formalism.

The above definition of "rational choice" includes everything that can be meant by "free will." If we do not wish to equate "free will" with complete arbitrariness, lack of any value judgment and therefore completely consequential actions (like the philosopher's favorite example: It is my free will whether or not to wiggle my left little finger) it is a fair definition of those actions with which the moralist, priest or historian is concerned: free decision between alternatives based upon insight into the situation and its consequences and guided by values.

The difficulty to apply the theory even to simple, actual situations is of course enormous; so is the difficulty in establishing over-all laws. However, without explicit formulation, both approaches can be evaluated in principle—leading to an unexpected paradox.

The "principle of rationality" fits—no the majority of human actions but rather the "unreasoning" behavior of animals. Animals and organisms in general do function in a "ratio-morphic" way, maximizing such values as maintenance, satisfaction, survival, etc.; they select, in general, what is biologically good for them, and prefer more of a commodity (e.g., food) to less.

Human behavior, on the other hand, falls far short of the principle of rationality. It is not even necessary to quote Freud to show how small is the compass of rational behavior in man. Women in a supermarket, in general, do not maximize utility but are susceptible to the tricks of the advertiser and packer; they do not make a rational choice surveying all possibilities and consequences; and do not even prefer more of the commodity packed in an inconsiderate way to less when packed in a big red box with attractive design. In our society, it is the job of an influential specialty—advertisers, motivation researchers, etc.—to make choices irrational which essentially is done by coupling biological factors—conditioned reflex, unconscious drives—with symbolic values.

And there is no refuge by saying that this irrationality of human behavior concerns only trivial actions of daily life; the same principle applies to "historical" decisions. That wise old man, Oxenstierna, Sweden's Chancellor during the Thirty Years' War, has perfectly expressed this by saying: Nescia, mi fili, quam illa ratione mundus regulatur—you don't know, my dear boy, with what little reason the world is governed. Reading newspapers or listening to the radio readily shows that this applies perhaps even more to the 20th than the 17th century.

Methodologically, this leads to a remarkable conclusion. If one of the two models is to be applied, and if the "actuality principle" basic in historical fields like geology and evolution is adopted (i.e., the hypothesis that no other principles of explanation should be used than can be observed as operative in the present)—then it is the statistical or mass model which is backed by empirical evidence. The business of the motivation and opinion researcher, statistical psychologist, etc., is based upon the premise that statistical laws obtain in human behavior; and that, for this reason a small but well-chosen sample allows for extrapolation to the total population under consideration. The generally good working of a Gallup poll and prediction verifies the premise—with some incidental failure like the well-known example of the Truman election thrown in, as is to be expected with statistical predictions. The opposite contention—that history is governed by "free will" in the philosophical sense (i.e., rational decision for the better, the higher moral value or even enlightened self-interest) is hardly supported by fact. That here and there the statistical law is broken by "rugged individualists" is in its character. Nor does the role played in history by "great men" contradict the systems concept in history; they can be conceived as acting like "leading parts," "triggers" or "catalyzers" in the historical process—a phenomenon well accounted for in the general theory of systems.

(5) A further question is the "organismic analogy" unanimously condemned by historians. They combat untriringly the "metaphysical," "poetical," "mythical" and thoroughly unscientific nature of Spencer's assertion that civilizations are a sort of "organisms," being born, developing
according to their internal laws and eventually dying. Toynbee takes great pains to emphasize that he did not fall into Spengler’s trap—even though it is somewhat difficult to see that his civilizations, connected by the biological relations of “affiliation” and “apparition,” even according to the latest version of his system) with a rather strict time span of development, are not conceived organismically.

Nobody should know better than the biologist that civilizations are no “organism.” It is trivial to the extreme that a biological organism, a material entity and unity in space and time, is something different from a social group consisting of distinct individuals, and even more from a civilization consisting of generations of human beings, of material products, institutions, ideas, values, and what not. It implies a serious underestimate of Vico’s, Spengler’s (or any normal individual’s) intelligence to suppose that they did not realize the obvious.

Nevertheless, it is interesting to note that, in contrast to the historians’ scruples, sociologists do not abhor the “organismic analogy” but rather take it for granted. For example, in the words of Rapoport and Horvath:

There is some sense in considering a real organization as an organism, that is, there is reason to believe that this comparison need not be a sterile metaphorical analogy, such as was common in scholastic speculation about the body politic. Quasi-biological functions are demonstrable in organizations. They maintain themselves; they sometimes reproduce or metastasize; they respond to stresses; they age, and they die. Organizations have discernible anatomies and those at least which transform material inputs (like industries) have physiologists.

Or Sir Geoffrey Vickers:

Institutions grow, repair themselves, reproduce themselves, decay, dissolve. In their external relations they show many characteristics of organic life. Some think that in their internal relations also human institutions are destined to become increasingly organic, that human cooperation will approach ever more closely to the integration of cells in a body. I find this prospect unconvincing (and unpleasant. (N.B. so does the present author.)

And Haire:

The biological model for social organizations—and here, particularly for industrial organizations—means taking as a model the living organism and the processes and principles that regulate its growth and development. It means looking for lawful processes in organizational growth.

The fact that simple growth laws apply to social entities such as manufacturing companies, to urbanization, division of labor, etc., proves that in these respects the “organismic analogy” is correct. In spite of the historians’ protests, the application of theoretical models, in particular, the model of dynamic, open and adaptive systems to the historical process certainly makes sense. This does not imply “biologism,” i.e., reduction of social to biological concepts, but indicates system principles applying in both fields.

(6) Taking all objections for granted—poor method, errors in fact, the enormous complexity of the historical process—we have nevertheless reluctantly to admit that the cyclic models of history pass the most important test of scientific theory. The predictions made by Spengler in the Decline of the West, by Toynbee when forecasting a time of trouble and contending states, by Orwell in the Uprising of the Masses—well add Brave New World and 1984—have been verified to a disquieting extent and considerably better than many respectable models of the social scientists.

Does this imply “historic inevitability” and inexorable dissolution? Again, the simple answer was masked by moralizing and philosophizing historians. By extrapolation from the life cycles of previous civilizations nobody could have predicted the Industrial Revolution, the Population Explosion, the development of atomic energy, the emergence of underdeveloped nations, and the expansion of Western civilization over the whole globe. Does this refute the alleged model and “law” of history? No—it only says that this model—as every one in science—mirrors only certain aspects or facets of reality. Every model becomes dangerous only when it commits the “Nothing-but” fallacy which mars not only theoretical history, but the models of the mechanistic world picture, of psychoanalysis and many others as well.

We have hoped to show in this survey that General System Theory has contributed toward the expansion of scientific theory; has led to new insights and principles; and has opened up new problems that are “researchable,” i.e., are amenable to further study, experimental or mathematical. The limitations of the theory and its applications in their present status are obvious; but the principles appear to be essentially sound as shown by their application in different fields.