FACETS OF SYSTEMS SCIENCE
SECOND EDITION

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Binghamton, New York

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CHAPTER 1

What Is Systems Science?

_The aim of science is not things themselves, . . . but the relations between things; outside those relations there is no reality knowable._

—HENRI POINCARÉ

An inevitable prerequisite for this book, as implied by its title, is a presupposition that systems science is a legitimate field of scientific inquiry. It is self-evident that I, as the author of this book, consider this presupposition valid. Otherwise, clearly, I would not conceive of writing the book in the first place.

I must admit at the outset that my affirmative view regarding the legitimacy of systems science is not fully shared by everyone within the scientific community. It seems, however, that this view of legitimacy is slowly but steadily becoming predominant. It is my hope that this book, whose purpose is to characterize the essence and spirit of systems science, will have a positive influence in this regard.

**What is systems science?** This question, which I have been asked on countless occasions, can basically be answered either in terms of activities associated with systems science or in terms of the domain of its inquiry. The most natural answers to the question are, almost inevitably, the following definitions:

1. Systems science is what systems scientists do when they claim they do science.
2. Systems science is that field of scientific inquiry whose objects of study are systems.

Without further explanation, these definitions are clearly of little use.

Definition (1) is meaningful but somewhat impractical. It is meaningful since systems scientists do, indeed, exist. I, for example, claim to be one of them, and so do colleagues at my department and other departments of systems science. Hence, the meaning of systems science could, in principle, be determined by observing and analyzing our scientific activities. This strategy, however, involves some inherent practical difficulties. First, systems scientists are still a rather rare species among all scientists and, consequently, they are relatively hard to find. Second, scientific activities of scientists who are officially labeled as systems scientists vary from person to person, and, moreover, some of these activities are clearly associated with
other, well-established areas of science. Third, the strategy would require a massive data collection and extensive and sophisticated data analysis.

For all the reasons mentioned, and possibly some additional ones, it is virtually impossible to utilize definition (1) in an operational way for our purpose. Therefore, let me concentrate on definition (2). To be made operational, this definition requires that some broad and generally acceptable characterization of the concept of a system be established.

The term "system" is unquestionably one of the most widely used terms not only in science, but in other areas of human endeavor as well. It is a highly overworked term, which enjoys different meanings under different circumstances and for different people. However, when separated from its specific connotations and uses, the term "system" is almost never explicitly defined. To elaborate on this point, let me quote from a highly relevant paper by Rosen [1986]:

Let us begin by observing that the word "system" is almost never used by itself; it is generally accompanied by an adjective or other modifier: physical system; biological system; social system; economic system; axiomatic system; religious system; and even "general" system. This usage suggests that, when confronted by a system of any kind, certain of its properties are subsumed under the adjective, and other properties are subsumed under the "system," while still others may depend essentially on both. The adjective describes what is special or particular; i.e., it refers to the specific "thinghood" of the system; the "system" describes those properties which are independent of this specific "thinghood."

This observation immediately suggests a close parallel between the concept of system and the development of the mathematical concept of a set. Given any specific aggregate of things; e.g., five oranges, three sticks, five fingers, there are some properties of the aggregate which depend on the specific nature of the things of which the aggregate is composed. There are others which are totally independent of this and depend only on the "setness" of the aggregate. The most prominent of these is what we call the cardinality of the aggregate.

It should now be clear that systemhood is related to thinghood in much the same way as setness is related to thinghood. Likewise, what we generally call system properties are related to systemhood in the same way as cardinality is related to setness. But systemhood is different from both setness and from thinghood: it is an independent category.

To begin our search for a meaningful definition of the term "system" from a broad perspective, let us consult a standard dictionary. We are likely to find that a system is "a set or arrangement of things so related or connected as to form a unity or organic whole" (Webster's New World Dictionary), although different dictionaries may contain stylistic variations of this particular formulation. It follows from this common-sense definition that the term "system" stands, in general, for a set of some things and a relation among the things. Formally, we have
What Is Systems Science?

\[ S = (T, R), \]

(1.1)

where \( S, T, R \) denote, respectively, a system, a set of things distinguished within \( S \), and a relation (or, possibly, a set of relations) defined on \( T \). Clearly, the thinghood and systemhood properties of \( S \) reside in \( T \) and \( R \), respectively.

The common-sense definition of a system, expressed by Eq. (1.1), is rather primitive. This, paradoxically, is its weakness as well as its strength. The definition is weak because it is too general and, consequently, of little pragmatic value. It is strong because it encompasses all other, more specific definitions of systems. In this regard, this most general definition of systems provides us with a criterion by which we can determine whether any given object is a system or not: an object is a system if and only if it can be described in a form that conforms to Eq. (1.1).

For example, a collection of books is not a system, only a set. However, when we organize the books in some way, the collection becomes a system. When we order them, for instance, by authors' names, we obtain a system since any ordering of a set is a relation defined on the set. We may, of course, order the books in various other ways (by publication dates, by their size, etc.), which result in different systems. We may also partition the books by various criteria (subjects, publishers, languages, etc.) and obtain thus additional systems since every partition of a set emerges from a particular equivalence relation defined on the set. Observe now that a relation defined on a particular set of books, say the ordering by publication dates, may be applied not only to other sets of books, but also to sets whose elements are not books. For example, members of a human population may be ordered by their dates of birth.

These simple examples illustrate that the same set may play a role in different systems; these systems are distinguished from each other by different relations on the set. Similarly, the same relation, when applied to different sets, may play a role in different systems. In this case, the systems are distinguished by their sets or, in other words, by their thinghood properties.

Once we have the capability of distinguishing objects that are systems from those that are not, the proposed definition of systems science—"a science whose objects of study are systems"—becomes operational. Observe, however, that the term "system" is used in this definition without any adjective or other modifier. This indicates, according to the distinction between thinghood and systemhood, that systems science focuses on the study of systemhood properties of systems rather than their thinghood properties. Taking this essential aspect of systems science into consideration, the following, more specific definition of systems science emerges:

"Systems science is a science whose domain of inquiry consists of those properties of systems and associated problems that emanate from the general notion of systemhood."

The principal purpose of this book is to elaborate on this conception of systems science. It is argued throughout the book that systems science, like any other
science, contains a *body of knowledge* regarding its domain, a *methodology* for acquisition of new knowledge and for dealing with relevant problems within the domain, and a *metamethodology*, by which methods and their relationship to problems are characterized and critically examined. However, in spite of these parallels with classical areas of science, systems science is fundamentally different from science in the traditional sense. The difference can best be explained in terms of the notions of thinghood and systemhood.

It is a truism that classical science has been far more concerned with thinghood than systemhood. In fact, the many disciplines and specializations that have evolved in science during the last five centuries or so reflect predominantly the differences between the things studied rather than the differences in their ways of being organized. This evolution is still ongoing. Since at least the beginning of the 20th century, however, it has increasingly been recognized that studying the ways in which things can be, or can become, organized is equally meaningful and may, under some circumstances, be even more significant than studying the things themselves. From this recognition, a new kind of science eventually emerged, a science that is predominantly concerned with systemhood rather than thinghood. This new science is, of course, systems science.

Since disciplines of classical science are largely thinghood-oriented, the systemhood orientation of systems science does not make it a new discipline of classical science. With its orientation so fundamentally different from the orientation of classical science, systems science transcends all the disciplinary boundaries of classical science. From the standpoint of systems science, these boundaries are totally irrelevant, superficial, and even counterproductive. Yet, they are significant in classical science, where they reflect fundamental differences, for example, differences in measuring instruments and techniques. In other words, the disciplinary boundaries of classical science are thinghood-dependent but systemhood-independent. If systems science becomes divided into special disciplines in the future, the boundaries between these disciplines will inevitably be systemhood-dependent but thinghood-independent.

Classical science, with all its disciplines, and systems science, with all its prospective disciplines, thus provide us with two distinct perspectives from which scientific inquiry can be approached. These perspectives are complementary. Either of them can be employed without the other only to some extent. In most problems of scientific inquiry, the two perspectives must be applied in concert.

It may be argued that traditional scientific inquiries are almost never totally devoid of issues involving systemhood. This is true, but these issues are handled in classical science in an opportunistic, ad hoc fashion. There is no place in classical science for a comprehensive and thorough study of the various properties of systemhood. The systems perspective is thus suppressed within the confines of classical science in the sense that it cannot develop its full potential. It was liberated only through the emergence of systems science. While the systems perspective was
not essential when science dealt with simple systems, its significance increases with the growing complexity of systems of our current interest and challenge.

From the standpoint of the disciplinary classification of classical science, systems science is clearly cross-disciplinary. There are at least three important implications of this fact. First, systems science knowledge and methodology are directly applicable in virtually all disciplines of classical science. Second, systems science has the flexibility to study systemhood properties of systems and the associated problems that include aspects derived from any number of different disciplines and specializations of classical science. Such multidisciplinary systems and problems can thus be studied as wholes rather than collections of the disciplinary subsystems and subproblems. Third, the cross-disciplinary orientation of systems science has a unifying influence on classical science, increasingly fractured into countless number of narrow specializations, by offering unifying principles that transcend its self-imposed boundaries.

Classical science and systems science may be viewed as complementary dimensions of modern science. As is argued later (Sec. 3.4), the emergence and evolution of systems science and its integration with classical science into genuine two-dimensional science are perhaps the most significant features of science in the information (or postindustrial) society.
CHAPTER 2

More about Systems

What is a system? As any poet knows, a system is a way of looking at the world.
—GERALD M. WEINBERG

2.1. Common-Sense Definition

The common-sense definition, as expressed by Eq. (1.1), looks overly simple:

\[
S = (T, R)
\]

- a relation defined on \( T \) (systemhood)
- a set of certain things (thinghood)

a system

Its simplicity, however, is only on the surface. That is, the definition is simple in its form, but it contains symbols, \( T \) and \( R \), that are extremely rich in content. Indeed, \( T \) stands for any imaginable set of things of any kind, and \( R \) stands for any conceivable relation defined on \( T \). To appreciate the range of possible meanings of these symbols, let us explore some examples.

Symbol \( T \) may stand for a single set with arbitrary elements, finite or infinite, but can also represent, for example, a power set (the set of all subsets of another set), any subset of the power set, or an arbitrary family of distinct sets. The content of symbol \( R \) is even richer. For each set \( T \), with its special characteristics, the symbol stands for every relation that can be defined on the set.

To introduce the concept of a relation, we need first to introduce the underlying concept of a Cartesian product of sets. To do that, let us assume that the symbol \( T \) in Eq. (1.1) stands for the family of sets \( A_1, A_2, \ldots, A_n \). That is,

\[
T = \{A_1, A_2, \ldots, A_n\}
\]

In this case, the Cartesian product of sets in this family, which is usually denoted by the symbol

\[
A_1 \times A_2 \times \ldots \times A_n
\]
is the set of all possible ordered \( n \)-tuples formed by selecting the first component from set \( A_1 \), the second component from \( A_2 \), etc., and the last component from set \( A_n \). Formally,

\[
A_1 \times A_2 \times \ldots \times A_n = \{ (a_1, a_2, \ldots, a_n) | a_1 \in A_1, a_2 \in A_2, \ldots, a_n \in A_n \}.
\]

When \( n \) sets are involved in a Cartesian product, as in this case, we refer to it as an \( n \)-dimensional Cartesian product. For convenience, we often write

\[
\prod_{i=1}^{n} A_i
\]

instead of \( A_1 \times A_2 \times \ldots \times A_n \). Moreover, when \( A_i = A \) for all \( i = 1, 2, \ldots, n \), we often denote the Cartesian product by the symbol \( A^n \).

As an example, let \( T = (A_x, A_y) \), where \( A_x \) and \( A_y \) are given closed intervals of real numbers on Cartesian coordinates \( x \) and \( y \), respectively, in the two-dimensional Euclidean space \((xy\)-plane\). When, for example, \( A_x = [1, 4] \) and \( A_y = [1, 3] \), the Cartesian product \( A_x \times A_y \) consists of all points in the rectangle shown in Fig. 2.1.

Cartesian products provide a basis for defining relations. A relation, in general, is a subset of some Cartesian product of given sets. This means that many distinct relations can be defined on the same Cartesian product. Each Cartesian product characterizes a unique form of relations based on it. The number of distinct

![Figure 2.1. Geometric representation of Cartesian product.](image-url)
More about Systems

relational forms, each based upon a particular Cartesian product, increases with the increasing number of distinct sets subsumed under the symbol \( T \) in Eq. (1.1). To illustrate the tremendous variety of relational forms, let us consider a few examples.

When \( T \) stands for a single set, say set \( A \), the obvious relational forms are:

\[
R \subseteq A^2(= A \times A),
\]

\[
R \subseteq A^3(= A \times A \times A),
\]

\[
R \subseteq A^n(= \underbrace{A \times A \times \ldots \times A}_{n \text{-times}}).
\]

Relations of these forms are called, respectively, binary, ternary, \ldots, \( n \)-ary relations on \( A \). Examples of additional forms are:

\[
R \subseteq (A \times A) \times A,
\]

\[
R \subseteq A \times (A \times A),
\]

\[
R \subseteq (A \times A) \times (A \times A).
\]

All these forms define binary relations in which one or two sets, as designated in parentheses, are Cartesian products of \((A \times A)\). Similarly, the forms

\[
R \subseteq (A \times A) \times (A \times A) \times (A \times A)
\]

\[
R \subseteq (A \times A \times A) \times (A \times A \times A)
\]

define, respectively, ternary relations on \( A \times A \) and binary relations on \( A \times A \times A \).

When \( T \) consists of a family of two sets, \( T = \{A, B\} \), the number of possible relational forms further increases. A few examples are:

\[
R \subseteq A \times B,
\]

\[
R \subseteq (A \times A) \times B,
\]

\[
R \subseteq (A \times B) \times (A \times B),
\]

\[
R \subseteq (A \times A \times A) \times B,
\]

\[
R \subseteq (A \times A \times A) \times (B \times B),
\]

\[
R \subseteq (A \times B) \times (A \times B) \times (A \times B).
\]
It is now easy to see, I trust, how rapidly the number of possible relational forms increases with the increasing number of distinct sets in $T$, illustrating thus the tremendous richness of the systemhood symbol $R$. The fact that we discuss the meaning of this symbol solely in terms of mathematical relations is no shortcoming. The well-defined concept of a mathematical relation (as a subset of some Cartesian product) is sufficiently general to encompass the whole set of kindred concepts that pertain to systemhood, such as interaction, interconnection, coupling, linkage, cohesion, constraint, interdependence, function, organization, structure, association, correlation, pattern, etc.

Let us address now the issue of calculating the number of possible relations for each given relational form. First, it is easy to see that the number depends on both the Cartesian product and the sets employed in it. If the sets are infinite, as in the example illustrated in Fig. 2.1, the number of possible relations is also infinite (any subset of points in the rectangle in Fig. 2.1 is a relation). If the sets are finite, the number of possible relations is determined by the number of elements in each of the sets employed in the given Cartesian product. The number of elements in a finite set $A$ is usually denoted by the symbol $|A|$ and it is called the cardinality of $A$. It is easy to see that the cardinality of a Cartesian product of finite sets is the arithmetic product of the cardinalities of the sets employed in it. For example,

$$|A \times B| = |A| \cdot |B|,$$

$$|A_1 \times A_2 \times \cdots \times A_n| = |A_1| \cdot |A_2| \cdots |A_n|,$$

$$|A|^2 = |A|^2, |A^3| = |A|^3, \text{ etc.}$$

If some of the sets in the Cartesian product are themselves Cartesian products, the calculation remains the same. For example,

$$|(A \times B) \times (C \times D)| = |A \times B| \cdot |C \times D| = |A| \cdot |B| \cdot |C| \cdot |D|.$$

The set of all relations that can be defined on a given Cartesian product of finite sets is the power set (the set of all subsets) of the Cartesian product. To define a particular relation, two choices are available for each element ($n$-tuple) of the Cartesian product: the element is either included or not included in the relation. Let $|C|$ denote the cardinality of the given Cartesian product $C$. Then, the total number of choices and, therefore, the total number of relations on $C$, $\#R(C)$, is obtained by the arithmetic product

$$\#R(C) = 2 \cdot 2 \cdots 2 = 2^{\mid C\mid},$$

$|C|$-times
More about Systems

Thus, for example,

$$|R(A \times B)| = 2^{|A||B|},$$

$$|R(A^p)| = 2^{|A|^p}.$$

When some sets in the Cartesian product are power sets of some other finite sets, their cardinalities must be properly calculated. Denoting the power set of $A$ by $\mathcal{P}(A)$, we can apply the preceding argument to derive the formula

$$|\mathcal{P}(A)| = 2^{|A|}.$$

Similarly, for the power set of the power set of $A$, $\mathcal{P}(\mathcal{P}(A))$, we have

$$|\mathcal{P}(\mathcal{P}(A))| = 2^{2^{|A|}}.$$

Thus, for example,

$$|A \times \mathcal{P}(B)| = 2^{|A|} \cdot 2^{|B|} = 2^{|A|+|B|},$$

$$|A \times \mathcal{P}(B) \times \mathcal{P}(\mathcal{P}(C)) \times D| = |A| \cdot 2^{|B|} \cdot 2^{|C|} \cdot |D|$$

$$= |A| \cdot 2^{|B|+2^{|C|} \cdot |D|}.$$

2.2. More about Relations

To appreciate the richness of the concept of a relation, let us examine in more detail the simplest possible relations. These are binary relations of the form

$$R \subseteq T \times T$$

where $T$ is a single set of things. In any relation of this form, things in $T$ are related to themselves according to some given criterion $c$. Let $R_c$ denote a relation based on criterion $c$. Then $(x, y) \in R_c$ if and only if thing $x$ is related to thing $y$ according to the given criterion $c$. Let us illustrate this general definition by the following examples:

- $T$ = a set of people.
  
  Person $x$ is related to person $y$ iff $x$ is equal to $y$ in terms of a given characteristic (age, income, education, occupation, sex, citizenship, height, weight, name, employer, job performance, etc.). Relations of this kind are called equivalence relations.
• $T$ is a set of English words.
  A word $x$ is related to word $y$ iff $x$ is a synonym of $y$. Relations of this kind are called compatibility relations.

• $T$ is a set of countries.
  Country $x$ is related to country $y$ iff it is smaller than or equal to it in terms of both the geographic area and the number of inhabitants. Relations of this kind are called partial orderings.

• $T$ is a set of numbers.
  Number $x$ is related to number $y$ iff $x$ is smaller than $y$. Relations of this kind are called strict orderings (also called linear, or total orderings).

The most fundamental classification of relations $R \subseteq T \times T$ (into equivalence relations, various ordering relations, etc.) is based on the following properties:

• $R$ is reflexive iff $(x, x) \in R$ for each $x \in T$.
• $R$ is antireflexive iff $(x, x) \notin R$ for each $x \in T$.
• $R$ is symmetric iff, for every $x$ and $y$ in $T$, whenever $(x, y) \in R$, then also $(y, x) \in R$.
• $R$ is antisymmetric if and only if, for every $x$ and $y$ in $T$, whenever $(x, y) \in R$ and $(y, x) \in R$, then $x = y$.
• $R$ is transitive iff, for any three elements $x$, $y$, $z$ in $T$, whenever $(x, y) \in R$ and $(y, z) \in R$, then also $(x, z) \in R$.

Various combinations of these properties lead to distinct classes of relations. The following classes of relations are the most common.

• Equivalence relations: reflexive, symmetric, and transitive.
• Compatibility relations: reflexive and symmetric.
• Partial orderings: reflexive, antisymmetric, and transitive.
• Strict orderings: antireflexive, antisymmetric, and transitive.

To illustrate these classes of relations, let us consider the set of students who took together the same course. They are listed by their first names (all distinct from each other) in Table 2.1 together with four characteristics: the grade of each student in the course, major field of study, age, and the status as either full-time or part-time student. Let $T$ denote this set of students. Choosing one or more of the four characteristics, we may define a particular equivalence relation on $T$. We consider students who are not distinguished by the chosen characteristics as equivalent in terms of these characteristics. Thus, for example, Alan and Bob are equivalent in terms of their age and full-time status, but are not equivalent in terms of their grades and their major fields of study. On the other hand, Debby and George are equivalent in terms of each of the characteristics or any combination of them.

The equivalence relation $R_{eq} \subseteq T \times T$ defined in terms of grades is expressed by the matrix in Table 2.2. Entries in this matrix are values of the characteristic
function of this equivalence relation: 1 indicates that the associated pair of students are equivalent in terms of their grades, 0 means that they are not equivalent.

The same equivalence may also be expressed, perhaps more vividly, by the diagram in Fig. 2.2a, in which nodes (circles) represent elements of \( T \) (students) and their connections represent pairs that are contained in \( R_\sim \). For convenience, the diagram is simplified. The connection from each node to itself (as required by

<table>
<thead>
<tr>
<th>Student</th>
<th>Grade</th>
<th>Major</th>
<th>Age</th>
<th>Full-time/part-time</th>
</tr>
</thead>
<tbody>
<tr>
<td>Alan</td>
<td>B</td>
<td>Biology</td>
<td>19</td>
<td>Full-time</td>
</tr>
<tr>
<td>Bob</td>
<td>C</td>
<td>Physics</td>
<td>19</td>
<td>Full-time</td>
</tr>
<tr>
<td>Cliff</td>
<td>C</td>
<td>Mathematics</td>
<td>20</td>
<td>Part-time</td>
</tr>
<tr>
<td>Debby</td>
<td>A</td>
<td>Mathematics</td>
<td>19</td>
<td>Full-time</td>
</tr>
<tr>
<td>George</td>
<td>A</td>
<td>Mathematics</td>
<td>19</td>
<td>Full-time</td>
</tr>
<tr>
<td>Jane</td>
<td>A</td>
<td>Business</td>
<td>21</td>
<td>Part-time</td>
</tr>
<tr>
<td>Lisa</td>
<td>B</td>
<td>Chemistry</td>
<td>21</td>
<td>Part-time</td>
</tr>
<tr>
<td>Mary</td>
<td>C</td>
<td>Biology</td>
<td>19</td>
<td>Full-time</td>
</tr>
<tr>
<td>Nancy</td>
<td>B</td>
<td>Biology</td>
<td>19</td>
<td>Full-time</td>
</tr>
<tr>
<td>Paul</td>
<td>B</td>
<td>Business</td>
<td>21</td>
<td>Part-time</td>
</tr>
</tbody>
</table>

<table>
<thead>
<tr>
<th>( R_\sim )</th>
<th>A</th>
<th>B</th>
<th>C</th>
<th>D</th>
<th>G</th>
<th>J</th>
<th>L</th>
<th>M</th>
<th>N</th>
<th>P</th>
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<td>1</td>
<td>0</td>
<td>0</td>
<td>0</td>
<td>0</td>
<td>0</td>
<td>1</td>
<td>0</td>
<td>1</td>
<td>1</td>
</tr>
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reflexivity) is omitted and connections between nodes are not directed and represent thus both directions (as required by symmetry).

A diagram of another equivalence relation defined on the set of students, one based on their major field of study, $R_m$, is shown in Fig. 2.2b.

We can see that both equivalence relations expressed in Fig. 2.2 ($R_g$ and $R_m$) partition in distinct ways the set of students into subsets such that students in each subset are related to each other while they are not related to students in any of the other subsets. These subsets are called equivalence classes. Each of them consists of students who are equivalent in terms of the relevant characteristics. Thus, for example, students with A grades form an equivalence class based on $R_g$, while
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students majoring in biology form an equivalence class based on \(R_m\). It is easy to see that each equivalence relation partitions the set on which it is defined into equivalence classes.

Consider now a relation defined on the set of students by the following criterion: student \(x\) is related to student \(y\) iff \(x\) does not differ from \(y\) in more than two of the four chosen characteristics. Let \(R_{2c}\) denote this relation. Since the relation is obviously reflexive and symmetric, it can be represented by the simple diagram in Fig. 2.3. We can see from the diagram that \(R_{2c}\) is not transitive. For example, \((N, M) \in R_{2c}\) and \((M, B) \in R_{2c}\), but \((N, B) \not\in R_{2c}\). Hence, \(R_{2c}\) is a compatibility relation. In this case students are classified into six compatibility classes shown in Fig. 2.3. These classes overlap, contrary to equivalence classes induced by an equivalence relation.

Different types of relations can be defined on the set of students by either of the following criteria: \(x\) is related to \(y\) iff \(x\) is younger than \(y\); or \(x\) is related to \(y\) iff \(x\) has a lower grade than \(y\). It is easy to check that both relations are antireflexive, antisymmetric, and transitive. Hence, they are strict orderings. Consider one additional criterion: \(x\) is related to \(y\) iff the grade of \(x\) is lower than or equal to the grade of \(y\). The relation based on this criterion is clearly reflexive, antisymmetric, and transitive and, hence, it is a partial ordering.

When we deal with infinite sets, such as those illustrated in Fig. 2.1, each relation is again defined in terms of a chosen criterion by which it is decided for each element (a point in this case) of the relevant Cartesian product whether it is or is not included in the relation. Examples of six relations on the Cartesian product \(A \times A\) (as defined in Fig. 2.1) are shown in Fig. 2.4. For each relation, the criterion upon which it is based is specified in the figure. The criterion for relation

Figure 2.3. Compatibility relation \(R_{2c}\) defined on the set of students in Table 2.1 by the following criterion: two students are compatible if and only if they do not differ in more than two of the four characteristics.
Figure 2.4. Examples of relations on $A_x \times A_y$ defined in Fig. 2.1.
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$R_1$, for example, can be expressed as follows: a point $x \in A_x$ is related to point $y \in A_y$ iff the inequality

$$(x - 3)^2 + (y - 2)^2 \leq 1$$

holds. Criteria for $R_2 - R_6$ can be expressed in a similar way.

To close this section, let us illustrate how the common-sense definition of systems can be used to determine if a given object is a system. As an example, let us consider a discrete random variable, a variable that takes values in the set $V = \{1, 2, \ldots, n\}$ (for some integer $n \geq 2$) with probabilities $p(v)$ for all $v \in V$. Given a random variable, is it a system? The answer is affirmative if we can express the random variable in terms of the common-sense definition of systems. It is obvious that this can be done. Any random variable can be expressed as a system of the form

$$S = (T = \{V, [0, 1]\}, R \subseteq V \times [0, 1]).$$

Consider now a finite-state machine. It consists of
- a set of input states $X$
- a set of output states $Y$
- a set of internal states $Z$
- an output function $f$ such that $y = f(x, z)$, where $x \in X$, $y \in Y$, and $z \in Z$
- a state-transition function (next-state function) $g$ such that $z' = g(x, z)$, where $x \in X$, $z \in Z$, and $z' \in Z$.

This can be expressed in the form

$$S = (T = \{X, Y, Z\}, R \subseteq X \times Z \times Y \times Z).$$

Hence, any finite-state machine is a system.

2.3. Constructivism versus Realism

Although the common-sense conception of systems allows us to recognize a system, when one is presented to us, it does not help us to construct it. Whence do systems arise? To address this question, let me begin with some relevant thoughts offered by Brian Gaines [1979]:

Definition: A system is what is distinguished as a system. At first sight this looks to be a nonstatement. Systems are whatever we like to distinguish as systems. Has anything been said? Is there any possible foundation here for a systems science? I want to answer both these questions affirmatively and show that this definition is full of content and rich in its interpretation. Let me first answer one obvious objection to the definition above and turn it to my advantage. You may ask, "What is peculiarly systemic about this definition?
Could I not equally well apply it to all other objects I might wish to define?" i.e., "A rabbit is what is distinguished as a rabbit." "Ah, but," I shall reply, "my definition is adequate to define a system but yours is not adequate to define a rabbit." In this lies the essence of systems theory: that to distinguish some entity as being a system is a necessary and sufficient criterion for its being a system, and this is uniquely true for systems. Whereas to distinguish some entity as being anything else is a necessary criterion to its being that something but not a sufficient one.

More poetically we may say that the concept of a system stands at the supremum of the hierarchy of being. That sounds like a very important place to be. Perhaps it is. But when we realize that getting there is achieved through the rather negative virtue of not having any further distinguishing characteristics, then it is not so impressive a qualification. I believe this definition of a system as being that which uniquely is defined by making a distinction explains many of the virtues, and the vices, of systems theory. The power of the concept is its sheer generality; and we emphasize this naked lack of qualification in the term general systems theory, rather than attempt to obfuscate the matter by giving it some respectable covering term such as mathematical systems theory. The weakness, and paradoxically the prime strength, of the concept is in its failure to require further distinctions.

It is a weakness when we fail to recognize the significance of those further distinctions to the subject matter in hand. It is a strength when those further distinctions are themselves unnecessary to the argument and only serve to obscure a general truth through a covering of specialist jargon. No wonder general systems theory is subject to extremes of vilification and praise. Who is to decide in a particular case whether the distinction between the baby and the bath water is relevant to the debate?

What then of some of the characteristics that we do associate with the notion of a system, some form of coherence and some degree of complexity? The Oxford English Dictionary states that a system is "a group, set or aggregate of things, natural or artificial, forming a connected or complex whole." I would argue that any other such characteristics arise out of the process of which making a distinction is often a part, and are some form of post hoc rationalization of the distinction we have made. One set of things is treated as distinct from another and it is that which gives them their coherence; it is that also which increases their complexity by giving them one more characteristic than they had before—that they have now been distinguished. Distinguish the words on this page that contain an "e" from those which do not. You now have a "system" and you can study it and rationalize why you made that distinction, how you can explain it, why it is a useful one. However, none of your postdistinction rationalizations and studies of the "coherency" and "complexity" of the system you have distinguished is intrinsically necessary to it being a "system." They are just activities that naturally follow on from making a distinction when we take note that we have done it and want to "explain" to ourselves, or others, why.
The point made by Gaines in this interesting discussion is that we should not expect that systems can be discovered, ready made for us. Instead, we should recognize that systems originate with us, human beings. We construct them by making appropriate distinctions, be they made in the real world by our perceptual capabilities or conceived in the world of ideas by our mental capabilities.

These sentiments are echoed and articulated with remarkable clarity by Coguen and Varela [1979]:

A distinction splits the world into two parts, “that” and “this,” or “environment” and “system,” or “us” and “them,” etc. One of the most fundamental of all human activities is the making of distinctions. Certainly, it is the most fundamental act of system theory, the very act of defining the system presently of interest, of distinguishing it from its environment.

The world does not present itself to us neatly divided into systems, subsystems, environments, and so on. These are divisions which we make ourselves, for various purposes, often subsumed under the general purpose evoked by saying “for convenience.” It is evident that different people find it convenient to divide the world in different ways, and even one person will be interested in different systems at different times, for example, now a cell, with the rest of the world its environment, and later the postal system, or the economic system, or the atmospheric system.

The established scientific disciplines have, of course, developed different preferred ways of dividing the world into environment and system, in line with their different purposes, and have also developed different methodologies and terminologies consistent with their motivation.

All these considerations are extremely important for proper understanding of the nature of systems, at least as I conceive them, and consequently, as they are viewed in this book. According to this view, systems do not exist in the real world independent of the human mind. They are created by the acts of making distinctions in the real world or, possibly, in the world of ideas. Every act must be made by some agent, and, of course, the agent is in this case the human mind, with its perceptual and mental capabilities.

The view just stated is usually referred to as the constructivist view of reality and knowledge, or constructivism. The most visible contemporary proponent of this view, particularly well recognized within the systems science and cognitive science communities, is Ernst von Glasersfeld. To illuminate the constructivist view

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1The founder of constructivism is generally considered Giambattista Vico (1668–1744), an Italian philosopher. The principal ideas from which constructivism emerged are presented in his early work, On the Most Ancient Wisdom of the Italians (Cornell University Press, Ithaca, New York, 1988), which was originally published in Italian in 1710. Perhaps the most visible contributor to constructivism in this century is Jean Piaget. Basic ideas of constructivism are well overviewed in some writings by Glasersfeld [1987, 1990, 1995]. Arguments supporting constructivism, primarily of biological nature, are well presented by Maturana and Varela [1987].
a little more, let me use a short quotation from his many writings on constructivism [Glasersfeld, 1987]:

Quite generally, our knowledge is useful, relevant, viable, or however we want to call the positive end of the scale of evaluation, if it stands up to experience and enables us to make predictions and to bring about or avoid, as the case may be, certain phenomena (i.e., appearance, events, experiences). If knowledge does not serve that purpose, it becomes questionable, unreliable, useless, and is eventually devalued as superstition. That is to say, from the pragmatic point of view, we consider ideas, theories, and “laws of nature” as structures which are constantly exposed to our experiential world (from which we derived them), and they either hold up or they do not. Any cognitive structure that serves its purpose in our time, therefore, proves no more and no less than just that—namely, given the circumstances we have experienced (and determined by experiencing them), it has done what was expected of it. Logically, that gives us no clue as to how the “objective” world might be; it merely means that we know one viable way to a goal that we have chosen under specific circumstances in our experiential world. It tells us nothing—and cannot tell us anything—about how many other ways there might be, or how that experience which we consider the goal might be connected to a world beyond our experience. The only aspect of that “real” world that actually enters into the realm of experience, are its constraints. . . .

Radical constructivism, thus, is radical because it breaks with convention and develops a theory of knowledge in which knowledge does not reflect an “objective” ontological reality, but exclusively an ordering and organization of a world constituted by our experience.

To avoid any confusion, let me emphasize that the constructivist view does not imply that the existence of the real world independent of the human mind is necessarily denied. This is a different issue, on which constructivism remains neutral. The constructivist view, at least from my perspective, is not an ontological view (concerned with the existence and ultimate nature of reality), but an epistemological view (concerned with the origin, structure, acquisition, and validity of knowledge).

The essence of constructivism is well captured by the following four quotes. The first is due to Giambattista Vico, the founder of constructivism:

God is the artificer of Nature,
man the god of artifacts.

The second quote is from a book by Stephane Leduc [1911]:

Classes, divisions, and separations are all artificial, made not by nature but by man.

The third quote is due to Humberto R. Maturana, an important contributor to modern systems thinking:
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I maintain that all there is is that which the observer brings forth in his or her distinctions. We do not distinguish what is, but what we distinguish is.

The last quote is from a book by Maturana and Varela [1987], in which they bring forth convincing biological arguments in support of constructivism:

All doing is knowing, and
all knowing is doing.

Although the constructivist view may not be typical in classical science (at least not yet), it is undoubtedly the predominant view in contemporary systems science. Indeed, most writings on various aspects of systems science are either openly supportive of the view or, at least, compatible with it. My commitment to the constructivist view in this book reflects, therefore, not only my personal conviction, but also the mainstream of contemporary systems science.

The basic position regarding systems that I take in this book can thus be summarized as follows: Every system is a construction based upon some world of experiences, and these, in turn, are expressed in terms of purposeful distinctions made either in the real world or in the world of ideas.

Given some world of experiences, systems may be constructed in many different ways. Each construction employs some distinctions as primitives and others for characterizing a relation among the primitives. The former distinctions represent thinghood, while the latter represent systemhood of the system constructed.

The chosen primitives may be not only simple distinctions, but also sets of distinctions or even systems constructed previously. To illustrate this point, let all words printed on this page be taken as primitives and those that are verbs be distinguished from those that are not. Clearly, the primitives themselves are in this case rather complex systems, based upon many distinctions (visual, grammatical, semantic), but all these finer distinctions clustered around each word are taken for granted and left in the background when we choose to employ the words as primitives in a larger system. We consider the words as things whose recognition is assumed, and focus on the extra distinction by which verbs are distinguished from other words. This distinction imposes an equivalence relation on the set of words, which partitions the set into two equivalence classes, the class of all verbs and the class of all nonverbs on this page.

Constructivism is one of two opposing views about the nature of systems. The other view is usually referred to as realism.

According to realism, each system that is obtained by applying correctly the principles and methods of science represents some aspect of the real world. This representation is only approximate, due to limited resolution of our senses and measuring instruments, but the relation comprising the system is the homomorphic image (see Sec. 5.2) of its counterpart in the real world. When we use more refined
instruments, the homomorphic mapping between entities of the system of concern and those of its real-world counterpart (the corresponding "real system") becomes also more refined, and the system becomes a better representation of its real-world counterpart.

Realism thus assumes the existence of systems in the real world, which are usually referred to as "real systems." It claims that any system obtained by sound scientific inquiry is an approximate (simplified) representation of a "real system" via an appropriate homomorphic mapping.

According to constructivism, all systems are artificial abstractions. They are not made by nature and presented to us to be discovered, but we construct them by our perceptual and mental capabilities with the domain of our experiences. The concept of a system that requires correspondence to real world is illusory because there is no way of checking such correspondence. We have no access to the real world except through experience.

The constructivist position liberates us from the commitment (inherent in realism) of viewing systems we deal with as models of "real systems." This commitment is vacuous since we have no access to the original system in this modeling relationship—the presumed "real system." Hence, we could define the homomorphic mapping between the two systems (assuming the existence of the "real system") only if we were omnipotent. Then, however, we would not have to worry about epistemology at all. This issue is discussed in more detail in Chapter 5.

2.4. Classification of Systems

Although much more could be said about the common-sense conception of systems (see, e.g., a thorough discussion by Marchal [1975]), I believe that enough has already been said here for our purpose. To make the concept of system useful, the common-sense definition must be refined in the sense that specific classes of ordered pairs \((T, R)\), relevant to recognized problems, must be introduced. This can be done in one of two fundamentally different ways:

a. By restricting \(T\) to certain kinds of things;
b. By restricting \(R\) to certain kinds of relations.

Although the two types of restrictions are independent of each other, they can be combined.

Restrictions of type (a) are exemplified by the traditional classification of science into disciplines and specializations, each focusing on the study of certain kinds of things (physical, chemical, biological, economic, social, etc.) without committing to any particular kind of relations. Since different kinds of things are based on different types of distinctions, they require the use of different senses or
measuring instruments and techniques. Hence, this classification is essentially experimentally based.

Restrictions of type (b) lead to fundamentally different classes of systems, each characterized by special kinds of relations, with no commitment to any particular kind of things on which the relations are defined. Since systems characterized by different types of relations require different theoretical treatment, this classification, which is fundamental to systems science, is predominantly theoretically based.

A prerequisite for classifying systems by their systemhood properties is a conceptual framework within which these properties can properly be codified. Each framework determines the scope of systems conceived. It captures some basic categories of systems, each of which characterizes a certain type of knowledge representation, and provides a basis for further classification of systems within each category. To establish firm foundations of systems science, a comprehensive framework is needed to capture the full scope of systemhood properties.

The issue of how to form conceptual frameworks for codifying systemhood properties, and the associated issue of how to classify systems by their systemhood properties are addressed in Chap. 4. Without waiting for the full discussion of these issues, however, we are able to introduce one rather important systemhood-dependent and thinghood-independent classification of systems right now, using only the common-sense definition of systems. This classification is based upon an important equivalence relation defined on the set of all systems of interest, say all systems captured by the common-sense definition. According to this relation, which is called an isomorphic relation, two systems are considered equivalent if their systemhood properties are totally preserved under some suitable transformation from the set of things of one system into the set of things of the other system.

To illustrate the notion of isomorphic systems, let us consider two systems, \( S_1 = (T_1, R_1) \) and \( S_2 = (T_2, R_2) \). Assume, for the sake of simplicity, that \( T_1, T_2 \) are single sets and \( R_1 \subseteq T_1 \times T_1, R_2 \subseteq T_2 \times T_2 \). Then \( S_1 \) and \( S_2 \) are called isomorphic systems if and only if there exists a transformation from \( T_1 \) to \( T_2 \) expressed in this case by a bijective (one-to-one) function \( h: T_1 \rightarrow T_2 \), under which things that are related in \( R_1 \) are also related in \( R_2 \) and, conversely, things that are related in \( R_2 \) are also related in \( R_1 \). Formally, systems \( S_1 \) and \( S_2 \) are isomorphic if and only if, for all \( (x_1, x_2) \in T_1 \times T_1 \),

\[
(x_1, x_2) \in R_1 \text{ implies } [h(x_1), h(x_2)] \in R_2
\]

and, for all \( (y_1, y_2) \in R_2 \),

\[
(y_1, y_2) \in R_2 \text{ implies } [h^{-1}(y_1), h^{-1}(y_2)] \in R_1
\]
where $f^{-1}$ denotes the inverse of function $f$. This definition is illustrated in Fig. 2.5. It must be properly extended when $T_1$, $T_2$ are families of sets and $R_1$, $R_2$ are not binary but $n$-dimensional relations with $n > 2$.

The notion of isomorphic systems imposes a binary relation on $\mathcal{S} \times \mathcal{S}$ where $\mathcal{S}$ denotes the set of all systems. Two systems, $S_1$ and $S_2$, are related by this relation (the pairs $S_1$, $S_2$ and $S_2$, $S_1$ are contained in the relation) if and only if they are isomorphic.

It is easy to verify that the isomorphic relation (or isomorphism) is reflexive, symmetric, and transitive. Consequently, it is an equivalence relation defined on the set of all systems (or on its arbitrary subset), which partitions the set into equivalence classes. These are the smallest classes of systems that can be distinguished from the standpoint of systemhood. In fact, we may view each of these equivalent classes as being characterized by a unique relation, defined on some particular set of things, which is freely interpreted in terms of other sets of things within the class. This unique relation may be taken as a canonical representative of the class.

Which set of things should be chosen for these canonical representations? Although the choice is arbitrary, in principle it is essential that the same selection criteria be used for all isomorphic classes. Otherwise, the representatives would not be compatible and, consequently, it would be methodologically difficult to deal with them. Therefore, it is advisable to define the representatives as systems whose

\[
S_1 = (T_1, R_1) \\
T_1 = \{1, 2, 3, 4\} \\
\]

\[
S_2 = (T_2, R_2) \\
T_2 = \{a, b, c, d\} \\
\]

![Figure 2.5. An example of isomorphic systems.](image-url)
sets of things consist of some standard symbols that are abstract (interpretation-free), such as integers or real numbers, and whose relations are described in some convenient standard form.

Since the canonical representatives of isomorphic equivalence classes of systems are devoid of any interpretation, it is reasonable to call them general systems. Hence, a general system is a standard and interpretation-free system chosen to represent a particular equivalence class of isomorphic systems.

Observe that, according to this definition, each isomorphic equivalence class may potentially contain an infinite number of systems, owing to the unlimited variety of thinghood, but it contains only one general system. The number of isomorphic equivalence classes and, thus, the number of general systems, may also be potentially infinite owing to the unlimited variety of systemhood.

Each isomorphic equivalence class of systems contains not only a general system and its various interpreted systems, but also other abstract systems, different from the general system. The isomorphic transformations between the general system and the other abstract systems (described by the bijective function \( h \)) are rather trivial. They are just arbitrary replacements of one set of abstract symbols with another set. It seems appropriate to call these transformations relabelings.

The transformations between a general system and its various interpreted systems are by far not trivial since they involve different types and distinctions made in the real world, and these, in turn, are subject to different constraints of the real world. The isomorphic transformation from an interpreted system into the corresponding general system, which may be called an abstraction, is always possible.

The inverse transformation, which may be called an interpretation, is not guaranteed and must be properly justified in each case. Indeed, relations among things based upon distinctions made in the real world cannot be arbitrary, but must reflect genuine constraints of the real world, as represented in our world of experiences. Hence, each interpreted system determines uniquely its representative general systems by the isomorphic transformation, but not the other way around.

The independence (or orthogonality) of the two ways of classifying systems, by thinghood and by systemhood, is visually expressed in Fig. 2.6. This figure also illustrates the role of general systems and their connection to other abstracted systems and to interpreted systems.

The two dimensions of science, which reflect the two-dimensional classification of systems symbolized by Fig. 2.6, are complementary. When combined in scientific inquiries, they are more powerful than either of them alone. The traditional perspective of classical science provides a meaning and context to each inquiry. The perspective of systems science, on the other hand, provides a means for dealing with any desirable system, regardless of whether or not it is restricted to a particular discipline of classical science.
Exercises

2.1. How many possible relations can be defined on each of the following Cartesian products of finite sets?
   (a) $A \times B^2 \times C^3$;
   (b) $(A \times B \times C)^2$
   (c) $\mathcal{P}(A) \times (\mathcal{P}(B))^2 \times (\mathcal{P}(B))^3$;
   (d) $A^2 \times \mathcal{P}(B) \times C \times \mathcal{P}(D)$;
   (e) $\mathcal{P}(\mathcal{P}(A)) \times \mathcal{P}(\mathcal{P}(B))$;
   (f) $(A \times B) \times (B \times C) \times (D \times E \times F)$;
   (g) $(A \times B) \times \mathcal{P}(\mathcal{P}(C)) \times D^2$.

2.2. For each of the following relations, determine whether or not it is reflexive, antireflexive, symmetric, antisymmetric, or transitive.
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(a) \( R \subseteq \mathcal{P}(X) \times \mathcal{P}(X) \); \((A, B) \in R \) iff \( A \subseteq B \) for all \( A, B \in \mathcal{P}(X) \);

(b) \( R \subseteq C \times C \); where \( C \) denotes the set of courses in a graduate program; \((a, b) \in R \) iff course \( a \) is a prerequisite of course \( b \);

(c) \( R_n \subseteq \mathbb{N} \times \mathbb{N} \); where \( \mathbb{N} \) denotes the set of all natural numbers; \((a, b) \in R \) iff the remainders obtained by dividing \( a \) and \( b \) by \( n \) are the same, where \( n \) is some specific natural number greater than 1;

(d) \( R \subseteq W \times W \); where \( W \) denotes the set of all English words; \((a, b) \in R \) iff \( a \) is a synonym of \( b \);

(e) \( R \subseteq F \times F \); where \( F \) denotes the set of all five-letter English words; \((a, b) \in R \) iff \( a \) differs from \( b \) in at most one position;

(f) \( R \subseteq A \times A \); \((a, b) \in R \) iff \( f(a) = f(b) \), where \( f \) is a function of the form \( f : A \rightarrow A \);

(g) \( R \subseteq T \times T \); where \( T \) denotes all persons included in a family tree; \((a, b) \in R \) iff \( a \) is an ancestor of \( b \);

(h) \( R = \{(0, 0), (0, 1), (1, 0), (1, 1), (1, 2), (2, 2), (0, 2), (3, 3)\} \);

(i) \( R = \{(0, 0), (0, 3), (1, 1), (2, 2), (1, 0), (0, 1), (3, 1), (3, 3), (3, 0)\} \).

2.3. A finite Markov chain is a system based on a finite set of states, \( A \), in which the next state \( a' \in A \) is determined by the present state \( a \in A \) via conditional probability \( p(a'|a) \), where

\[
\sum_{a' \in A} p(a'|a) = 1
\]

for each \( a \in A \). Express the Markov chain based on state set \( A \) and conditional probabilities \( p(a'|a) \) for all \( a \in A \) and \( a' \in A \) in terms of the common-sense definition of systems, \( S = (T, R) \).

2.4. Consider an archive that contains a set of documents, \( D \), each of which is characterized by a set of relevant index terms (keywords) taken from a set of all index terms employed, \( I \). Describe the archive as a system \( S = (T, R) \).

2.5. The Shannon (probabilistic) finite-state machine is a quadruple

\[ M = (X, Y, Z, P), \]

where \( X, Y, Z \) are finite sets of input states, output states, and internal states, respectively, and \( P \) is a set of conditional probabilities

\[ p(y_{t+1}|x_t, z_t), \]

where \( x_t, y_t, z_t \) denote, respectively, input, output, and internal states at time \( t \), and \( y_{t+1} \) denotes an internal state at time \( t + 1 \). Show that the Shannon machine is a system since it can be expressed in terms of the common-sense definition.
2.6. Given two systems $S_1$ and $S_2$ of the form

\[ S_1 = (\{A_1, A_2, A_3\}, R_1 \subseteq A_1 \times A_2 \times A_3), \]

\[ S_2 = (\{B_1, B_2, B_3\}, R_2 \subseteq B_1 \times B_2 \times B_3), \]

define the conditions under which these systems are isomorphic.

2.7. Show that the system based on the set of all letters of the English alphabet and their alphabetical order and the system based on the set \(\{10, 11, \ldots, 35\}\) with the usual numerical ordering are isomorphic.

2.8. Determine which pairs of relations defined by their diagrams in Fig. 2.7 are isomorphic and specify for each pair the function \(h\) under which the relations are isomorphic.
CHAPTER 3

Systems Movement

*The more science becomes divided into specialized disciplines, the more important it becomes to find unifying principles.*

—HERMAN HAKEN

*Systems science* is a phenomenon of the second half of the 20th century. It developed within a movement that is usually referred to as *systems movement*. In general, systems movement may be characterized as a loose association of people from different disciplines of science, engineering, philosophy, and other areas, who share a common interest in ideas (concepts, principles, methods, etc.) that are applicable to all systems and that, consequently, transcend the boundaries between traditional disciplines.

Systems movement emerged from three principal roots: *mathematics, computer technology*, and a host of ideas that are well captured by the general term *systems thinking*.

3.1. The Role of Mathematics and Computer Technology

Since at least the publication of Newton's *Principia* in 1687, mathematics has played a key role in describing and dealing with systems in various areas of science and engineering. Prior to the 20th century, however, mathematics was capable of dealing only with rather simple systems, consisting of a mere handful of variables (usually two or three) related in a functional way. This was adequate for typical problems in science and engineering until physics became concerned with processes at the molecular level in the late 19th century. It was obvious that the available mathematical methods were completely useless for investigating processes of this sort. They were not useless in principle, but owing to the enormous number of entities involved: a gas in a small closed space would typically contain on the order of $10^{23}$ molecules. The need for fundamentally new mathematical tools resulted eventually in statistical methods, which turned out to be applicable not only to the study of molecular processes (statistical mechanics), but to a host of other areas such as the actuarial profession, design of large telephone exchanges, and the like.
While the classical mathematical tools, exemplified by the calculus and differential equations, are applicable to problems that involve only a very small number of components that are related in perfectly predictable ways, the applicability of statistical tools is exactly the opposite: they require a very large number of components and a high degree of unpredictability (randomness). These two types of mathematical tools are thus complementary. When one of them excels, the other totally fails. Despite their complementarity, these mathematical tools can deal only with problems that are clustered around the two extremes of complexity and randomness scales. In his classic paper, Warren Weaver [1948] refers to them as problems of organized simplicity (simple, deterministic) and problems of disorganized complexity (complex, random). He describes them with unmatched clarity:

The classical dynamics of the nineteenth century was well suited for analyzing and predicting the motion of a single ivory ball as it moves about on a billiard table. In fact, the relationship between positions of the ball and the times at which it reaches these positions forms a typical nineteenth-century problem of simplicity. One can, but with a surprising increase in difficulty, analyze the motion of two or even of three balls on a billiard table. There has been, in fact, considerable study of the mechanics of the standard game of billiards. But, as soon as one tries to analyze the motion of ten or fifteen balls on the table at once, as in pool, the problem becomes unmanageable, not because there is any theoretical difficulty, but just because the actual labor of dealing in specific detail with so many variables turns out to be impracticable.

![Figure 3.1](image.png)

Figure 3.1. Three classes of systems and associated problems that require distinct mathematical tools [Weaver, 1948].
Imagine, however, a large billiard table with millions of balls rolling over its surface, colliding with one another and with the side rails. The great surprise is that the problem now becomes easier, for the methods of statistical mechanics are applicable. To be sure the detailed history of one special ball cannot be traced, but certain important questions can be answered with useful precision, such as: On the average how many balls per second hit a given stretch of rail? On the average how far does a ball move before it is hit by some other ball? On the average how many impacts per second does a ball experience?

Earlier it was stated that the new statistical methods were applicable to problems of disorganized complexity. How does the word “disorganized” apply to the large billiard table with the many balls? It applies because the methods of statistical mechanics are valid only when the balls are distributed, in their positions and motions, in a helter-skelter, that is to say a disorganized, way. For example, the statistical methods would not apply if someone were to arrange the balls in a row parallel to one side rail of the table, and then start them all moving in precisely parallel paths perpendicular to the row in which they stand. Then the balls would never collide with each other nor with two of the rails, and one would not have a situation of disorganized complexity.

From this illustration it is clear what is meant by a problem of disorganized complexity. It is a problem in which the number of variables is very large, and one in which each of the many variables has a behavior which is individually erratic, or perhaps totally unknown. However, in spite of this helter-skelter, or unknown, behavior of all the individual variables, the system as a whole possesses certain orderly and analyzable average properties.

Weaver further argues that problems of organized simplicity and disorganized complexity cover only a tiny fraction of all systems problems. Most problems are located somewhere between the two extremes of complexity and randomness, as illustrated by the gray area in Fig. 3.1; Weaver calls them problems of organized complexity and explains the reason for coining this name:

This new method of dealing with disorganized complexity, so powerful an advance over the earlier two-variable methods, leaves a great field untouched. One is tempted to oversimplify, and say that scientific methodology went from one extreme to the other—from two variables to an astronomical number—and left untouched a great middle region. The importance of this middle region, moreover, does not depend primarily on the fact that the number of variables involved is moderate—large compared to two, but small compared to the number of atoms in a pinch of salt. The problems in this middle region, in fact, will often involve a considerable number of variables. The really important characteristic of the problems of this middle region, which science has as yet little explored or conquered, lies in the fact that these problems, as contrasted with the disorganized situations with which statistics can cope, show the essential feature of organization. In fact, one can refer to this group of problems as those of organized complexity.

What makes an evening primrose open when it does? Why does salt water
fail to satisfy thirst? Why can one particular genetic strain of microorganisms synthesize within its minute body certain organic compounds that another strain of the same organism cannot manufacture? Why is one chemical substance a poison when another, whose molecules have just the same atoms but assembled into a mirror-image pattern, is completely harmless? Why does the amount of manganese in the diet affect the maternal instinct of an animal? What is the description of aging in biochemical terms? What meaning is to be assigned to the question: Is a virus a living organism? What is a gene, and how does the original genetic constitution of a living organism express itself in the developed characteristics of the adult? Do complex protein molecules "know how" to reduplicate their pattern, and is this an essential clue to the problem of reproduction of living creatures? All these are certainly complex problems, but they are not problems of disorganized complexity, to which statistical methods hold the key. They are all problems which involve dealing simultaneously with a sizable number of factors which are interrelated into an organic whole. They are all, in the language here proposed, problems of organized complexity.

On what does the price of wheat depend? This too is a problem of organized complexity. A very substantial number of relevant variables is involved here, and they are all interrelated in a complicated, but nevertheless not in helter-skelter, fashion.

How can currency be wisely and effectively stabilized? To what extent is it safe to depend on the free interplay of such economic forces as supply and demand? To what extent must systems of economic control be employed to prevent the wide swings from prosperity to depression? These are also obviously complex problems, and they too involve analyzing systems which are organic wholes, with their parts in close interrelation.

How can one explain the behavior pattern of an organized group of persons such as a labor union, or a group of manufacturers, or a racial minority? There are clearly many factors involved here, but it is equally obvious that here also something more is needed than the mathematics of averages. With a given total of national resources that can be brought to bear, what tactics and strategy will most promptly win a war, or better: what sacrifices of present selfish interest will most effectively contribute to a stable, decent, and peaceful world?

These problems—and a wide range of similar problems in the biological, medical, psychological, economic, and political sciences—are just too complicated to yield to the old nineteenth-century techniques which were so dramatically successful on two-, three-, or four-variable problems of simplicity. These new problems, moreover, cannot be handled with the statistical techniques so effective in describing average behavior in problems of disorganized complexity.

These new problems, and the future of the world depends on many of them, require science to make a third great advance, an advance that must be even greater than the nineteenth-century conquest of problems of simplicity or the twentieth-century victory over problems of disorganized complexity. Sci-
ence must, over the next 50 years, learn to deal with these problems of organized complexity.

Instances of problems with the characteristics of organized complexity are abundant, particularly in the life, behavioral, social, and environmental sciences, as well as in applied fields such as modern technology or medicine. Some of the problem areas that involve organized complexity are especially profound, such as cancer research, the study of aging, or the rich area of difficult and diverse problems associated with modern technology. This last area is well characterized by George B. Dantzig in his 1979 Distinguished Lecture at the International Institute for Applied Systems Analysis in Laxenburg, Austria, an institute that has played an important role in this new thrust of science into the domain of organized complexity:

It is not easy to paint a picture of just how complex modern technology is. One way to start is to list the activities of a small town. By using the classified section of the telephone directory, I can list a few activities of the town of Richmond, California. Here are those that begin with the letters Br: Bridge Builders, Bridge Tables, Broadcasting Stations, Brochures, Brokers, Bronze, Brushes, Brooches, Brakes, Brandies, Brazing, Bricks, Brick Stain, Bric-a-Brac. I counted over 6,000 activities in all.

Another way to see the diversity of the material side of life is to look at a catalog of electronic supply items that are for sale. These are thousands upon thousands of different kinds of resistors, condensers, vacuum tubes, transistors, cables, sockets, knobs, switches, dials, circuit boards, cabinets. Look up the number of different items listed in a chemical supply catalog or a Sears, Roebuck catalog, and again the number of different items runs into many thousands. A modern university can have a hundred different departments. The United States Government has nearly 2,000 different kinds of offices in San Francisco alone, each presumably carrying out a different function for the public good. So far we have spoken only of diversity, but complexity has other dimensions.

The Leontief input–output model of the national economy of the United States classifies industries into about 400 major types and requires data for each of these industries about how much it shipped (or received) from every other industry. The resulting $400 \times 400$ table contains 160,000 numbers. Each region of the country has such an input–output table, and there are many regions. Each number in an input–output table expresses a dependency of one industry upon another; the transactions between regions and industries represent further dependencies; there are a great number of cross combinations. Countries depend on each other in the same way.

There are also time dependencies: facilities are built and maintained for future use; material is stockpiled for future use; people are trained for future jobs. There are locational dependencies as well: men, material, and facilities are moved to new locations, not only on the surface of the globe but below and above.
While we may easily understand the ins and outs of each small part of this vast web of activities, the problem is how to track all the interactions at once. We know that the powerful forces of population growth, shortages of raw materials, food, energy, growing affluence, and so on, are rapidly reshaping this complexity. There is a fear that the structure that interconnects these activities may not hold up very well under these stresses. We see the possibility of all kinds of system failure if we let the changes go on uncontrolled.

There is no doubt that both analytical and statistical methods are relevant to systems science. However, the primary orientation of systems science, at least as I view it, is to study systems and associated problems that possess the characteristics of organized complexity. Studying the domain of organized complexity requires, in comparison with the domains of organized simplicity and disorganized complexity, a much higher level of systemhood expertise. To achieve such a level of expertise for a traditional scientist, in addition to the ever-increasing thinghood-oriented knowledge pertaining to his or her specialization, is virtually impossible owing to the fundamental limits of the human mind. Hence, the role of developing and applying the systemhood expertise must be undertaken by a scientist of a different kind, a systems scientist, whose specialization is this very expertise.

One of the major difficulties with problems of organized complexity is that one cannot often avoid massive combinatorial searches of various kinds without losing relevance. This is a weakness of the human mind, while, at the same time, it is a strength of the computer. This explains why attempts to investigate systems with the characteristics of organized complexity had not been successful prior to the emergence of computer technology. This also explains why the evolution of systems science has been so closely correlated with the evolution of computer technology.

Systems science is strongly dependent upon the computer, which is its laboratory (Sec. 7.2) as well as its most important methodological tool (Sec. 5.2). It is thus not surprising that the visibility of modern systems ideas, which have been emerging in science and engineering since the beginning of this century, significantly increased shortly after the fully automatic digital computers were built in the late 1940s and early 1950s. It is also not surprising that the beginnings of systems movement are clearly associated with this period. Since these beginnings, systems movement and computer technology have been developing side by side and have been influencing each other.

Systems science, an offspring of systems movement, has a particularly strong linkage with computer technology. The steadily increasing computing power opens new methodological possibilities, which stimulate novel developments in mathematics, and these, in turn, make advances in systems science. These three areas—mathematics, computer technology, and systems science—are thus intimately interrelated. Computer technology has a key role in this relationship; it is a sort of catalyzer that influences the conversion of methodological possibilities into methodological actualities.
3.2. Systems Thinking

I hope it is now clear that mathematics and computer technology, which are fundamental for describing and dealing with systemhood phenomena, played an important role in the emergence of systems movement. The crucial factor in this emergence, however, was a host of systemhood-oriented ideas that have arisen in philosophy, science, and engineering since the beginning of the 20th century. Although these ideas are quite diverse, they are all concerned with systemhood properties. Due to this unique feature, they are usually subsumed under the general term systems thinking. Let me overview some of these ideas, which, in my opinion, were decisive in the initial, formative stage of systems movement.

Perhaps the most important of these ideas that contributed to the emergence of systems movement were ideas associated with a cluster of related views that are referred to as holism (from Greek holos, which means a whole). The basic idea of holism is well captured by the famous statement, “The whole is more than the sum of its parts,” which has a long history and whose author is, presumably, Aristotle.

It is well known that holism was already well entrenched in classical Greek philosophy (as well as, for example, in the ancient Chinese philosophy of Taoism) and, consequently, it is not a product of this century. However, it reappeared, rather forcefully, at the turn of the century, as an antithesis of reductionism, a methodological view predominant in science since about the 16th century. The latter claims that properties of a whole are explicable in terms of properties of the constituent elements. Holism rejects this claim and maintains that a whole cannot be analyzed, in general, in terms of its parts without some residuum.

The rejuvenation of holism in the 20th century was a result of the recognition (or frustration) in some disciplines of science that reductionism was not able to explain certain phenomena belonging to these disciplines. Let me look at just two disciplines, in which the need for holism was most visible: psychology and biology.

The holistic movement in psychology, which is referred to as Gestalt theory (or Gestalt psychology), originated in Germany in the 1910s and 1920s. The word Gestalt means in German an organized whole whose parts belong together. To characterize the spirit of Gestalt theory, let me use the words of one of its founders, Max Wertheimer:

It has long seemed obvious—that is, in fact, the characteristic tone of European science—that “science” means breaking up complexes into their component...

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*Three German psychologists, Max Wertheimer, Wolfgang Köhler, and Kurt Koffka, are usually recognized as founders of Gestalt theory. Our quotation is from a conference address by Wertheimer in Berlin in 1924; English translation of this address is included with other classical writing on Gestalt theory in a useful source book (Ellis, 1938). Classic monographs on Gestalt theory were written by Köhler (1929) and Koffka (1935); textbooks by Hartmann (1935) and Katz (1950) are recommended as excellent introductions to Gestalt theory.*
relation between them. This knowledge may have been obtained by three simpler monitors, each of which allows us to monitor only two units simultaneously. Clearly, each of these monitors must give us the same observation regarding the two observed units as the larger monitor, but it does not give us any observations regarding the third unit. That is, observations of the three monitors can be obtained from the table of observations of the larger monitor by selecting from it the three possible pairs of columns:

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How much can we infer from these parts (binary relations) about the whole (the ternary relation)? We can easily see that we can infer very little. Since each of the binary relations contains all possible pairs L and H, we may conclude that the attributes (utilizations of the three computing units) are pairwise independent. Yet, they are far from being independent when considered all together as a whole: they are not triplewise independent. It is clear that the ternary relation cannot be understood in terms of the binary relations in this case. Without knowing the ternary relation, we have no way to reconstruct it from the binary relations. In fact, we can find by simple combinatorial considerations that our ternary relation is only one of 35 different ternary relations that result in the same binary relations. Hence, knowing only the binary relations, we cannot decide which of the 35 ternary relations is the actual one.

Perhaps the most comprehensive and thorough exposition of holism is covered in a classic book Holism and Evolution by Jan Christian Smuts [1926]. In this book, Smuts elevates holistic thinking into a general self-organizing principle that is fundamental to evolution of nature at all levels, from inorganic matter through the many forms of life to the human mind and the human society. The book is important not only from a historical perspective, as it certainly contributed to the emergence of systems movement, but also from the standpoint of current developments in systems science. It is particularly relevant to recent interests in studying self-organizing processes through which order develops from chaos. The coherent holistic view carefully developed and clearly expressed by Smuts seems still the best framework for pursuing these studies in a unified fashion.

To get a better feeling for this important book, let me present here a few quotations from it:

The creation of wholes, and ever more highly organized wholes, and of wholeness generally as characteristic of existence, is an inherent character of
the universe. There is not a mere vague indefinite creative energy or tendency at work in the world. This energy or tendency has specific characters; the most fundamental of which is whole-making. And the progressive development of the resulting wholes at all stages—from the most inchoate, imperfect, inorganic wholes to the most highly developed and organized—is what we call Evolution. The whole-making, holistic tendency, or Holism, operating in and through particular wholes, is seen at all stages of existence, and is by no means confined to the biological domain to which science has hitherto restricted it. With its roots in the inorganic, this universal tendency attains clear expression in the organic biological world, and reaches its highest expressions and results on the mental and spiritual planes of existence. Wholes of various grades are the real units of Nature. Wholeness is the most characteristic expression of the nature of the universe in its forward movement in time. It marks the line of evolutionary progress. And Holism is the inner driving force behind that progress.

It is evident that if this view is correct, very important results must follow for our conceptions of knowledge and life. Wholes are not mere artificial constructions of thought, they point to something real in the universe; and Holism as the creative principle behind them is a real vera causa. It is the motive force behind Evolution. We thus have behind Evolution not a mere vague and indefinable creative impulse or elan vital, the bare idea of passage or duration without any quality or character, and to which no value or character could be attached, but something quite definite. Holism is a specific tendency, with a definite character, and creative of all characters in the universe, and thus fruitful of results and explanations in regard to the whole course of cosmic development.

It is possible that some may think I have pressed the claims of Holism and the whole too far; that they are not real operative factors, but only useful methodological concepts or categories of research and explanation. There is no doubt that the whole is a useful and powerful concept under which to range the phenomena of life especially. But to my mind there is clearly something more in the idea. The whole as a real character is writ large on the face of Nature. It is dominant in biology; it is everywhere noticeable in the higher mental and spiritual developments; and science, if it had not been so largely analytical and mechanical, would long ago have seen and read it in inorganic nature also. The whole as an operative factor requires careful exploration. That there are wholes in Nature seems to me incontestable. That they cover a very much wider field than is generally thought and are of fundamental significance is the view here presented. But the idea of the whole is one of the neglected matters of science and to a large extent of philosophy also. It is curious that, while the general viewpoint of philosophy is necessarily largely holistic, it has never made real use of the idea of the whole. The idea runs indeed as a thread all through philosophy, but mostly in a vague intangible way....

It is very important to recognize that the whole is not something additional to the parts: it is the parts in a definite structural arrangement and with mutual
activities that constitute the whole. The structure and the activities differ in character according to the stage of development of the whole; but the whole is just this specific structure of parts with their appropriate activities and functions. . . . The concept of Holism and the whole is as nearly as possible a replica of Nature's observed process, and its application will prevent us from appearing to run the stuff of reality into a mould alien to Nature. It will, therefore, enable us to explain Nature from herself, so to say, and by her own standards. In this way justice can be done to the concrete character of natural phenomena. . . .

A whole is a synthesis or unity of parts, so close that it affects the activities and interactions of those parts, impresses on them a special character, and makes them different from what they would have been in a combination devoid of such unity or synthesis. That is the fundamental element in the concept of the whole. It is a complex of parts, but so close and intimate, so unified that the characters and relations and activities of the parts are affected and changed.

Holism is also creative of all values. Take the case of organic Beauty. It is undeniable that Beauty rests on a holistic basis. Beauty is essentially a product of Holism and is inexplicable apart from it. Beauty is of the whole; Beauty is a relation of parts in a whole, a blending of elements of form and colour, of foreground and background of expression and suggestion, of structure and function, of structure and field, which is perceived and appreciated as harmonious and satisfying, according to laws which it is for Aesthetics to determine. . . .

For me the great problem of knowledge, indeed the great mystery of reality, is just this: How do elements or factors \( a \) and \( b \) come together, combine and coalesce to form a new unity or entity \( x \) different from both of them? To my mind this simple formula of synthesis sums up all the fundamental problems of matter and life and mind. The answer to this question will in some measure supply the key to all or most of our great problems. My answer has already been given; it is in the word Holism.

The status of a system as either a whole (an overall system) or a part (a subsystem) is, of course, not absolute. The same system may be viewed in one context as a whole and in another context as a part. We may say, more poetically, that a part is a whole in a role (in one context), and a whole is a part in a role (in another context). This duality makes it possible to represent systems hierarchically in the sense that a system conceived as a whole may consist of interconnected parts that themselves are systems, and each of these parts may again consist of interconnected parts that are systems, etc., until some primitive parts are reached that do not qualify as systems. The nature of this hierarchy is concisely captured by Goguen and Varela [1979]:

At a given level of the hierarchy, a particular system can be seen as an outsider to systems below it, and as an insider to systems above it; thus, the status (i.e., the mark of distinction) of a given system changes as one passes through its level, in either the upward or the downward direction. The choice of consider-
Systems Movement

ing the level above or below corresponds to a choice of treating the given system as autonomous or controlled (constrained).

To emphasize the dual role of a system at some level of the hierarchy, either as a part or a whole, Arthur Koestler suggested a new term, holon [Koestler and Smythies, 1969]:

A part, as we generally use the word, means something fragmentary and incomplete, which by itself would have no legitimate existence. On the other hand, there is a tendency among holists to use the word “whole” or “Gestalt” as something complete in itself which needs no further explanation. But wholes and parts in this absolute sense do not exist anywhere, either in the domain of living organisms or of social organizations. What we find are intermediary structures on a series of levels in ascending order of complexity, each of which has two faces looking in opposite directions: the face turned towards the lower levels is that of an autonomous whole, the one turned upward that of a dependent part. I have elsewhere [Koestler, 1967] proposed the word “holon” for these Janus-faced sub-assemblies—from the Greek holos—whole, with the suffix on (cf. neutron, proton) suggesting a particle or part.

Holism, in opposition to reductionism, was undoubtedly one of the main roots from which systems movement sprang. Initially, a tendency toward a full commitment to holism and a total rejection of reductionism was quite visible in systems movement. Over the years, this extreme position became slowly moderated. Now, the two doctrines are viewed, by and large, as complementary. While holism is accepted as a thesis that is correct on logical grounds (as documented by our simple example of computer monitoring and endless other examples) and, consequently, desirable to follow as an ideal guiding principle, it is recognized that its applicability is often limited on pragmatic grounds. For example, simultaneous monitoring of a large number of variables may be technically impossible or impractical, computational demands for dealing with a desirable overall system may exceed our computational limits, the overall system may be incomprehensible to the human mind, etc. The complementarity of holism and reductionism is well described by Goguen and Varela [1979]:

Most discussions place holism/reductionism in polar opposition. This seems to stem from the historical split between empirical sciences, viewed as mainly reductionist or analytic, and the (European) schools of philosophy and social science that grope toward a dynamics of totalities.

Both attitudes are possible for a given descriptive level, and in fact they are complementary. On the one hand, one can move down a level and study the properties of the components, disregarding their mutual interconnection as a system. On the other hand, one can disregard the detailed structure of the components, treating their behavior only as contributing to that of a larger unit. It seems that both these directions of analysis always coexist, either implicitly or explicitly, because these descriptive levels are mutually interdependent for
the observer. We cannot conceive of components if there is no system from which they are abstracted; and there cannot be a whole unless there are constitutive elements.

These descriptive levels haven’t been generally realized as complementary largely because there is a difference between publicly announced methodology and actual practice, in most fields of research in modern science. A reductionist attitude is strongly promoted, yet the analysis of a system cannot begin without acknowledging a degree of coherence in the system to be investigated; the analyst has to have an intuition that he is actually dealing with a coherent phenomenon. Although science has publicly taken a reductionist attitude, in practice both approaches have always been active. It is not that one has to have a holistic view as opposed to a reductionist view, or vice versa, but rather that the two views of systems are complementary. . . . Reductionism implies attention to a lower level, while holism implies attention to a higher level. They are intertwined in any satisfactory description; and each entails some loss relative to our cognitive preferences, as well as some gain.

By studying the relationship between wholes and parts from the methodological point of view (as overviewed in Chapter 9), current systems thinking goes far beyond thinking molded from either reductionism or holism. We may say that it represents a synthesis of the reductionist thesis and the holistic antithesis, as poetically expressed by Patrick Suppes [1983]:

I am for the delicate dance from parts to wholes
and back again. We should not be captured at
either end. The dance should go forever.

Enough of holism and reductionism. Let me turn to other developments during the first half of the 20th century that contributed to systems thinking and influenced thus the emergence of systems movement. One of them was the increasing awareness that there were many phenomena and problems that could not be studied within the boundaries of individual disciplines of science. This eventually led to the emergence of interdisciplinary areas, such as biophysics, biochemistry, physiological psychology, and social psychology. The existence of these interdisciplinary areas was probably the first step leading to the recognition that systems may be defined across disciplinary boundaries. We may say that it was the first step in recognizing the notion of systemhood. Another step was the recognition of isomorphisms (often called analogies) between systems describing different physical phenomena, such as mechanical, electrical, acoustic, and thermal. Once an isomorphic (relation preserving) correspondence was established between two or more areas of physics, any method developed in one area became readily applicable to corresponding problems in the other areas.

The discovery of isomorphisms between different areas of physics contributed to a new way of thinking about systems. Systemhood similarities became more and more recognized as at least equally important as thinghood differences. Moreover,
the discovered isomorphisms were not only of theoretical significance, but also of
great practical value. They made it possible, for example, to transfer methods from
a methodologically well-developed area to areas methodologically less developed.
A visible result of this possibility was the notion of generalized circuits, a framew
work within which well-developed methods for analyzing electric circuits were
transferred through established isomorphisms to less developed areas of mechanica
al, acoustic, magnetic, and thermal systems [Thorn, 1963].

The established isomorphisms made it also possible to study systems indi
rectly, in terms of other systems, isomorphic to them. For example, experimental
investigations of dynamic properties of new designs of automobiles, aircraft,
helicopters, or rockets may be done in wind tunnels on models of these man-made
objects, appropriately scaled, rather than on the objects themselves. This is often
more convenient, cheaper, and safer. The experiments may also be performed on
objects that belong to a different area of physics, say on electric circuits whose
dynamic properties are known to be isomorphic with those of the investigated
objects. This may even be more convenient and less costly than the construction
and use of scale models.

Basic concepts, principles, and methods regarding the utilization of isomor
phisms for dealing with practical problems, mostly in engineering, were eventually
incorporated into a new subject area referred to as the theory of similarity or
similitude [Skoglund, 1967; Szucz, 1980]. This theory also deals with the construc
tion and use of analog computers. These are devices whose basic units are physical
models of some mathematical operations (addition, multiplication, integration, etc.)
and functions (exponential, trigonometric, etc.). They are used for solving algebraic
or differential equations. When appropriate units of an analog computer are con
nected according to the form of a mathematical equation, the solution is obtained
by measuring relevant physical quantities.

The ideas of holism, the emergence of interdisciplinary areas in science, and
the increasing recognition of the existence and utility of isomorphisms between
disciplines of science created a growing awareness among some scholars that
certain concepts, ideas, principles, and methods were applicable to systems in
general, regardless of their disciplinary categorization. This eventually led to the
notions of general systems, general theory of systems, general systems research,
and the like.

There seems to be no doubt that the terms general systems and general systems
research (or general systems theory) are due to Ludwig von Bertalanffy. Although
he introduced them orally in the 1930s, the first written presentation appeared only
after World War II; they are included, together with some of his later writings, in
one of his books [Bertalanffy, 1968]. According to Bertalanffy, general systems
research is a discipline whose subject matter is “the formulation and derivation of
those principles which are valid for ‘systems’ in general.”
Bertalanffy was not only the originator of the idea of general systems research, but also one of the principal organizers of systems movement that sprang from this idea. In 1954, he and three other distinguished scholars with similar systems ideas, Kenneth Boulding (an economist), Ralph Gerard (a physiologist), and Anatol Rapoport (a mathematical biologist), happened to spend some time together as Fellows at the just founded Center for Advanced Study in the Behavioral Sciences in Palo Alto, California. These four scholars, who are often referred to as the founding fathers of systems movement, apparently influenced each other in a highly synergetic fashion. This synergy led to the formation of the first organization fully devoted to the promotion of systems thinking, the Society for General Systems Research (SGSR), in December 1954. The Society was founded with the following four objectives:

1. To investigate the isomorphy of concepts, laws, and models from various fields, and to help in useful transfers from one field to another;
2. To encourage development of adequate theoretical models in fields which lack them;
3. To minimize the duplication of theoretical effort in different fields; and
4. To promote the unity of science through improving communication among specialists.

These objectives are as meaningful now as they were when the Society was founded. Their spirit is well captured in the following extract from a classic paper by Boulding [1956]:

General Systems Theory is a name which has come into use to describe a level of theoretical model-building which lies somewhere between the highly generalized constructions of pure mathematics and the specific theories of the specialized disciplines . . .

Because in a sense mathematics contains all theories it contains none; it is the language of theory, but it does not give us the content. At the other extreme, we have the separate disciplines and sciences with their separate bodies of theory. Each discipline corresponds to a certain segment of the empirical world, and each develops theories which have particular applicability to its own empirical segment. Physics, Chemistry, Biology, Psychology, Economics and so on all carve out for themselves certain elements of the experience of men and develop theories and patterns of activity (research) which yield satisfaction in understanding, and which are appropriate to their special segments.

In recent years increasing need has been for a body of systematic theoretical construction which will discuss the general relationships of the empirical world. This is the quest of General Systems Theory. It does not seek, of course, to establish a single, self-contained “general theory of practically everything” which will replace all special theories of particular disciplines. Such a theory would be almost without content, for we always pay for generality by sacrifice-
ing content, and all we can say about practically everything is almost nothing. Somewhere however between the specific that has no meaning and the general that has no content there must be, for each purpose and at each level of abstraction, an optimum degree of generality.

Over the years, the Society for General Systems Research has become the main professional organization supporting all facets of the emerging systems movement. Each year, the Society organizes an Annual Meeting. In 1988, the name of the Society was changed to the International Society for the Systems Sciences (ISSS), reflecting perhaps the maturity of the field.

3.3. Other Relevant Developments

Ideas quite similar to those associated with general systems research, although more focusing on information processes in systems such as communication and control, were proposed in the late 1940s under the name cybernetics. This name was coined by the promoter of these ideas, Norbert Wiener; it is based on the Greek word kybernetes, which means steerman. In his seminal book, Wiener [1948] defines cybernetics as the study of "control and communication in the animal and in the machine."

To capture the essence of Wiener's motivation to introduce this new field, I can hardly do better than use his own words [Wiener, 1948]:

Since Leibniz there has perhaps been no man who has had a full command of all the intellectual activity of his day. Since that time, science has been increasingly the task of specialists, in fields which show a tendency to grow progressively narrower. . . . Today there are few scholars who can call themselves mathematicians or physicists or biologists without restriction. A man may be a topologist or an acoustician or a coleopterist. He will be filled with jargon of his field, and will know all its literature and all its ramifications, but, more frequently than not, he will regard the next subject as something belonging to his colleague three doors down the corridor, and will consider any interest in it on his own part as an unwarrantable breach of privacy . . . There are fields of scientific work, which have been explored from the different sides of pure mathematics, statistics, electrical engineering, and neurophysiology; in which every single notion receives a separate name from each group, and in which important work has been triplicated or quadruplicated, while still other important work is delayed by the unavailability in one field of results that may have already become classical in the next field.

It is these boundary regions of science which offer the richest opportunities to the qualified investigator. They are at the same time the most refractory to the accepted techniques of mass attack and the division of labor. If the difficulty of a physiological problem is mathematical in essence, ten physiologists ignorant of mathematics will get precisely as far as one physiologist
ignorant of mathematics, and no further. If a physiologist who knows no mathematics works together with a mathematician who knows no physiology, the one will be unable to state his problem in terms that the other can manipulate, and the second will be unable to put the answers in any form that the first can understand. . . . A proper exploration of these blank spaces on the map of science could only be made by a team of scientists, each a specialist in his own field but each possessing a thoroughly sound and trained acquaintance with the fields of his neighbors; all in the habit of working together, of knowing one another’s intellectual customs, and of recognizing the significance of a colleague’s new formal expression. The mathematician need not have the skill to conduct a physiological experiment, but he must have the skill to understand one, to criticize one, and to suggest one. The physiologist need not be able to prove a certain mathematical theorem, but he must be able to grasp its physiological significance and to tell the mathematician for what he should look. We have dreamed for years of an institution of independent scientists, working together in one of these backwoods of science, not as subordinates of some great executive officer, but joined by the desire, indeed by the spiritual necessity, to understand the region as a whole, and to lend one another the strength of that understanding.

Cybernetics is based upon the recognition that certain information-related problems, such as some problems of communication and control, can be meaningfully and beneficially studied; to some extent, independently of any specific context. When Wiener proposed cybernetics, the circumstances were favorable. Communication theory and control theory made tremendous progress during World War II, and a new field of information-processing machines (computers) was just emerging. These developments resulted in a rich body of knowledge bound together by the notion of information. Claude Shannon [1948] showed how to measure information for the purposes of telecommunication and control, and established some basic laws of information that govern systems; control theory provided practitioners with a respectable inventory of rigorously formulated concepts, such as stability, positive and negative feedback, observability, controllability, and feedforward and feedback control, as well as with principles and methods for analyzing and designing control systems of various types; and the emerging area of information-processing machines began to develop basic ideas regarding the design of general-purpose computing machines, which involved, for example, the issues of physical encoding of information, processing of higher types of information in terms of elementary logical operations, controlling the computation process, and the like.

During World War II, Wiener worked, as a mathematician, on various war-related engineering projects, in which aspects of communication, control, and information processing were predominant. At the same time, he was attracted to certain issues of neurophysiology and, eventually, he became involved in some neurological projects jointly with a Mexican neurophysiolo-
gist Arthur Rosenblueth. He discovered that communication, control, and information processing were also fundamental in this area. This apparently helped him to recognize the cross-disciplinary nature of problems connected with information and led eventually to his formulation of cybernetics.

Important early contributors to cybernetics include Warren McCulloch [1965], a founder of neural nets; Stafford Beer [1966, 1979, 1981], who introduced cybernetic principles and systems thinking into the area of management; and Gordon Pask [1975, 1976], who made important connections of cybernetics with cognitive science.

General systems research and cybernetics have developed side by side since their emergence, and a considerable cross-fertilization has occurred between them. Perhaps the most important person in this cross-fertilization was W. Ross Ashby, whose profound contributions to various issues of cybernetics were consistently formulated and dealt with as systems problems.* There are different opinions about the relationship between general systems research and cybernetics. In one opinion, which seems to have become predominant within systems movement, cybernetics is a subarea of general systems research that focuses on the study of information processes in systems, particularly communication and control. I fully share this opinion [Klir, 1970] since I consider all properties and problems connected with the notion of information as fundamentally systemhood properties and problems. Indeed, I cannot conceive of the possibility to conceptualize information without any reference to a system of some sort.

Another major factor in the formation of systems movement can best be captured by the term mathematical systems theory. This term, in fact, represents a broad variety of mathematical theories (emerging mostly in the 1960s) which attempt to formalize fundamental systems concepts and develop a formal framework for formalizing and dealing with systems problems. These general theories of systems evolved, by and large, from various special systems theories, which in the late 1950s and early 1960s were already mathematically well developed for specific purposes within the engineering milieu. Most visible of these special

*According to a survey regarding influences among systems researchers, whose results are published in Appendix B in Klir [1978]. W. Ross Ashby was by far the most influential person in systems movement. The survey showed that Ashby had a major influence on almost twice as many systems researchers as the second most influential person, Ludwig von Bertalanffy, and almost three times as many systems researchers as the third most influential persons, Norbert Wiener and Anatol Rapoport. The great impact of Ashby’s work on systems movement can be explained, at least partially, by the superior clarity of his writings, his unusual capability to recognize important principles where others saw only trivialities, his great gift for essence-preserving simplification, his broad interests, encompassing both cybernetics and general systems research, and his meticulous scholarship. He wrote two book masterpieces [Ashby, 1952, 1959] and many influential articles, most of which are included in a book [Ashby, 1981] edited by Conant.
theories were the mathematical theories of control, electric and generalized circuits, switching circuits, and finite-state automata."

One of the most comprehensive mathematical systems theories was initiated and its foundations developed by Mihajlo Mesarovic. The significance of this theory is that it is based on the common-sense conception of systems. That is, the theory begins with only one axiom by which the concept of a system is formulated at the most general level: a system is a family of sets and a relation defined on the sets. Additional axioms are then added to formalize pragmatically significant special classes of systems. The most comprehensive exposition of the theory is given in two books by Mesarovic and Takahara [1975, 1988].

The emergence of systems movement was also connected with some developments in the areas of engineering and management in the 20th century. Throughout the century, it became increasingly important in both of these areas to think in terms of systems. Engineering was challenged with designing systems of rapidly increasing complexity, from telephone networks through production automation to the design of computers and computer-based systems. The situation in management was quite similar; the challenge came from the increasingly more complex organizations to be managed and from the increasing complexity of the associated decision making.

The developments in engineering and management were somewhat connected. Clearly, the increasing complexity of engineering tasks made increasing demands on management. These demands increased drastically during World War II, when complex military problems entered into the realm of management. Since critical strategic and tactical decisions had to be made quickly, multidisciplinary teams of scientists, mathematicians, engineers, and managers were formed to analyze the issues involved, such as optimal scheduling and resource allocation, cost–benefit and risk analysis, planning, budgeting, and the like. An outgrowth of these activities was a new discipline for which the name operations research was coined. In general,

*One major predecessor of modern mathematical systems theories was the theory of feedback control. Although various devices based upon feedback control have presumably been built since the third century B.C. [Mayr, 1970], conscious efforts to develop a theory of systems with feedback control began only in the 19th century. These efforts were stimulated primarily by the success of the centrifugal governor invented around 1790 by James Watt to control the speed of the steam engine [Bennett, 1979]. The theory was already well developed in the late 1930s and further extended and perfected during World War II. Methods of analysis and design of electric circuits, which were essential in the development of modern control theory, also played an important role in the formation of more general mathematical systems theories [Zadeh, 1962].

Another major predecessor of modern mathematical systems theories was the theory of finite-state automata. This theory, which emerged after World War II, was primarily motivated by problems associated with the design of digital computers and some questions connected with the idea of artificial intelligence. A comprehensive coverage of automata theory was prepared by Booth [1967]. An offspring of automata theory, which is more intimately connected with the design issues, is the theory of switching circuits [Kleis, 1972b].
operations research can be characterized as the study of possible activities or operations within a particular institutional and organizational framework (e.g., a firm, a military organization, or a government) for the purpose of determining an optimum plan for reaching a given goal.

The main methodological resources of operations research are the various optimization methods for single as well as multiple objective criteria, decision-making methods, and methods derived from game theory.

After World War II, some new disciplines evolved from operations research. Two of them, with the strongest connection to systems movement, are referred to as systems analysis and systems engineering. The aim of systems analysis is to use systems thinking and methodology (including methodological tools inherited from operations research) for analyzing complex problem situations that arise in private and public enterprises and organizations as a support for policy and decision making [Miser and Quade, 1985]. Systems engineering, on the other hand, is oriented to planning, design, construction, evaluation, and maintenance of large-scale systems that may involve both machines and human beings [Flagle et al., 1960].

This concludes a summary of the most visible developments that contributed to the emergence and evolution of systems movement. It is concisely captured in Fig. 3.2. Although numerous other developments could be justifiably mentioned in this regard, it is not the purpose of this text to trace the history of systems ideas as completely as possible."

What is the current status of systems movement? After the Society for General Systems Research was formed in 1954, other professional societies oriented to systems research or cybernetics were formed in different countries. Perhaps the most visible, active, and stable have been the American Society for Cybernetics, the Austrian Society for Cybernetic Studies, and the Netherlands Society for Systems Research. Since 1980, systems movement has been united under the auspices of the International Federation for Systems Research (IFSR). The aims of the Federation, which was officially incorporated in Austria on March 12, 1980, are "to stimulate all activities associated with the scientific study of systems and to coordinate such activities at the international level."

Let me only mention one additional development of considerable historical significance. It is now increasingly recognized that an important early precursor of general systems theory was a theory developed by A. Bogdanov (1873–1928), a prominent Russian thinker, at the beginning of the 20th century under the names tekology (inspired by the Greek word tekton, which means "a builder") or general organizational science. Ideas pertaining to tekology were published in different forms between 1912 and 1928. Unfortunately, they sank into oblivion, primarily to their rejection by Lenin and their suppression by Soviet authorities. As a consequence, one of Bogdanov's books on tekology became available to the English-speaking world only some 60 years after its original publication [Bogdanov, 1980]. An excellent exposition of the development of systems thinking in the Soviet Union, which elaborates on the significance of Bogdanov's ideas, was prepared by Susichko [1982].
Systems movement is now supported not only organizationally through the IF SR and its member societies, but also by a respectable number of scholarly journals and other publications. Furthermore, there are some academic programs in systems science, systems engineering, cybernetics, and related areas that are already well established and stable, and additional programs seem to emerge at a steady pace.

3.4. Two-Dimensional Science

To understand properly the significance of systems movement, we need to examine its role in the profound societal changes that took place in the 20th century. This is the purpose of this section.

It is now well recognized that a number of countries, primarily the United States and other countries in the West, were after World War II at some unique historical crossroads of great significance. This crossroads is usually described as a transition from an industrial into postindustrial phase of society. It is compared in its significance with the previous major societal transition—the change from the pre-industrial into industrial society, which occurred in most Western countries in the 19th and early 20th centuries.
Some of the main characteristics of the preindustrial society are: the labor force is overwhelmingly involved in the extractive industries such as agriculture, mining, forestry, or fishing; the main resources are raw materials and natural power as human or animal muscles, wind, or water; life is basically a game against nature. Characteristics of the industrial society are radically different: the major part of the labor force is involved in goods-production based on machine technology; the main resources are the financial capital and created energy such as electricity, coal, oil, gas, or nuclear energy; life is primarily a game against fabricated nature [Bell, 1973].

The term postindustrial society was originally used for a society which is primarily involved in services such as transportation, utilities, trade, finance, healthcare, education, arts, research, government, recreation, and others. This view was based on the observation of the societal trends in the United States after World War II, where for the first time more than 50% of the labor force became engaged in the production of various services. It was later recognized that the real increase in the so-called service sector has been in the various information-related occupations (programmers, systems analysts, educators, accountants, managers, secretaries, etc.). Hence, the term postindustrial society was gradually replaced with the more descriptive term information society.

One of the major characteristics of each of the three levels of societies—the preindustrial, industrial, and information societies—is its intellectual base of technology. The preindustrial society is characterized by common sense, the method of trial and error, craft skills, and the emphasis on tradition. The industrial society is primarily characterized by machine technology based on advanced disciplines of science and their engineering counterparts. It is important to realize that science in industrial society is basically one-dimensional in the sense that its various disciplines and specializations emerge primarily due to differences in experimental (instrumentation) procedures rather than differences in the relational properties of the investigated systems.

The information society is clearly characterized by the emergence of the computer (information) technology and a new dimension in science. According to this new dimension, which is referred to in this paper as systems science, systems are recognized and classified by their relational properties rather than the kind of things that form the relations. Such an alternative point of view, which emerged from systems movement, transcends the artificial boundaries between the experimentally based science and makes it possible to develop a genuine cross-disciplinary methodology, more adequate for dealing with the large-scale societal problems inherent in the information society.

In summary, the role of science in the three types of societies can be described as follows: the preindustrial society is basically prescientific; the industrial society is associated with one-dimensional science, which is essentially experimentally based; the information society is characterized by the emergence of a new dimension in science, the theoretically based science or systems science, and its integra-
tion with the experimentally based science. Science in the information society can thus be described as a *two-dimensional science*.

**Exercises**

3.1. Consider the ternary relation among variables CH1, CH2, and CPU that is discussed in Sec. 3.2. Show that there are 35 ternary relations that are not distinguishable in terms of the associated binary relation.

3.2. Assume that the ternary relation

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was obtained by monitoring variables CH1, CH2, and CPU instead of the one discussed in Sec. 3.2. Determine the number of ternary relations that are not distinguishable in this case in terms of:

(a) the three associated binary relations;
(b) each pair of these binary relations.

3.3. Give examples of typical problems for the three problem areas introduced by Warren Weaver: organized simplicity, disorganized complexity, and organized complexity.

3.4. Explain differences between deterministic and nondeterministic systems and illustrate by specific examples these two types of systems.
CHAPTER 8

Complexity

Complexity is a paradoxical newcomer to the history of science. By a twist of semantic perversity, in proposing an intelligence of complexity, we look first for support from the complexity of intelligence.
—JEAN-LOUIS LE MOIGNE

8.1. What Is Complexity?

Complexity is perhaps as important a concept for systems science as the concept of a system. It is a difficult concept, primarily because it has many possible meanings. While various specific meanings of complexity have been proposed and discussed on many occasions, there is virtually no sufficiently comprehensive study that attempts to capture its general characteristics. The reason for this situation is well expressed by John Casti [1986]:

The notion of system complexity is much like St. Augustine's description of time: "What then is time [complexity]? If no one asks me, I know; if I wish to explain it to one that asks, I know not." There seems to be fairly well-developed intuitive ideas about what constitutes a complex system, but attempts to axiomatize and formalize this sense of the complex all leave a vague, uneasy feeling of basic incompleteness, and a sense of failure to grasp important aspects of the essential nature of the problem.

In line with the purpose of this book, an attempt is made in this chapter to capture the essence of complexity, rather than to discuss its various narrow, technical meanings. My primary aim is to show that the concept of complexity has many faces while, at the same time, it is associated with some general properties that remain invariant when one face is replaced with another.

To begin with a broad perspective, let us consult a common dictionary first; we find that complexity is "the quality or state of being complex," i.e.,

- "Having many varied interrelated parts, patterns, or elements and consequently hard to understand fully"; or
- "marked by an involvement of many parts, aspects, details, notions, and necessitating earnest study or examination to understand or cope with" (Webster's Third International Dictionary).
This common-sense characterization of complexity does not contain any qualification regarding the kind of entities to which it is applicable. As such, it can be applied to virtually any kinds of entities, material or abstract, natural or man-made, products of art or science. Regardless of what it is that is actually considered as being complex or simple, its degree of complexity is, according to the common-sense characterization, associated with the number of recognized parts as well as the extent of their interrelationship; in addition, complexity is given a somewhat subjective connotation since it is related to the ability to understand or cope with the thing under consideration.

We can see that the common-sense characterization of complexity assumes an interaction between the object (a part of the world that may have “many varied interrelated parts . . .”) and a human being (or, perhaps, a computer) for whom it may be difficult “to understand or cope with” the object. This means that the complexity of an object for a particular human being depends on the way he or she interacts with it (i.e., on his or her interests and capabilities). More poetically, we may say that the complexity of an object is in the eyes of the observer.

In most cases, there is virtually an unlimited number of ways one can interact with an object. As a consequence, the interaction is almost never complete. It is based on a limited (and, usually, rather small) number of attributes that the observer is capable of distinguishing on the object and that are relevant to his interests. These attributes are not available to the observer directly, but only in terms of their abstract images, which are results of perception or some specific measurement procedures. These abstract images are usually called variables. When a set of variables is established as a result of our interaction with an object of interest, we say that a system (or, more precisely, a source system) is distinguished on the object.

Since we deal with systems distinguished on objects and not with the objects themselves (in their totality), it is not operationally meaningful to view complexity as an intrinsic property of objects. This does not mean, however, that I deny the existence of complexity of objects in the ontological sense. I only recognize that the notion of object complexity is epistemologically and methodologically vacuous, in contrast to the notion of systems complexity. This point is well expressed by Ashby in one of his last writings [Ashby, 1973]:

The word “complex,” as it may be applied to systems, has many possible meanings, and I must first make my use of it clear. There is no obvious or preeminent meaning, for although all would agree that the brain is complex and a bicycle simple, one has also to remember that to a butcher the brain of a sheep is simple while a bicycle, if studied exhaustively (as the only clue to a crime) may present a very great quantity of significant detail.

Without further justification, I shall follow, in this paper, an interpretation of “complexity” that I have used and found suitable for about ten years. I shall measure the degree of “complexity” by the quantity of information required to describe the vital system. To the neurophysiologist the brain, as a feltwork of
fibers and a soup of enzymes, is certainly complex; and equally the transmission of a detailed description of it would require much time. To a butcher the brain is simple, for he has to distinguish it from only about thirty other "meats," so not more than log_2 30, i.e., about 5 bits, are involved. This method admittedly makes a system's complexity purely relative to a given observer; it rejects the attempt to measure an absolute, or intrinsic, complexity; but this acceptance of complexity as something in the eye of the beholder is, in my opinion, the only workable way of measuring complexity.

Others have expressed their views on this important issue differently, though in the same spirit. The following two imaginative quotes should reinforce the point I want to make here:

We can only hope for explicit models of the world and not for reality itself or even a small part of it [Suppes, 1977].

One of the functions of the experimental method is to substitute simple artificial systems for the complex systems that Nature presents to us [Simon, 1977a].

Complexity (in the epistemological and methodological sense) is thus associated with systems, that is, some abstractions distinguished on objects that reflect the way in which the objects are interacted with. Systems, however, have many different faces, each represented by one of the epistemological categories of systems and, possibly, by some methodological distinctions within the category (Chap. 4). As a consequence, the concept of complexity, when applied to these various systems categories, has many different faces as well. That is, different types of systems give the concept of complexity different meanings, each of which requires a special treatment.

8.2. Complexity and Information

Notwithstanding the differences in complexities of the various systems types, two general principles of systems complexity can be recognized; they are applicable to any of the systems types and can thus be utilized as guidelines for a comprehensive study of systems complexity.

According to the first general principle, the complexity of a system (of any type) should be proportional to the amount of information required to describe the system. Here, the term "information" is used solely in a syntactic sense; no semantic or pragmatic aspects of information are employed. One way of expressing this descriptive complexity, perhaps the simplest one, is to measure it by the number of entities involved in the system (variables, states, components) and the variety of relationship among the entities. Indeed, everything else being the same, our ability to understand or cope with a system tends to decrease when the number of entities involved or the variety of their relationship increase. There are, of course, many
different ways in which descriptive complexity can be measured even within a particular category of systems. However, each way of measuring complexity must satisfy at least the following general requirements, which are rather obvious on intuitive grounds:

1. The complexity should be expressed in terms of nonnegative real numbers.
2. If system A is a strong homomorphic image of system B, then the complexity of A should not be greater than the complexity of B.
3. If systems A and B are isomorphic, then their complexities should be equal.
4. If system C consists of two subsystems, A and B, that do not interact with each other and neither is a homomorphic image of the other one, then the complexity of C should be equal to the sum of the complexities of A and B.

It seems that any additional requirements can be introduced only in the context of specific categories of systems.

Descriptive complexity can also be characterized in a universal way, independent of the nature of systems to which it is applied. In this sense, descriptive complexity of a system (of any type) is defined to be the size of the shortest description of the system in some standard language or, alternatively, the size of the smallest program in a standard language by which the system can be simulated on a canonical universal computer. The primary advantage of this definition of descriptive complexity is that it is theoretically sound and applicable to all systems, regardless of their classification. Its primary weakness is methodological: it is rather difficult to determine in many cases the shortest description of a system.

According to the second general principle, systems complexity should be proportional to the amount of information needed to resolve any uncertainty associated with the system involved (predictive, retrodictive, prescriptive). Here, again, syntactic information is used, but information that is based on a measure of uncertainty.

Uncertainty, which is an inherent property of every nondeterministic system, is now well understood in several mathematical frameworks. It was first conceived in terms of set theory by Hartley [1928]. He derived a simple class of functions,

\[ I(A) = K \log_b |A|, \]

as the only meaningful functions by which to measure the amount of information needed to resolve the uncertainty associated with |A| alternatives that are left undecided (or to characterize one particular element of the set A); |A| denotes the cardinality of a finite set A and \( K > 0, b > 1 \) are constants that distinguish individual functions in this class. Later, the uniqueness of this class of functions to measure uncertainty in set-theoretic terms was proven axiomatically by Rényi [1970]. A choice of one of these functions, by choosing some particular values of K and b,
determines basically the unit by which uncertainty is measured. An intuitively appealing unit of uncertainty (and the associated information) results in \( I(A) = 1 \) when the set \( A \) contains 2 elements. In this case, the *unit of uncertainty* expresses the total ignorance regarding the truth or falsity of one proposition; it was given the name *bit*, which is an abbreviation of "binary digit" (values 0 and 1 of a binary digit are often used for encoding truth values of a proposition). The Hartley measure of uncertainty in bits is thus expressed by the unique function

\[
I(A) = \log_2 |A|.
\]  
(8.1)

Another classical mathematical framework for conceptualizing uncertainty is probability theory. A measure of *probabilistic uncertainty* (and the associated information) was established by Shannon [1948]. This measure, whose basic form is

\[
H(p(x) \mid X) = - \sum_{x \in X} p(x) \log_2 p(x),
\]  
(8.2)

where \((p(x) \mid X)\) denotes a probability distribution on a finite set \( X \), is usually called the *Shannon entropy*. It is well justified, in a number of alternative ways, as a unique measure of uncertainty conceptualized in terms of probability theory under the assumption that the units of measurement are bits [Klir and Wierman, 1998; Rényi, 1971; Shannon, 1948]. In practical applications, the Shannon entropy is usually used in a conditional form (based on appropriate conditional probabilities) or in a generalized form that involves two probability distributions.

Since the Shannon entropy plays an important role in systems science, let me overview Shannon entropies of joint, marginal, and conditional probability distributions defined on two sets, \( X \) and \( Y \). In agreement with a common practice in the literature, let me simplify the notation by using \( H(X) \) instead of \( H(p(x) \mid X) \) to denote the simple entropy defined by Eq. (8.2). On two sets, \( X \) and \( Y \), we can recognize three types of entropies:

1. Two *simple entropies* based on the marginal probability distributions,

\[
H(X) = - \sum_{x \in X} p(x) \log_2 p(x),
\]  
(8.2')

\[
H(Y) = - \sum_{y \in Y} p(y) \log_2 p(y).
\]  
(8.3)

2. A joint entropy defined in terms of the joint probability distribution, \( p(x, y) \), on \( X \times Y \),
\[ H(X,Y) = - \sum_{x \in X} \sum_{y \in Y} p(x, y) \log_2 p(x, y). \]

(8.4)

3. Two conditional entropies defined in terms of weighted averages of local conditional entropies as

\[ H(X|Y) = \sum_{y \in Y} p(y) \sum_{x \in X} p(x|y) \log_2 p(x|y), \]

(8.5)

\[ H(Y|X) = - \sum_{x \in X} p(x) \sum_{y \in Y} p(y|x) \log_2 p(y|x). \]

(8.6)

In addition to the simple, joint, and conditional entropies, the function

\[ T(X, Y) = H(X) + H(Y) - H(X, Y) \]

(8.7)

is frequently used in the literature as a measure of the strength of relationship (in the probabilistic sense) between elements of sets \( X \) and \( Y \). This function is called information transmission.

The following are some fundamental properties of the various entropies and the information transmission:

\[ H(X|Y) = H(X,Y) - H(Y), \]

(8.8)

\[ H(Y|X) = H(X,Y) - H(X), \]

(8.9)

\[ T(X,Y) = H(X) - H(X|Y), \]

(8.10)

\[ T(X,Y) = H(Y) - H(Y|X), \]

(8.11)

\[ H(X) - H(Y) = H(X|Y) - H(Y|X), \]

(8.12)

\[ H(X,Y) \leq H(X) + H(Y), \]

(8.13)

\[ H(X) \geq H(X|Y), \]

(8.14)
These properties, as well as their generalizations to more than two sets [Ashby, 1969] can be easily derived from the basic definitions [Klir and Folger, 1985].

Since the mid-1960s, the two classical theories capable of conceptualizing uncertainty, classical set theory and probability theory, have been generalized into fuzzy set theory and fuzzy measure theory. Fuzzy set theory deals with sets whose boundaries are not sharp. That is, the change from nonmembership to membership in a fuzzy set is gradual rather than abrupt. Fuzzy measure theory deals with measures that are not necessarily additive (as probability measures are), but only monotonic with respect to the ordering based upon the relation to be a subset of. Each of these general theories encompasses various special theories (including the two classical theories). Well-justified measures of uncertainty in some of these new theories are now available [Wang and Klir, 1992], but their coverage is beyond the scope of this book.

Studying uncertainty from the broader perspectives of fuzzy set theory and fuzzy measure theory made us aware that more than one type of uncertainty must be distinguished. At this time, we recognize three types of uncertainty, which are distinguished from one another by the suggestive names nonspecificity, dissonance (conflict), and fuzziness (vagueness). Nonspecificity is exemplified by the Hartley measure in classical set theory: the greater the set of alternatives that are left undecided in a situation (e.g., predictions or prescriptions), the less specific the situation is; when only one alternative is possible, the situation is fully specific. Dissonance is exemplified by the Shannon entropy in probability theory: it is required that probabilities be assigned to mutually exclusive alternatives and, hence, they conflict with one another and create dissonance; the greater the lack of discrimination among the probabilities, the greater the dissonance. Fuzziness (or vagueness) of a fuzzy set expresses the lack of distinctions between its members and nonmembers.

While only one type of uncertainty is captured by either of the classical theories (nonspecificity in classical set theory and dissonance in probability theory), some of the broader theories capture more than one type of uncertainty. Fuzzy sets, for example, have not only degrees of fuzziness, but also degrees of nonspecificity: the fuzzy set of all numbers close to zero (defined by an appropriate membership grade function) is certainly less specific than the set of all numbers very close to zero (defined by a comparable membership grade function). Two or three types of uncertainty also coexist in some special theories of fuzzy measures. Both nonspecificity and dissonance appear, for example, in the so-called evidence theory, in which the additivity requirement of probability theory is replaced with two weaker requirements [Klir and Folger, 1988].

Both descriptive complexity and uncertainty-based complexity are connected with information: information needed to describe a system and information needed
to resolve uncertainty embedded in it. These two complexities (and the associated kinds of information) conflict with each other. When we want to reduce one of them, the other is likely to increase or, at best, remain the same. This trade-off is one of the most fundamental methodological issues in systems science.

To capture adequately phenomena within the realm of organized complexity (biological, medical, economic, social, etc.), we need models of great descriptive complexity. When the required complexity for obtaining realistic models becomes unmanageable, we must simplify. In every simplification, unfortunately, we are doomed to lose something. When we insist that the simplified models give us predictions with no uncertainty, we often lose relevance of these predictions to the real world. We can preserve some of this relevance only by allowing some uncertainty in the models. That is, we can trade certainty for relevance under a given limit of acceptable descriptive complexity. Furthermore, as previously explained, there are various types of uncertainty we can utilize in this trade.

The relationship among relevance (or credibility) and the two kinds of complexities of systems models, which is of utmost importance to systems modeling, is not as yet well understood. In general, we try to construct highly relevant (credible) models that are simple (in the descriptive sense) and, if possible, we want to avoid uncertainty. Unfortunately, these objectives conflict with one another in a rather complicated way. Although uncertainty is undesirable when considered alone, it becomes very valuable when considered in connection with descriptive complexity and relevance. It is the only commodity that can be traded for a reduction of complexity of a model, an increase in its relevance, or both. By investigating the broad theories of fuzzy sets and fuzzy measures, we try to extend the scope of this important commodity. This purpose of the broad theories of uncertainty is still not generally understood [Klir, 1989]. It is appropriate to quote in this context Lotfi Zadeh [1973], the founder of the theory of fuzzy sets:

Given the deeply entrenched tradition of scientific thinking which equates the understanding of a phenomenon with the ability to analyze it in quantitative terms, one is certain to strike a dissonant note by questioning the growing tendency to analyze the behavior of humanistic systems as if they were mechanistic systems governed by difference, differential, or integral equations.

Essentially, our contention is that the conventional quantitative techniques of system analysis are intrinsically unsuited for dealing with humanistic systems or, for that matter, any system whose complexity is comparable to that of humanistic systems. The basis for this contention rests on what might be called the principle of incompatibility. Stated informally, the essence of this principle is that as the complexity of a system increases, our ability to make precise and yet significant statements about its behavior diminishes until a threshold is reached beyond which precision and significance (or relevance) become almost mutually exclusive charac-
teristics. It is in this sense that precise quantitative analyses of the behavior of humanistic systems are not likely to have much relevance to the real-world societal, political, economic, and other types of problems which involve humans either as individuals or in groups.

An alternative approach ... is based on the premise that the key elements in human thinking are not numbers, but labels of fuzzy sets, that is, classes of objects in which the transition from membership to nonmembership is gradual rather than abrupt. Indeed, the pervasiveness of fuzziness in human thought processes suggests that much of the logic behind human reasoning is not the traditional two-valued or even multivalued logic, but a logic with fuzzy truths, fuzzy connectives, and fuzzy rules of inference. In our view, it is this fuzzy, and as yet not well-understood, logic that plays a basic role in what may well be one of the most important facets of human thinking, namely, the ability to summarize information—to extract from the collections of masses of data impinging upon the human brain those and only those subcollections which are relevant to the performance of the task at hand.

By its nature, a summary is an approximation to what it summarizes. For many purposes, a very approximate characterization of a collection of data is sufficient because most of the basic tasks performed by humans do not require a high degree of precision in their execution. The human brain takes advantage of this tolerance for imprecision by encoding the “task-relevant” (or “decision-relevant”) information into labels of fuzzy sets which bear an approximate relation to the primary data. In this way, the stream of information reaching the brain via the visual, auditory, tactile, and other senses is eventually reduced to the trickle that is needed to perform a specific task with a minimal degree of precision. Thus, the ability to manipulate fuzzy sets and the consequent summarizing capability constitute one of the most important assets of the human mind as well as a fundamental characteristic that distinguishes human intelligence from the type of machine intelligence that is embodied in present-day digital computers.

Viewed in this perspective, the traditional techniques of system analysis are not well suited for dealing with humanistic systems because they fail to come to grips with the reality of the fuzziness of human thinking and behavior. Thus to deal with systems radically, we need approaches which do not make a fetish of precision, rigor, and mathematical formalism, and which employ instead a methodological framework which is tolerant of imprecision and partial truths.

The two types of complexity introduced thus far, the descriptive complexity and the uncertainty-based complexity, pertain to systems. Yet another face of complexity exists, a complexity that pertains to systems problems. This complexity, which is usually referred to as computational complexity, is a characterization of the time or space (memory) requirements for solving a problem by a particular algorithm. Either of these requirements is usually expressed in terms of a single parameter that represents the size of the problem.
To appreciate the significance of computational complexity for systems methodology, let me first address the question of fundamental computational limits.

8.3. Bremermann's Computational Limit

The following conjecture is the central theme of a paper by Hans Bremermann [1962]:

No data processing system, whether artificial or living, can process more than \(2 \times 10^{37}\) bits per second per gram of its mass.

To process a certain number of bits means in this statement to transmit that many bits over one or several channels within the computing system. Let me overview the arguments by which Bremermann derived this conjecture.

It is obvious that information that is to be acted upon by a machine must be physically encoded in some manner. Assume that it is encoded in terms of energy levels within the interval \([0,E]\) of energy of some sort; \(E\) is viewed as the total energy available for this purpose. Assume further that energy levels can be measured with an accuracy of only \(\Delta E\). Then, the most refined encoding is defined in terms of markers by which the whole interval is divided into \(N = E/\Delta E\) equal subintervals, each associated with the energy amount \(\Delta E\). If at each instant no more than one of the levels (represented by the markers) is occupied, then

\[
\log_2(N + 1)
\]

is the maximum number of bits that are representable by energy \(E\); \(N + 1\) is used here to account for the case in which none of the levels is occupied. If, instead of one marker with energy levels in \([0,E]\), \(K\) markers \((2 \leq K \leq N)\) are used simultaneously, then

\[
K \log_2 \left(1 + \frac{N}{K}\right)
\]

bits become representable. The optimal utilization of the available amount of energy \(E\) is obtained when \(N\) markers with levels in the interval \([0,\Delta E]\) are used. In this optimal case, \(N\) bits of information can be represented.

In order to represent more information by the same amount of energy, it is desirable to reduce \(\Delta E\). This is possible only to a certain extent since the resulting levels must be distinguished by some measurement process which, regardless of its nature, always has some limited precision. The extreme case is expressed by the Heisenberg principle of uncertainty: energy can be measured to the accuracy of \(\Delta E\) if the inequality

\[
\Delta E \Delta t \geq \hbar
\]
is satisfied, where $\Delta t$ denotes the time duration of the measurement, $h = 6.625 \times 10^{-27}$ ergs/sec is Planck's constant, and $\Delta E$ is defined as the mean deviation from the expected value of the energy involved. This means that

$$N \leq \frac{E \Delta t}{h}.$$

Now, the available energy $E$ can be expressed in terms of the equivalent amount of mass $m$ by Einstein’s formula

$$E = mc^2,$$

where $c = 3 \times 10^{10}$ cm/sec is the velocity of light in a vacuum. If we take the upper (most optimistic) bound of $N$ in the inequality, we get

$$N = \frac{mc^2 \Delta t}{h}.$$

Substituting the numerical values for $c$ and $h$, we obtain

$$N = 1.36m\Delta t \times 10^{47}.$$

For a mass of 1 g ($m = 1$) and time of 1 sec ($\Delta t = 1$), we obtain the value

$$N = 1.36 \times 10^{47},$$

which implies the conjecture.

Using the limit of information processing obtained for one gram of mass and one second of processing time, Bremermann then calculates the total number of bits processed by a hypothetical computer the size of the Earth within a time period equal to the estimated age of the Earth. Since the mass and age of the Earth are estimated to be less than $6 \times 10^{27}$ grams and $10^{10}$ years, respectively, and each year contains approximately $3.14 \times 10^9$ seconds, this imaginary computer would not be able to process more than $2.56 \times 10^{92}$ bits or, when rounding up to the nearest power of ten, $10^{93}$ bits. The last number—$10^{93}$—is usually referred to as Bremermann’s limit and problems that require processing more than $10^{93}$ bits of information are called transcomputational problems.

Bremermann’s limit seems at first sight rather discouraging for system problem solving, even though it is quite conservative (more reasonable assumptions would lead to a number smaller than $10^{93}$). Indeed, many problems dealing with systems of even modest size exceed it in their information-processing demands. Consider, for example, a system of $n$ variables, each of which can take $k$ different states. The set of all overall states of the variables consists clearly of $k^n$ states. In each particular
system, however, the actual overall states are restricted to a subset of this set. There are \(2^k\) such subsets. Suppose we need to select, identify, distinguish, or classify one system from the set of all possible systems of this sort. Then, under the assumption that the most efficient method of searching is used, in which each bit of information (the answer to a dichotomous question) allows us to cut the remaining choices in half,

\[
\log_2 2^k = n
\]

bits of information have to be processed. The problem becomes transcomputational when

\[
k^n > 10^{93}.
\]

That happens, e.g., for the following values of \(k\) and \(n\):

<table>
<thead>
<tr>
<th>(k)</th>
<th>2</th>
<th>3</th>
<th>4</th>
<th>5</th>
<th>6</th>
<th>7</th>
<th>8</th>
<th>9</th>
<th>10</th>
</tr>
</thead>
<tbody>
<tr>
<td>(n)</td>
<td>308</td>
<td>194</td>
<td>154</td>
<td>133</td>
<td>119</td>
<td>110</td>
<td>102</td>
<td>97</td>
<td>93</td>
</tr>
</tbody>
</table>

The problem of transcomputationality arises in various contexts. One of them is pattern recognition. Consider, for example, a \(q \times q\) spatial array of the chessboard type, each square of which can have one of \(k\) colors. There are clearly \(k^n\) color patterns, where \(n = q^2\). Suppose we want to determine the best classification (according to certain criteria) of these patterns. This requires a search through all possible classifications of the patterns. In the case of only two classes, the problem becomes isomorphic to the previous one. For two colors, for example, the problem becomes transcomputational when the array is \(18 \times 18\); for a \(10 \times 10\) array, the problem becomes transcomputational when nine colors are used. This pattern recognition problem is directly relevant to physiological studies of the retina, but its complexity is tremendous. The retina contains about a million light-sensitive cells. Even if we consider (for simplicity) that each of the cells has only two states (active and inactive), the attempt to study the retina as a whole would require the processing of

\[
21,000,000 \equiv 10^{300,000}
\]

bits of information. This is far beyond Bremermann’s limit.

Another context in which the same problem occurs is the area of testing large-scale integrated digital circuits. These are tiny electronic chips with considerable complexity and a large number of inputs and outputs. For properly defined electric signals (each, usually, with two ideal states), the individual outputs should represent some specific logic functions of the logic variables associated with the inputs. To test a particular integrated circuit means to analyze it as a “black box”: 
to determine the actual logic functions it implements, solely by manipulating the input variables and observing the output variables. For each output variable, the testing problem is thus basically the same as the problem previously discussed for $k = 2$ (unless a multiple-valued logic is used). It follows that testing of circuits, for example, with 308 inputs and one output is a transcomputational problem. However, it is well known that the practical complexity limits of this testing problem are considerably lower. Some currently manufactured large-scale integrated circuits cannot be in fact completely tested. The focus is thus on developing testing methods that can be practically implemented and guarantee only that the testing be almost complete, that say, well over 90% of all possibilities be tested.

A more detailed characterization of the complexity of this problem, from the practical domain to Bremermann’s limit, is expressed by Fig. 8.1. The figure shows the dependence of the time (in years) required to select (identify, classify, distinguish, etc.) one logic function of $n$ variables under the consideration of different information-processing rates in the range from 10 through $10^{100}$ bits per second. Two significant values of time are also shown in the figure: $L$ indicates the approximate age of the oldest fossil records of life on the Earth; $M$ shows the approximate time since men first appeared on the Earth.

The testing example is in no way exceptional. Genuine systems problems are notorious for their huge demands on information-processing capabilities. This point is illustrated by specific examples on various occasions elsewhere in this book. It is also well depicted by Bremermann [1962] in the conclusion of his paper:

**Figure 8.1.** Time required to select or identify one logic function of $n$ variables for information processing rates of 10, $10^{10}$, ..., $10^{100}$ bits per second.
The experience of various groups who work on problem solving, theorem proving and pattern recognition all seem to point in the same direction: These problems are tough. There does not seem to be a royal road or a simple method which at one stroke will solve all our problems. My discussion of ultimate limitations on the speed and amount of data processing may be summarized like this: Problems involving vast numbers of possibilities will not be solved by sheer data processing quantity. We must look for quality, for refinements, for tricks, for every ingenuity that we can think of. Computers faster than those of today will be a great help. We will need them. However, when we are concerned with problems in principle, present-day computers are about as fast as they ever will be.

If a problem is transcomputational, it is obvious that it can be dealt with only in some modified form. It is desirable to modify it no more than is necessary to make it manageable. The most natural way of modifying a problem is to soften its requirements. For instance, a requirement of getting the best solution may be replaced with a requirement of getting a good solution, instead of requiring a precise solution we may accept an approximate solution, and so on. Such softening of requirements permits the use of heuristic methods, in which vast numbers of unpromising possibilities are ignored, or approximate (fuzzy) methods, in which substantial aggregation takes place.

Bremermann’s limit allows us to make only the most rudimentary categorization of systems problems by their complexities. It does not say anything about the actual practical computational limits. Nevertheless, it is a useful benchmark for a preliminary evaluation of each problem situation, as emphasized by Ashby [1973]:

One of its most obvious consequences, yet one almost universally neglected today, is that, before the study of a complex system is undertaken, at least a rough estimate of its informational demands should be made. Should the estimate be 2000 bits we have little to worry about, but should it prove to be $10^{300}$ bits we would know that our whole strategic approach to the system needs re-formulating.

For purposes of practical problem solving, this simple benchmark—$10^{93}$—must be supplemented, of course, by sharper bounds on problem complexity. Its principal significance is epistemological: it is an indicator of fundamental limits to our knowledge, as explained by Ashby [1968]:

The most obvious fact is that we, and our brains, are themselves made of matter, and are thus absolutely subject to the limit. Not only are we subject as individuals, but the whole cooperative organization of World Science is also made of matter, and is therefore subject to it. Thus both the total information that I can use personally, and the information that World Science can use, are limited, on any ordinary scale, to about $10^{80}$ bits. Whatever our science will become in the future, all will lie below this ceiling.

We cannot claim any special advantage because of our preeminent position
in the world of organisms. We have been shaped, and selected to be what we are, by the process of natural selection. As a selection, this process can be measured by an information-measure; it is therefore subject to its limits. In any type of selection, under any planetary conditions, a planetary surface made of matter cannot produce adaptation faster than the rate of the limit. However good we may think we are, \(10^{80}\) measures something that we do not exceed. The science of the future will be built by brains that cannot have had more than \(10^{50}\) bits used in their preparations, and they themselves will advance only by something short of \(10^{50}\). This is our informational universe: what lies beyond is unknowable.

[Note: Ashby derives the value of \(10^{80}\) from the Bremermann limit for one second and one gram by considering “centuries of time and tons of computers” (e.g., about ten thousand centuries and \(10^{15}\) tons of mass). It is obviously not important for the argument whether we take \(10^{80}\) or \(10^{93}\).]

Bremermann’s limit works well, as a simple benchmark, for problems whose information-processing demands exceed it, but it does not say much about the remaining problems. Even if a problem is not rejected by Bremermann’s limit, it may still be practically intractable. A more refined understanding of the notion of computational complexity is thus needed.

8.4. Computational Complexity

Computational properties of problems are studied under the general theory of algorithms. This general theory includes three large subject areas: the theory of computability, design of algorithms, and the theory of computational complexity. It is beyond the scope of this book to cover these areas in any depth. It is desirable, however, to provide the reader with a brief overview, focusing primarily on computational complexity, of those results and issues that are of particular significance to systems methodology. No proofs of the summarized results are presented here.¹

An algorithm is understood intuitively as a set of instructions, expressed in some language, for executing a sequence of operations for solving a problem of some specific type. Algorithms are required to be finite, i.e., each algorithm must terminate after a finite number of steps (operations) have been executed.

The intuitive notion of an algorithm was formalized in several ways, including formalizations based on the concepts of Turing machines, Markov algorithms, and

¹Computational complexity has been extensively investigated since the early 1970s. A good overview of the main issues regarding computational complexity and results available in the late 1970s was prepared by Garey and Johnson [1979]. An excellent coverage of computational complexity is in the book of Harel [1987], which is self-contained and contains extensive bibliographical notes on the subject.
recursive functions, which were all proven to be equivalent. One of the concepts—that of a Turing machine—is envisioned as a simple device that consists of a finite-state control unit and a tape. The control unit has a memory, which makes it capable of being in any one of a finite set of states, say set $Z = \{z_1, z_2, \ldots, z_n\}$. The tape is potentially infinite in both directions, and is marked off along its length into spaces of equal size. Each of these spaces, referred to as cells, has written on it a symbol from a finite set of symbols, say set $X = \{x_0, x_1, \ldots, x_m\}$. One of the symbols, say symbol $x_0$, is always interpreted as a blank space (empty cell). Communication between the control unit and tape is provided by a read-write head, which is capable of reading symbols from the tape and writing over the symbols that are written on it. Only one cell of the tape is accessible to the head at any time.

The control unit of a Turing machine operates in discrete steps. In each step it replaces the current state with a new one, and performs a single operation of one of the following three types:

1. It replaces the current symbol on the tape with a new one;
2. It moves the tape by one cell to the left or right;
3. It stops the computation (the so-called halt operation).

The new state as well as the operation performed are uniquely determined by the current state and the symbol read on the tape.

Let $z_0$, $z_1$ denote the current and next state of a Turing machine, respectively, let $x_j$ denote the symbol that is read on the tape, and let $y_j$ denote the operation performed. Then, given an initial string of symbols on the tape (any cell for which a symbol is not given is assumed to be blank) and a particular initial state a computation on the Turing machine is defined by an ordered set of quadruples

$$(z_0, x_j, z_1, y_j).$$

If no two quadruples in the set are allowed to begin with the same pair $z_0, x_j$, the Turing machine is said to be deterministic; otherwise, it is said to be nondeterministic.

A hypothesis that has become known as Church's thesis (or the Church–Turing thesis), and which has been generally accepted, states that any function regarded naturally as computable can be computed on a deterministic Turing machine. According to this hypothesis, a Turing machine is taken to be a precise formal equivalent of the intuitive notion of an algorithm. The hypothesis cannot be proven mathematically, but it is well justified by informal arguments and empirical evidence. It can be overthrown only by proposing an alternative formalization of computation, generally acceptable on intuitive grounds and capable of describing computations that are beyond the capabilities of Turing machines. The existence of such a formalization is considered highly unlikely.
In general, a problem is considered unsolvable if no algorithm exists by means of which a solution can be obtained. The notion of deterministic Turing machines together with Church's thesis have made possible the study of the existence of algorithms for various problems in a formal manner. To prove that a problem is unsolvable, it is sufficient to prove that it cannot be solved by a Turing machine. Such proofs of unsolvability have been obtained for a number of problems.

Unsolvable problems form one of three primary classes of problems. The second class consists of problems that have not been proven unsolvable, but for which no algorithms are known for solving them. These are thus problems whose solvability status has not been resolved as yet.

All remaining problems are solvable. That is, they are solvable in principle. In practice, however, many of them cannot be solved due to their excessive demands on computing resources such as computing time and memory size. Since the required computing time is usually the single factor that determines whether or not a problem is practically solvable, computational complexity has been predominantly studied in terms of this single resource.

The practical solvability of a problem depends on

1. The algorithm employed for solving the problem;
2. The size of the particular systems involved in the problem;
3. The computational power of the computing resources available.

Given a particular algorithm for solving a problem, it is convenient to express its time requirements in terms of a single variable that represents the size of the systems involved in the problem. This variable, which is often called the size of a problem instance, is supposed to express the amount of input data needed to describe the particular systems.

Given a particular systems problem instance, let \( n \) denote its size. Then, the time requirements of a specific algorithm for solving the problem are expressed by a function

\[ f: \mathbb{R} \to \mathbb{R} \]

such that \( f(n) \) is the largest amount of time needed by the algorithm to solve a problem instance of size \( n \). Function \( f \) is usually called a time complexity function.

It has been recognized that it is useful to distinguish two classes of algorithms by the rate of growth of their time complexity functions. One class consists of algorithms whose time complexity functions can be expressed in terms of a polynomial. They are called polynomial time algorithms. Since the degree of each polynomial is considerably more significant, especially for large values of \( n \), than its coefficients and lower-order terms, it is useful to classify polynomial time complexity functions by their order. A function \( f \) is said to be of complexity \( O(n^k) \), where \( k \) is a positive integer, if and only if there is a constant \( c > 0 \) such that
\[ f(n) \leq cn^k \]

for all \( n \geq n_0 \), where \( n_0 \) is a positive integer that usually represents the smallest size of the problem instances involved. For example, function

\[ f(n) = 25n^2 + 18n + 31 \]

is of complexity \( O(n^2) \) since

\[ f(n) \leq 74n^2 \]

when \( n_0 = 1 \), or

\[ f(n) \leq 42n^2 \]

when \( n_0 = 2 \), etc.

The second class of algorithms consists of those whose time complexity functions are not bounded by complexity \( O(n^k) \) for some \( k \). They are usually referred to as exponential time algorithms.

The distinction between the polynomial and exponential time algorithms is significant, especially when considering large problem instances. This is illustrated in Table 8.1 by showing differences in growth rates for several time complexity functions. The computing times in this table are based on the assumption that the computing is performed at a rate of one million operations per second. When comparing, for instance, \( n^2 \) with \( n^{10} \), we can see that the degree of a polynomial time complexity function has a considerable effect on practical limitations of the corresponding algorithms. However, polynomial time algorithms are substantially more responsive than exponential time algorithms to increases in computing power (except for small values of \( n \)). This can be seen by comparing plots of some polynomial and exponential time complexity functions in Fig. 8.2 and, even more explicitly, by examining the actual increases in the ranges of applicability due to increases in computing speed, as illustrated by the formulas in Table 8.2.

Because of the essential differences between polynomial and exponential time complexity functions, polynomial time algorithms are considered efficient, while exponential time algorithms are considered inefficient. As a consequence, problems for which it can be proven that they are not solvable by polynomial time algorithms are viewed as intractable, while problems for which polynomial time algorithms are known are viewed as tractable. The latter problems are usually called P-problems (i.e., solvable in polynomial time); the set of all such problems is called the problem class \( P \).

It is known that differences among standard schemes used in practice for encoding problems as well as differences in the computer types used do not affect the classification of problems into tractable and intractable. Standard encoding
schemes and computer types are known to differ from each other at most polynomially. Alternative encoding schemes or computer types may thus influence the practical range of solvability of a problem, but they do not affect its tractability status.

It turns out that for most of the problems encountered in practice, neither is a polynomial time algorithm known to solve them, nor have they been proven intractable. A common trait of such problems is that they can be "solved" in polynomial time by nondeterministic computers such as nondeterministic Turing machines. Such problems are called NP-problems (nondeterministic polynomial time problems) and form a set called the problem class NP. The term “solve” is used here in the sense that if the machine guesses the solution, it can verify its correctness in polynomial time. The notion of a nondeterministic polynomial time algorithm is thus used solely as a convenient definitional device for capturing the notion of polynomial time verifiability of a proposed (guessed) solution of the

Table 8.1. Illustration of Growth Rates of Several Polynomial and Exponential Time Complexity Functions

<table>
<thead>
<tr>
<th>Time complexity function</th>
<th>1</th>
<th>10</th>
<th>20</th>
<th>30</th>
<th>40</th>
<th>50</th>
<th>100</th>
</tr>
</thead>
<tbody>
<tr>
<td>(n)</td>
<td>0.000001</td>
<td>0.00001</td>
<td>0.00002</td>
<td>0.00003</td>
<td>0.00004</td>
<td>0.00005</td>
<td>0.0001</td>
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<td>sec</td>
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<td>sec</td>
<td>sec</td>
<td>sec</td>
<td>sec</td>
<td>sec</td>
<td>sec</td>
</tr>
<tr>
<td>(n^2)</td>
<td>0.000001</td>
<td>0.0001</td>
<td>0.00004</td>
<td>0.00009</td>
<td>0.0016</td>
<td>0.0025</td>
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<tr>
<td>(n^3)</td>
<td>0.000001</td>
<td>0.1</td>
<td>3.2</td>
<td>24.3</td>
<td>1.7</td>
<td>5.2</td>
<td>2.8</td>
</tr>
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<td>sec</td>
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<tr>
<td>(n^{10})</td>
<td>0.000001</td>
<td>2.8</td>
<td>118.5</td>
<td>18.7</td>
<td>3.3</td>
<td>31.0</td>
<td>3.2 \times 10^{4}</td>
</tr>
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<td>(2^n)</td>
<td>0.000002</td>
<td>0.001</td>
<td>1.0</td>
<td>17.9</td>
<td>12.7</td>
<td>35.7</td>
<td>4 \times 10^{4}</td>
</tr>
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<td>sec</td>
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<td>centuries</td>
</tr>
<tr>
<td>(3^n)</td>
<td>0.000003</td>
<td>0.059</td>
<td>58</td>
<td>6.5</td>
<td>3.855</td>
<td>2 \times 10^{8}</td>
<td>1.6 \times 10^{32}</td>
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<tr>
<td>(10^n)</td>
<td>0.000001</td>
<td>2.8</td>
<td>(3.2 \times 10^6)</td>
<td>(3.2 \times 10^{14})</td>
<td>(3.2 \times 10^{34})</td>
<td>(3.2 \times 10^{44})</td>
<td>(3.2 \times 10^{54})</td>
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<td>(2^{2n})</td>
<td>0.000004</td>
<td>(5.7 \times 10^{292})</td>
<td>(10^{30})</td>
<td>(10^{31})</td>
<td>(10^{32})</td>
<td>(10^{33})</td>
<td>(10^{34})</td>
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<tr>
<td>(n^n)</td>
<td>0.000001</td>
<td>2.8</td>
<td>(3.3 \times 10^9)</td>
<td>(6.5 \times 10^{38})</td>
<td>(3.8 \times 10^{68})</td>
<td>(-2.8 \times 10^{99})</td>
<td>(-3.2 \times 10^{184})</td>
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<td>centuries</td>
<td>centuries</td>
<td>centuries</td>
</tr>
<tr>
<td>(n!)</td>
<td>0.000001</td>
<td>3.6</td>
<td>771.5</td>
<td>(8.4 \times 10^{16})</td>
<td>(2.6 \times 10^{32})</td>
<td>(-9.6 \times 10^{46})</td>
<td>(-2.9 \times 10^{142})</td>
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actual problem. It is known that any NP-problem can be solved by a deterministic algorithm with time complexity $O(2^{p(n)})$, where $p$ is a polynomial function.

The class NP contains the class P because any problem that is solvable in polynomial time on a deterministic Turing machine is also solvable (i.e., verifiable) in polynomial time on a nondeterministic Turing machine. A considerable number of NP-problems have been proven to have the property that every other NP-problem can be converted to them in polynomial time. Such problems are distinguished as NP-complete problems.

Since the class NP consists of many practically important problems, it is highly desirable to resolve its status. The question of whether or not NP-problems are intractable is therefore one of the most important questions in mathematics, computer science, and systems science. Its implications for systems problem
Complexity

Table 8.2. Effects of Increases in Computing Speed on Problem-Solving Capabilities for Some Time Complexity Problems

<table>
<thead>
<tr>
<th>Time complexity function</th>
<th>Current computer technology</th>
<th>Technology hundred times faster</th>
<th>Technology thousand times faster</th>
<th>Technology million times faster</th>
<th>Technology X times faster</th>
</tr>
</thead>
<tbody>
<tr>
<td>$n$</td>
<td>$n_1$</td>
<td>$100n_1$</td>
<td>$1,000n_1$</td>
<td>$1,000,000n_1$</td>
<td>$Xn_1$</td>
</tr>
<tr>
<td>$n^2$</td>
<td>$n_2$</td>
<td>$10n_2$</td>
<td>$31.6n_2$</td>
<td>$1,000n_2$</td>
<td>$\sqrt{X}n_2$</td>
</tr>
<tr>
<td>$n^3$</td>
<td>$n_3$</td>
<td>$2.5n_3$</td>
<td>$3.98n_3$</td>
<td>$15.8n_3$</td>
<td>$\frac{5}{X}n_3$</td>
</tr>
<tr>
<td>$n^{10}$</td>
<td>$n_4$</td>
<td>$1.58n_4$</td>
<td>$2n_4$</td>
<td>$3.98n_4$</td>
<td>$10^{\sqrt{X}n_4}$</td>
</tr>
<tr>
<td>$2^n$</td>
<td>$n_5$</td>
<td>$n_5 + 6.64$</td>
<td>$n_5 + 9.97$</td>
<td>$n_5 + 19.93$</td>
<td>$n_5 + \log X/\log 2$</td>
</tr>
<tr>
<td>$y^n$</td>
<td>$n_6$</td>
<td>$n_6 + 4.19$</td>
<td>$n_6 + 6.29$</td>
<td>$n_6 + 12.58$</td>
<td>$n_6 + \log X/\log 3$</td>
</tr>
<tr>
<td>$10^n$</td>
<td>$n_7$</td>
<td>$n_7 + 2$</td>
<td>$n_7 + 3$</td>
<td>$n_7 + 6$</td>
<td>$n_7 + \log X/\log 10$</td>
</tr>
</tbody>
</table>

Solving is quite profound. The question is often stated in the form “is NP = P?” It can be answered by proving for any of the NP-complete problems that it is either a P problem (i.e., solvable in polynomial time) or a problem inherently intractable (i.e., solvable only in exponential time). If any one of the NP-complete problems is proven intractable, then NP ≠ P. If, on the other hand, such a problem is proven tractable, then NP = P. Since there are strong indications that NP ≠ P under the usual rules of inference, the question becomes primarily one of discovering some unorthodox rules of inference under which any one of the NP-complete problems could be proven tractable. The significance of this problem (NP = P?) was recognized when the Clay Mathematics Institute decided to consider it the first problem among seven “Millennium Prize Problems.” For solving any of these problems, the Institute announced (at a meeting on May 24, 2000) a prize of one million dollars.

The classification of problems from the standpoint of their solvability and computational complexity is summarized in Fig. 8.3. The class denoted as coNP consists of problems that are complementary to the NP-problems in the sense that their answers are complements of the answers obtained for the corresponding NP-problems. It is not known whether NP = coNP, but it is known that the intersection NP ∩ coNP is not empty and contains all P-problems as well as some other problems.

Although computational complexity has been predominantly studied in terms of the time it takes to perform a computation, the amount of computer memory required is frequently just as important. This requirement is usually referred to as the space requirement. It can be studied in terms of a space complexity function, analogous to the time complexity function. It is known, however, that any problem solvable in polynomial time can be solved in polynomial space as well. Indeed, the number of cells operated on by the read-write head of a Turing machine in a
particular computation (which represents the space requirement) cannot exceed the number of steps involved in the computation (which represents the time requirement). It is not certain, however, whether all problems that are solvable in polynomial space are solvable in polynomial time. It is for this reason that the time complexity is used to classify problems as either tractable or intractable. In practice, however, both of these requirements are equally important.

From a broader, more realistic perspective, the size of a problem instance is not the only determinant of its computational complexity. That is, problem instances of the same type and size may have very different computational demands. Most studies in the area of computational complexity are oriented primarily to the characterization of the worst-case problem instances. Although this orientation is theoretically sound, it usually results in estimates that are rarely reached in practice and are therefore too pessimistic. To ameliorate this situation, the worse-case estimates are sometimes supplemented with average-case estimates. However, such estimates are based on the assumption that all problem instances are equally likely, which does not necessarily reflect the actual probability distribution of problem instances encountered in practice. The problem of determining the actual distribution for various problem types is predominantly an empirical problem. This problem can be studied, in principle, by monitoring and analyzing problem in-
stances requested by users of the various systems problem packages. Any such study
is an example of a metamethodological inquiry (Chap. 6).

Exercises

8.1. Define a reasonable measure of descriptive complexity for systems of the type
exemplified in Fig. 5.3. Apply the measure to the systems shown in the figure, and
check if it satisfies the four general requirements for measures of descriptive com-
plexity.

8.2. Assume that system S_1 in Fig. 5.3 consists of states a, b, c, d and the diagram represents
its discrete-time state-transition relation. Using the Hartley measure of uncertainty
given by Eq.(8.1) and assuming that the system is at time t in state a, calculate the
uncertainty in predicting:
(a) the next state (state at time t + 1);
(b) the state at time t + k (k = 2, 3, \ldots);
(c) the sequence of states at time t + 1, t + 2, \ldots, t + k (k = 2, 3, \ldots).

Repeat the calculations under the assumption that the system at time t is either in
state b or c or d.

8.3. Consider a system with four states (denoted by the integers 1, 2, 3, 4) whose transitions
from present states to next states are characterized by the following state-transition
matrix of conditional probabilities:

<table>
<thead>
<tr>
<th>Present state</th>
<th>Next state</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>1  2  3  4</td>
</tr>
<tr>
<td>1</td>
<td>.2 0 .8 0</td>
</tr>
<tr>
<td>2</td>
<td>0 0 0 1</td>
</tr>
<tr>
<td>3</td>
<td>0 .9 0 .1</td>
</tr>
<tr>
<td>4</td>
<td>.5 .3 .2 0</td>
</tr>
</tbody>
</table>

Assuming that the state (at time t = 1) is state 1, determine the following:
(a) uncertainties in predicting states at times t = 2, 3, 4;
(b) uncertainty in predicting sequences of states of lengths 2, 3, 4.

Repeat (a) and (b) under the assumption that the state at the time t is either in state 2,
or state 3, or state 4.

8.4. Consider a set containing n elements (n \geq 2). Suppose we need to identify one element
from its power set. For which value of n does the searching become a transcompu-
tational problem?

8.5. Consider five problems whose computational complexities are n^4, 4^n, 3^{2n}, 3^n, and
3^2. For each of these functions, determine the value of n for which the associated
problem becomes transcomputational.
CHAPTER 11

Systems Science in Retrospect and Prospect

Knowledge grows by accretion, but we gain truth by pruning the tree of knowledge.
—KENNETH E. BOULDING

No historical reflection upon systems science and its impact on other areas of human endeavor can be definitive at this time since systems science is currently still in the process of forming. It is by far not established as yet to a degree comparable with traditional disciplines of science such as, e.g., physics, chemistry, psychology, or economics. One of the difficulties in examining systems science in this formative stage is the lack of unified terminology. Thus, the very notion of systems science, as conceived in this book, is often discussed in the literature under the names systems research or systems theory, sometimes with the adjective general.

Since the emergence of systems movement in the 1940s and 1950s, the progress in forming systems science as a new field of inquiry has been quite impressive. During this period, various aspects of the developing systems science have been subjected to criticism. Some of this criticism was justified, focusing mainly on exaggerated claims of certain systems ideas, and influenced the formation of systems science in a positive way. Other criticism was ill-conceived, sometimes quite vicious, and has been refuted. Let me overview some of the main arguments involved.

11.1. Criticism

One of the earliest criticisms of general systems theory, pursued vigorously by R. C. Buck, a mathematician, was concerned with the concept of a system. It focused on the following definition proposed by James Miller [1953]:

A system is a bounded region in space-time, in which the component parts are associated in functional relationship.

The essence of Buck's criticism is summarized in this quote [Buck, 1956]:

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

This claim of logical emptiness of general systems theory was later echoed by other critics. Although it seemed to present general systems theory with a serious challenge, the claim is not sustainable upon closer scrutiny. First, it applies only to a common-sense conception of systems. That is, it does not apply, for example, to the GSPPS framework (outlined in Chap. 4), within which a potentially infinite number of epistemologically distinct categories of systems are defined. Moreover, Miller's definition, by its reference to "a bounded region in space-time," is not in line with the modern, constructivist view, which is becoming predominant in systems science. It seems that it characterizes an object of the real world, in which a functional relationship among components is assumed on ontological grounds, rather than a system constructed on the object on pragmatic grounds.

Regardless of these broader considerations, Buck's claim can be refuted even in the specific context of the common-sense definition of systems. This was achieved, for example, by J. H. Marchal through his careful analysis of the common-sense definition of the concept of a system [Marchal, 1974]. Let me use a relevant quote from his paper, where the term "the definition" stands for "S is a system only if S is a set of elements and relations between the elements":

Turning to Buck's objection we see that it is really twofold: his claim is that we are unable to think of (a) anything or (b) any combination of things which cannot be regarded as a system. With the definition in hand and the twofold nature of Buck's objection made explicit, it will be seen that the objection is neither accurate nor decisive.

First, what we are (un)able to think of in a situation such as this is relevant only if it produces a counterexample. That is, what might be of interest is an example that satisfies the explication but fails to satisfy our preanalytic intuitions about systems, or, alternately, something that we took preanalytically to be a system which fails to satisfy the explication. Short of this, alluding to what we are (un)able to think of is simply quite unhelpful.

Second, I think that we can describe things that fail to satisfy the explication: thus showing that the definition does not lead to the kind of vacuity indicated in Buck's objection. For example, the piece of paper on which I am writing is in no obvious way either a thing (or combination of things) which is a set of elements and relations among the elements. So both (a) and (b) of Buck's objection miss their mark. [However, the retort might be, the piece of paper can be so regarded (represented, construed, viewed, seen)! And just what does this mean? Possibly it means that under some reasonable description the piece of paper can be seen to satisfy (a) or (b) and thus the explication. Well and good: let us have the description, or a method for routinely generating such descriptions, and then decide if we have a counterexample.]
Third, it should be pointed out that the presupposition that great generality or even "total universality” results in vacuity is mistaken; it is not at all clear that a very general concept such as that of a system is, because of its generality, logically empty (whatever that might mean here). The claim, were it made, that everything is countable would not lead us to say that counting (or the concept of countability) is vacuous. Or, again, the claim that everything can be construed as a set or as a member of some set would not lead us to reject the concept of a set as vacuous. Quite to the contrary, sometimes the ubiquitous applicability of such procedures and concepts is a mark of their usefulness, if not of their indispensability.

I think that the analogy with set theory is illuminating. It is worth noting that the concept of a set is itself intelligible, by and large, only in the context of some set theory. Within such a theory questions about sets can be raised and answered, while the employment of the concept outside the theory, in some "ordinary” fashion, leads, as we know, to infamous paradoxes. And, of course, the general question of what can be construed as a set is not answered by the theory (any more than the question what English sentences can be construed as formulae is answered by some predicate logic). I think that the concept of a system would also benefit from a more formal, theoretical, explication. The move to a formal theory of systems is not, as was in set theory, generated out of "contradictory” intuitions about systems; rather, it comes from the seeming dead end reached in nontheoretical discussion and the expressed dissatisfaction with informal analyses. If a formal theory could be developed (and just as with set theory, there might well be more than one viable alternative), then questions about systems could be raised and answered in a way that might avoid the difficulties found in the informal employment of the concept. At the same time, the ordinary-discourse ubiquity of the concept as characterized is to be preserved, for it is just this which makes its theoretical development inviting, not vacuous.

Except for the paper by Buck, systems movement has been virtually free of criticism throughout the 1950s and 1960s. Then, in the 1970s, it suddenly became a fashionable target of critics of all trades. The most visible critiques of various aspects of systems movement from this period are contained in three books written by Ida R. Hoos [1972], David Berlinski [1976], and Robert Lilienfeld [1978]. These books, by and large, focus their criticisms on some early claims and expectations of systems research, often overly enthusiastic and exaggerated, and on some ill-conceived applications of systems methodology, particularly in the social sciences.

The critique by Hoos is the most specific of the three. Her target is not systems science, as conceived in this book, but systems analysis or systems approach (she uses the two terms interchangeably), which she views as offsprings of operations research. The following is her own characterization of the book:
The research reported in this book represents a critical investigation of the state-of-the-art of systems analysis. The technique is examined in theory and practice, in its own circumscribed, structured, and simulated world and in the real world, where solutions must face pragmatic test and not merely satisfy an abstract set of conditions. The assumptions implicit in the approach as it developed over time and their validity as a basis for social planning are analyzed. They are examined in the context of the specific areas in which systems analyses are being applied, viz., education, crime, health, welfare, land use, transportation, and pollution of land, sea, and air. The experience with information systems as components of these areas and as entities in themselves are examined in detail. The process, procedures, and products of systems analysis are analyzed for the social, cultural, political, and economic factors that influence the adoption of this problem-solving technique in its various forms, notably cost–benefit ratios and planning–programming–budgeting.

Using numerous case studies, Hoos criticizes some exaggerated and highly over-sold claims of systems analysis as a tool for making public policy. Her critique is, with some exceptions, sound and appeared at the right time.

Berlinski’s critique is aimed at a larger target, including not only systems analysis, but also general systems theory and mathematical system theory, particularly the area of dynamical systems formulated in terms of differential equations. His criticism is more vigorous, but, unfortunately, far less substantiated than the one by Hoos. Moreover, it contains a fair number of obvious errors or misunderstandings. Berlinski focuses primarily on the misuse of mathematics by some systems researchers or, in his own words, on “the use of mathematical methods for largely ceremonial reasons.” The main point of his criticism is that it is not warranted to transfer mathematical methods (he almost exclusively refers to methods based on ordinary differential equations) from physics, where they work well, into the realm of social sciences or even biological sciences. In his own words:

Ultimately, I see my essay as a fragment of a larger and philosophically more stimulating mass dealing with the question of the extent to which minds constituted roughly as ours are constituted may hope to achieve an adequate understanding of social, political, and biological life. There is no obvious reason why the interiors of certain distant stars should be the subject of deep physical theories while the gross features of ordinary experience remain, by the standards of theoretical physics or pure mathematics, inaccessible to sophisticated speculation. Systems analysis I take to be a bouncy and outrageous attempt to pretend that such speculation, and the understanding that it sometimes prompts, is in fact within our grasp; to show that this is not so is not a very difficult task, but it is one that ought to be performed.

The criticism regarding the issue of transferring mathematical method from physics to biological or social sciences, which Berlinski illustrates primarily by the industrial, urban, and world dynamics of Jay W. Forrester [1961, 1968, 1969, 1971,
1975], is well taken. However, this criticism is applicable only to some activities within the systems movement (as well as some activities outside it) and it is by no means justified to extend it to the whole movement, as Berlinski seems to imply.

The book by Lilienfeld [1978], which is the most comprehensive of the three, was written as a critique of systems theory (or systems thinking—the author uses these terms interchangeably), particularly its societal claims. It consists of three parts. In the first part, which consumes approximately one half of the book, the author overviews historical roots and major characteristics of systems theory. He describes relevant views of Ludwig von Bertalanffy, Norbert Wiener’s formulation of cybernetics, ideas developed by W. Ross Ashby, Shannon’s information theory, operations research, systems analysis, artificial intelligence, and aspects of systems theory that developed within economics (input–output theory, game theory, decision theory).

The second part of the book is devoted to the societal claims of some systems thinkers. The author focuses mainly on the writings by Ervin Laszlo, Walter Buckley, Karl Deutsch, and David Easton.

In the third part, Lilienfeld tries to portray systems theory (or systems thinking) as an ideology of new scientific and technocratic elites. Similarly as in the book by Berlinski, considerable attention is given in this context to the work of Jay Forrester and to his association with the Club of Rome.

Although the overview part of the book is excellent (even though its scope is limited), the critical part is less commendable. For one thing, the criticism is restricted only to the ideas and claims of some representative pioneers of the systems movement, while it virtually ignores the mainstream of systems research at that time. Moreover, the criticism has a distinctly emotional flavor, exhibiting strong, almost religious, antisystem and, indeed, even antiscientific sentiments.

In spite of its shortcomings, the book by Lilienfeld contains some points of critique that are well taken. They include the criticism of “the passion for quantification” and exaggerated societal and philosophical claims of some systems theorists, empty importation of systems terminology into various traditional disciplines, and the lack of efforts to justify various simplifying assumptions by most systems analysts.

The best part of the book is, in my opinion, the critique of systems analysis, which is partially based on the previous critique of Ida Hoos. I fully agree with the criticism of so-called systems analysts who claim mastery in any discipline and ability to solve any problem (depending on the availability of funds). However, I consider it absurd to put these people in one bag with serious systems researchers and to claim, as the author does in the last part of the book, that all these people form a new social elite through their enormous influence on politicians, administrators, and businessmen.

Although published in the 1970s, the books by Hoos, Berlinski, and Lilienfeld are still the most challenging critiques of various aspects of the systems movement.
To see them in a proper historical perspective, let me quote from a short paper by Ervin Laszlo [1980]:

Publications such as those discussed here may represent points along a curve that marks the trajectory of a theory innovation. When a new theory or conceptual mode of thought appears, it is almost totally ignored by the adherents of the established paradigm (or paradigms). As it wins adherents here and there, then more eyebrows are raised in the circles of the establishment; some definitions or findings attributed to the challenger become known (falsely, as often as not). There are occasional references to it in books and journals. In time, it becomes acceptable to include a criticism of it in one's writings, and may even become fashionable to do so. The day is not far away when it becomes good academic politics to show one's sophistication by producing a full-fledged critique of the challenger. At first, these are somewhat condescending—after all, one does not want to commit the mistake of taking a new theory too seriously, lest one be misunderstood by one's colleagues. The purpose is to expose the sins and cradities of the challenger, making fun of it in an erudite manner. Whether the critic has truly understood and properly represented the criticized theory is of small moment, since most of his colleagues are certain neither to know nor to care.

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

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

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

After reading the three critiques, an uninitiated reader is likely to get the impression that systems thinking and methodology are fully embraced and well
supported by the government and business alike. This, unfortunately, is not the case, with the exception of some methods that evolved from operations research and are subsumed now under the fashionable term "systems analysis." These methods, as properly argued by the critics, have often been grossly oversold. Genuine systems science, on the other hand, has received only modest recognition and support thus far, and its impact on other areas, which has a great potential, has by far not been fully realized as yet. Let me elaborate a little more on these issues.

11.2. Status and Impact of Systems Science

Systems science, by its very nature, contributes a new perspective to science that is complementary to the perspective of traditional science. There are some important factors that favor the development of systems science, but there are also some factors that tend to block it. Let me extensively quote from a paper by Laszlo (1975), in which these positive and negative factors are well overviewed (although Laszlo refers to general systems theory, using the terminology common in the 1970s, his observations are equally relevant, with a few exceptions, under current situation to systems science):

Factors favoring the development of general systems theory operate both internally and externally to science. There is an intrinsic trend within science itself to maximize the scope of theories consistently with their precision. There are also extrinsic pressures on science to overcome traditional boundaries in producing multidisciplinary theories applicable to societal problems.

Modern science has made great progress by adopting the analytical method of identifying and, if possible, isolating the phenomena to be investigated. If effective isolation is not feasible, e.g., in the life and social sciences, it is replaced by the theoretical device of averaging the values of inputs and outputs to the investigated object, and varying the quantities with the needs of the experiment. Thus influences from what has often been disparagingly called the rest of the world can be disregarded. It appears, however, that the rest of the world is an important factor in many areas of investigation. The consequences of disregarding it are not immediately evident for a good detailed knowledge of the immediate phenomena in a short time-range can nevertheless be won. But the spin-offs, or side effects, of the phenomena will be incalculable, and such effects are not the secondary phenomena they were taken to be in the past. They are the results of the complex strands of interdependence which traverse all realms of empirical investigation but which science's analytical method selectively filters out. Hence, we get much detailed knowledge of local phenomena, and a great deal of ignorance of the interconnections between such phenomena. The analytical method produced the explosion of contemporary scientific information, and the dearth of applicable scientific knowledge. It has also engendered wasteful parallelisms in research due to
failures in the transfer of models and data between disciplines. . . . Scientists value theory refinement as well as theory extension, although they do so to differing degrees. The routine experimentalist, mainly involved with puzzles that can be solved through a suitable application of existing theories and techniques, tends to disparage the philosophizing of colleagues bent on the revision and refinement of the theories themselves. But scientists who perceive internal inconsistencies in their frameworks of explanation are greatly concerned with overcoming them through the creation of new, more general postulates, embracing existing theories as special cases, or reinterpreting them in the light of new axioms. Although the emphasis changes from person to person, from scientific community to scientific community, and from period to period, depending on the problems encountered in the given field, it remains true that, on the whole, the progress of science involves the integration of loosely joined, lower level concepts and hypotheses in mathematically formulated general theories. . . .

The historical trend in modern science is to counterbalance segmentation and specialization in patterns of research and experimentation. . . . In almost every case concrete societal problems call for interdisciplinary research and the integration of hitherto separately investigated variables. . . . Disciplinary compartmentalization is useful only if it is coupled with transdisciplinary integration. . . .

Intrinsic trends to balance fragmentation in mathematically elegant general theories, and extrinsic trends to overcome the limitations in the application of fragmented knowledge are factors which favor the evolution of any theory of integrative potential. They favor the development and acceptance of general system theory inasmuch as that theory is specifically designed to integrate theories of different fields of science, and to make possible the societal application of the integrated scientific knowledge. But general system theory is not thereby automatically elected as the paradigm of contemporary general theory in science. There is a great deal of skepticism that focuses on general theories as such, and on a general system theory especially.

The factors that block the progress on general system theory are due partly to intellectual and organizational inertia, and partly to confusion and suspicions centering on the general system concept itself.

Every theory innovation faces resistance due to intellectual inertia; the tendency of persons trained to work with earlier theories to fail to perceive, or perceive and fail to take seriously or perceive, take seriously, but feel threatened by, the innovation. Such factors of intellectual inertia have created resistance for the acceptance of general theories within individual disciplines—they were experienced by Maxwell, Lavoisier, Pareto, Parsons, Skinner, to mention but a few. . . .

Resistance to theories moving across disciplinary boundaries is stronger than resistance within the disciplines. It is due to several additional factors, including indifference and fear. A scientist confronted with a theory that did not originate in his field and is not confined to it can shrug off its meaning; it
does not concern him in his professional capacity. Thus an ecologist taking a systems approach to information and energy flows in the ecologies he studies may disavow interest in and responsibility for a general theory of systems which would apply, in addition to ecologies, to economics or politics. Mutatis mutandis, with other scientists working with systems concepts in any of the sciences of complexity.

The other generic blocking factor is fear. Specialists rely on knowing more about their specialty than anyone else. They may not know, or even care, about theories and phenomena not directly connected with their specialties, as long as they feel assured that they are masters in their own corner of the scientific edifice. It is unsettling to them to find that some general theorists claim to know their field, and indeed offer interpretations of their findings with which they themselves are not familiar. Instead of welcoming such interests from other scientists and seeking to strengthen the linkages of their specialty with other fields, they feel threatened and tend to block overtures for collaboration. These are psychosociological factors which operate in science no less than in other organizations. General system theory is fully exposed to them. Persistent neglect in some quarters and isolation in others manifest their effects.

Western science, while decentralized in its administrative structure, is also more difficult to move from its present tracks. These are deeply disciplinary in nature. Monies and prestige are vested in academic departments, and the departmental structure of colleges and universities is almost exclusively disciplinary. There have been experiments with multidisciplinary departments, but outside of colleges of general studies, few have managed to survive.

Left to themselves, departments in general show unwillingness to stake their precious resources on new ventures leading beyond the known disciplinary boundaries. Administrators charged with curriculum tasks likewise show unease and unwillingness when faced with multidisciplinary proposals. While agreeing on the need for such programs, they are, for the most part, unfamiliar with the conceptual content of the required offerings. What, for example, are general systems? How much can one say about them? It would seem to many that, as soon as one goes into detail, one is constrained to speak about and conduct research on some special kind of system. What then is the point of institutionalizing a program based on general systems?

Furthermore, who is qualified to teach or investigate general systems? Those who claim such qualification include engineers, life scientists, social scientists, and philosophers. But are the engineers not merely talking about artificial systems, the life scientists about the living systems, the social scientists about social systems, and the philosophers about conceptual systems? If so, they could well pursue their investigations and teaching programs within their existing departments.

The term general system theory is subject to basic misunderstandings. These often originate with the careless use of language but have a tendency to harden into metaphysical doctrines.
The practitioners of general system theory have an unfortunate tendency to speak of general systems as a subject that has predicates. . . . Current usage associates general with system instead of associating it with theory—and taking system as a predicate of theory. Thus we have general-system theory instead of general system-theory. This plays havoc with the legitimacy of the field. Elementary reflection discloses that there is no such real world entity as a general system. There is a kind of theory known as system theory, and there is a general form of this theory: general system theory. Assuming the contrary is nonsense. It is to assume that there is a theory of general systems. In fact, there is only a general theory of systems. . . .

The semantic confusion discussed here is a more than academic interest. It impedes the development and acceptance of general system theory. Those who, seduced by the careless linguistic habits of its practitioners believe that it is a theory of a curious entity called general system, view it with understandable skepticism. They are prevented from appreciating that general system theory is not the investigation of a mythological beast labelled general system but the investigation of the full scope of the phenomena conceptualized as various kinds of systems. . . .

A common fallacy is to hold that all general system theorists are metatheorists. In that event, they would not be system theorists, but theorists of system theories. What they would be concerned with would not be systems, but theories of systems—and such theories themselves are not systems. Those who investigate existing general theories of systems are not system theorists, but historians or philosophers of science, specialized to the field of general system theory. . . . The designation of the field as one of metatheory is false. As a source of confusion, the designation is dangerous, for it subsumes general system theory within another field and thus removes its individual raison d'être.

. . .

Some of the skepticism confronting general system theory is due to a (mistaken) belief that it is another terrible generalization. The most frequently heard opinions accuse it of being either a generalization of a theory of organism, or of a theory of automata depending on whether the accuser has heard more of von Bertalanffy or Ross Ashby, for example. Yet general system theory is innocent of such charges. It is not a generalized theory, but a general theory. We must not confuse the historical origins of a theory with its actual status and orientation.

In spite of the various unfavorable factors mentioned by Laszlo, systems science has made significant progress since the emergence of systems movement in the 1940s and 1950s. Perhaps the most fundamental role in forming systems science as a legitimate field of inquiry was played by the various conceptual frameworks (as overviewed in Chap. 4) through which the notion of systemhood is properly characterized and codified. Categories of systems that emerge from these frameworks delineate fairly precisely the domain of systems science.
Major progress has been made in using the computer as a laboratory. In fact, numerous laws of the various categories of systems have already been determined by properly designed experiments on computers, contributing thus to knowledge base of systems science. Although this knowledge base is still rather small when compared with other, well-established areas of science, it is slowly but steadily growing. Perhaps the most visible work regarding the search for systems laws, predominantly via computer experimentation, is currently pursued at the Santa Fe Institute in New Mexico. Since 1989, many results of these efforts have been published by the Institute in edited volumes *Santa Fe Institute Studies in the Science of Complexity.* Among the growing number of researchers contributing to the production of systems knowledge, Stuart Kauffman [1993, 1995], a member of the Santa Fe Institute, is generally recognized as a visionary pioneer who has pursued this line of research since the mid 1960s.

The progress in systems methodology over the last few decades has also been quite encouraging. On the conceptual level, it is important that systems problems are now well characterized and codified in terms of the various categories of systems. On the pragmatic level, it is clear that the methodologies for some important classes of problems have advanced considerably.

One such class consists of problems concerned with the relationship between overall systems and their various subsystems. These problems are modern formulations of some aspects of the age-old philosophical problem of wholes and parts [Lerner, 1963]. They are also closely connected with the polemic between the doctrines of reductionism and holism. Perhaps the most important outcome of a serious methodological work on these problems, as exemplified by the area referred to as *reconstructibility analysis* [Klir, 1985a, 1986], is that it provides us with new insights regarding the nature of the relationship between wholes and parts. The methodology resulting from this work goes far beyond the thinking emerging from both reductionism and holism. From the viewpoint of reconstructibility analysis, these doctrines are only two extreme positions in a broad spectrum of methodological possibilities. Reconstructibility analysis recognizes that it is often essential, and sometimes unavoidable, to reduce a complex system into appropriate subsystems in order to make it manageable. It makes us aware, however, that the choice of the subsystems is critical; some may represent the overall system quite well while others may be highly inadequate. We should not look for subsystems that look "natural," but rather for those that allow us to reconstruct the overall system with as high accuracy as possible.

As a rule, genuine systems problems are computationally extremely difficult. They are often made tractable by overly strong simplifying assumptions that are usually not explicitly stated. The resulting methods can then deal with sizable systems emerging from practical applications and produce "impressive" results, but the significance of these results is questionable at best. Such methodological dishonesty, which is one of the primary sources of the previously overviewed
critiques by Hoos, Berlinski, and Lilienfeld, is contrary to the spirit of systems science. The latter is not interested in producing immediately marketable methodological tools at the cost of convenient simplifying assumptions whose validity is dubious, but rather in pursuing basic methodological research involving genuine systems problems. To overcome the exploding computational complexity, which is characteristic of the later problems, is never easy and it usually requires years of concentrated research for each problem category. In his fight with computational complexity, the systems scientist has to explore various methodological strategies, such as, for example, the structuring of the computation hierarchically in terms of appropriate equivalence classes of the alternatives involved, the use of various heuristic procedures, the utilization of relevant background knowledge, the utilization of uncertainty of various types as a commodity that is traded for reduction of computational complexity, or the use of specialized computer technology based on massive parallel processing.

In spite of the difficulties and little support, systems methodology progresses in some areas rather well. Let me use the problems subsumed under reconstruc
tibility analysis as an example. This is a particularly difficult problem area since the number of collections of subsystems of a given overall system grows doubly exponential with the number of variables in the overall system. When reconstruc
tibility analysis was conceived in the mid 1970s [Klir, 1976], we were able to deal with systems of no more than seven or eight variables. Now, after more than a decade of research that involved virtually all the mentioned methodological strategies, we can handle systems with hundreds or even thousands of variables [Conant, 1988].

In parallel with the progress in systems methodology, we can also observe a progress at the metamethodological level. For one thing, the role of the computer in metamethodological studies (by which, for example, the performance of heuristic methods can be evaluated) has now become almost routine. However, one major task of systems metamethodology has not been accomplished as yet: to establish a rigorous relationship among the various conceptual frameworks overviewed in Chap. 4.

The progress in systems science can also be examined in terms of the growth of relevant publications, academic programs, and activities. In the mid 1970s, it was determined that the literature pertaining to systems science had been doubled approximately every four years since 1950 [Klir and Rogers, 1977]. This trend was still observed in the early 1980s [Trapp, Horn, and Klir, 1985], as expressed by the plot in Fig. 11.1. This is a good indicator of the progress in systems science. There is also some evidence; although less documented, that the number of academic programs and professional activities bearing upon systems science has been slowly but steadily growing since the 1950s.

How has the steady progress in systems science impacted other areas of science? This question is important but rather premature since systems science is
still in an early stage of its evolution. Moreover, the question is too broad to be adequately answered by any one person. To try to answer it anyhow, I can, at best, only direct the reader to the relevant literature.

The main impact of systems science on traditional science is, undoubtedly, its cross-disciplinary orientation. As a result of arguments pursued by systems science for decades, scientists are now becoming, in general, more sensitive to the limitations of their own disciplines. They tend to be considerably more aware now than a few decades ago that significant real-world problems involve almost always aspects that transcend disciplinary boundaries. This impact is, of course, only indirect and, consequently, it is virtually impossible to characterize it more specifically. Another indirect impact of systems science on the traditional science is the increasingly habitual use of systems thinking in the latter. For example, it is now quite common in most areas of traditional science to think in terms of systems concepts such as hierarchy, homomorphism, regulation, feedback, stability, adaptivity, complexity, information, and many others.

Some areas of traditional science have also been impacted by systems science more directly. In general, those most developed have been impacted least. Physics, for example, has been almost untouched by systems science. This does not mean, however, that systems science has no potential role in physics. In fact, the discussion of this role, which is potentially profound, has already started (see Special Issue of the International Journal of General Systems on Systems Thinking in Physics: Vol. 11, No. 4, 1985, pp. 279–345). The following are some ideas expressed by one of the main proponents of systems thinking in physics, Paul A. LaViolette [1985]:

Figure 11.1. The increase of significant new publications (books and papers in refereed journals) pertaining to systems science in the period 1950–1980.
The concept of the open system has proven to be applicable to a wide variety of fields. The functioning of biological organisms, business organizations, social systems, human personalities, and frameworks of knowledge may in each case be understood as systems whose structure is continuously sustained through the operation of import and export transactions and transformation processes. The open system concept is also found to be applicable to understanding the behavior of nonequilibrium phenomena studied in the physical sciences. However, there is one discipline which has thus far evaded the inroads of this general systems concept. This is microphysics, a field which is currently still framed within the mechanistic paradigm. Subatomic particles are today viewed in much the same way that atoms were once viewed in the time of Democritus, namely as closed, inert systems. Except, now it is understood that from time to time these “billiard balls” undergo abrupt changes of their internal structure (and identity) either through spontaneous decay or as a result of mutual collision. It is often claimed that modern science has “dematerialized” matter, particles now being represented in terms of probability density functions, rather than as solid bodies. However, the mechanistic fingerprint has left a deep impression on physics and its conceptualizations are still very much with us.

The question which naturally comes to the mind of the general system theorist is whether microphysical phenomena are indeed so different from other natural phenomena that they are the only ones for which open systems concepts are unsuitable. Or, is it that physics is still in its infancy and has not yet emerged from the mechanistic, closed system paradigm which also at one time characterized these other sciences. There are many who would agree that the latter may be the case. Indeed, of the sciences, microphysics deals with phenomena which take place on a scale that is quite far removed from direct experience. In studying a biological organism or a business organization it is quite easy to demonstrate, either through experiment or observation, that the continued existence of such systems depends on the import, transformation, and export of currencies such as chemical substances, energy, capital, and human labor. Detecting a comparable “currency” which might be actively sustaining a subatomic particle is quite a different story. In probing such a microscopic level of nature, one inevitably encounters an observational barrier which prevents the direct detection of processes at the subquantum level. Since all of our measuring probes are necessarily composed of matter or energy (subatomic particles or energy quanta), the most microscopic level we are able to directly sense by necessity is the quantum level. Even then we are prevented from simultaneously determining the precise position and energy of the particle under investigation, a restriction commonly known as the Heisenberg Uncertainty Principle. Therefore our understanding about the ultimate nature of particles and fields and the possible existence or nonexistence of an underlying dynamic, form-sustaining substrate must necessarily be arrived at through a process of inference.
The "unconquered" territory of physics, therefore, presents the following challenge to the general system theorist: Namely, is it possible to devise an open system model of space and time which provides for the emergence of quantum structures and fields, and which may perhaps be more suitable as a description of microphysical phenomena than present theoretical frameworks? Or more broadly speaking, is it possible to devise a new methodology for representing microphysical phenomena, one that is framed within the open system paradigm? It is the purpose of this paper to present such a new approach.

When the time is ripe, systems thinking, exemplified by these ideas, is likely to make a paradigm shift in physics. Thus far, however, the interest of physicists in ideas like these is lukewarm at best.

When we proceed from physics to biology, we find a considerably stronger connection with systems science. This is not surprising since biology belongs by very nature to the realm of organized complexity. The first serious discussions regarding the role of systems thinking, theory, and methodology in biology started in the late 1960s [Mesarovic, 1968]. These discussions involved systems scientists and biologists of both theoretical and experimental orientations. To illustrate the spirit of these discussions, let me use two quotes. In the first quote, D. F. Bradley, a submolecular biologist, illuminates an important role of systems thinking in biology by arguing the need of modeling biological phenomena as multilevel systems:

Most biological phenomena can be examined at many different magnifications. At each magnification interesting and even useful observations can be made and often prediction of future behavior or even correction of malfunctions can also be made. It is rather surprising that this is the case because there are very compelling reasons for believing that macroscopic biological behavior represents the summation of countless numbers of intermolecular reactions and interactions, each of which follows physical laws that operate at the angstrom level. Recognizing this, a being from outer space might logically assume that the proper course of study of earth life should begin at the submolecular level and proceed stepwise with larger and larger groups of individual reactions until the meter level was reached. Our sciences did not, however, develop historically in such a logical sequence. Sociology, economics and political science, psychology and psychiatry, anatomy, physiology, biochemistry, chemistry and physics have developed in parallel rather than sequential ways and often with little intercommunication. In the course of this development we have learned to treat illnesses of the whole being, such as mental disease, without referring to, or even knowing much about, the physiology, biochemistry, chemistry or physics underlying either the normal or the diseased condition. Mendel learned a great deal about heredity and could predict behavior in unborn organisms without knowing of the existence of deoxyribonucleic acid (DNA), much less that it carries the genetic information. . . .

Analysis at any given level may "work" for solving some problems and not for others. Psychiatry may prove useful in treating neurotics but not
psychotics. The tremendous difficulties inherent in attempting to analyze social or emotional behavior at the molecular level make attempts to analyze them at the sociological and psychiatric level well worthwhile. It is far too early to make valid predictions of the behavior of such large molecular systems organized into a living system and under genetic and environmental constraints. The time to begin thinking about the possibility of analyzing at levels of higher magnification is when analysis at the chosen level, the level which *a priori* seems most appropriate, fails. . . .

Success at problem solving at levels of low magnification is therefore to be applauded since it obviates the necessity of working a great deal harder at higher magnification levels. The best single criterion for going to higher magnification levels is persistent failure to solve specific or general problems at the *a priori* most appropriate level.

There are currently working in biology investigators who have moved from lower to higher magnification levels and those who have moved in the opposite direction. Doctors of medicine have become biochemists, or even molecular physicists, and physicists have left physics for biochemistry. The former are usually dissatisfied with the slowness with which progress is being made in solving problems at the medical level and turn to smaller systems whose problems they hope to solve more completely, precisely and rapidly. The physicist-turned-biochemist usually wishes to apply his proven problem-solving abilities on more complex systems which appear to hold more human interest. Collaboration between those who have moved in opposite directions to meet at the same level can be very productive, an outstanding example being the collaboration between the physicist, F.H.C. Crick and the biologist, J.D. Watson, which resulted in the determination of the three-dimensional structure of DNA.

In the second quote, R.E. Kalman, a mathematical systems theorist, explains his view of systems theory in biology:

Today systems theory is making an increasingly important impact on systems technology in providing new solutions to old problems or suggesting new kinds of systems. I have an (unproved) theorem which claims that "Systems Technology" = "Artificial Biology"; after all, the aim of the systems theorist is to create systems which approach or perhaps even surpass capabilities normally observed only in the living world. The aims of the systems theorists are not unlike those of the biologists, though we must remember that the two groups work under very different kinds of constraints. The systems theorist does not claim that computers will provide ready-made models for explaining the brain, but he is optimistic that the methods devised to gain a deep understanding of inanimate computers will have some relevence to the understanding of living computers. The systems theorist is not an engineer—he wants to know the capabilities and utilization of computers, but he does not worry about the fine constructional details. By analogy, the systems theorist will add little to the experimental phases of brain research, but he should be very useful (if he really
is a good theoretician) in the scrutiny and evolution of models which embody
and explain the experimental results.

As anticipated here by Kalman, systems theory has indeed played a major role
in the formation of modern theoretical biology. This is well explained by Robert
Rosen [1978], perhaps the most important liaison between systems science and
theoretical biology:

Biology is the science of life and of the living. The term “life” encompasses a
unique range of phenomena, and biology is correspondingly a unique science
in many ways. Among these is the fact that biology alone seems to sit at the
intersection of all other sciences, so that no advance in any science can be
without impact on biology; and conversely, any advance in biology ultimately
propagates into every corner of science.

The basic object of study in biology is the individual organism. The basic
question is: how does it work? What are the basic forces which shaped it and
maintain it? In attempting to come to grips with this basic question, we soon
find that it is not one question, but many different questions, the answer to each
of which gives rise to a different branch of biology.

For instance, we may ask how an organism maintains its structure and
organization against the disordering forces of its ambient environment. By its
very nature, the dominant ideas in physiology will be those of homeostasis,
stability and control.

We may ask how an organism comes to have the particular structure and
organization which characterize it. There are two kinds of answers to this
question. One is in terms of development; the elaboration of this structure and
organization from its simplest direct antecedent, usually a zygote or fertilized
egg. Another kind of answer is found in evolutionary biology, the study of the
continuity of life over geological time back to, and including, the origin of life
on the planet. The dominant feature in both these areas is not so much
homeostasis but what Waddington has termed homeorhesis: the stability of
dynamical processes. Characteristically we have to study here the emergence
of complex structures from simpler ones, and hence both of these areas are
intimately involved with ideas of self-organization.

We may ask how it is that organisms tend to resemble their ancestors, and
why they resemble their proximal ancestors more than their remote ancestors.
The study of this question resides in the science of genetics. The prevailing
idea here is that of a code or program, which is successively “read out” or
expressed in each individual organism, and is transmitted from ancestor to
descendants modulated by a sexual mechanism. The idea of a code carries with
it a corresponding duality between genotype (the program) and phenotype (an
expression of the program), a duality which is so characteristic of organisms.

We can ask how it is that an individual organism adapts or modifies its
behavior in response to environmental fluctuations. Here the objects of study
are the transducers between environmental stimulus and organismic response;
the most important such transducer is the central nervous system of higher
animals. The dominant ideas here are those of computation and communication.

We can ask how a number of organisms will interact with each other and with their environment. Here again, a number of answers can be given to such a question. One kind of answer will lead off into population biology and ecology. Another kind of answer will lead in the direction of the study of communities and social structures (including human societies in all their aspects). Still another kind of answer will take us into psychology, with its ideas of learning, conditioning, intelligence and consciousness.

Resting on all of these sciences which collectively comprise biology, we find a number of unique technologies which seek to regulate or control biological processes: agriculture; husbandry; and medicine. . . .

Biology has provided for system theory a unique source of challenge, of inspiration, and a field for applications. Let us note a few. The field of cybernetics is largely an outgrowth of the physiological concept of homeostasis, or the "constancy of the milieu interior," enunciated by Claude Bernard in the 19th century. This was combined with the recognition that we could explicitly design devices which would similarly exhibit adaptive behavior in order to maintain constancy of some associated quantity. Out of this came the integrative notion of feedback control; it is significant to note that Wiener subtitled his definitive work on cybernetics, "Control and Communication in the Animal and the Machine." The dissection of an organism, or a machine, into a set of interacting feedback loops, provides a specific analysis of dissimilar systems behaving similarly.

Another example: what we now call the theory of automata arose in large part through explicit and tacit attempts to understand the behavior of the brain. Turing's initial approach to the machines which bear his name was an abstraction, not from a mechanical device, but from how a human being would go about solving a mathematical problem, or computing a number. The neural nets of McCulloch & Pitts [1943] were an explicit attempt to model the brain in terms of a network of threshold elements. It was quickly recognized that such ideas were intimately related to the switching networks of communication theory; to digital computation in its widest sense; to the mathematical theory of effective or recursive processes (and hence to the foundation of mathematics itself); to linguistics; and to psychology. An entire research area, generally called "artificial intelligence," has grown up to exploit the possibilities of generating brainlike behavior in technological systems through the employment of automata-theoretic concepts.

Still a third example may be mentioned, the theory of "self-organization" or adaptive systems whose paradigms are most often drawn from the phenomena of developmental biology, but which are also found in learning systems, social systems, linguistic systems in their widest sense, in technological artifacts, and in purely physical systems exhibiting co-operative effects.

From all of these diverse sources, the concept of a system has been distilled; both as an object of study in its own right, and as a vehicle for the
Examples of the cross-fertilization between biology and systems science are the various types of goal-oriented, self-organizing biological organisms. On the other hand, the general theory of living systems developed by James G. Miller (1978) for the study of self-organizing, self-reproducing, autopoietic, adaptive, anticipatory, or morphogenetic systems is an example of a degree of freedom in the social sciences. Since the emergence of cybernetics and systems research in the late 1940s and early 1950s, there have been increasing expectations that these new cross-disciplinary areas will play an increasing role in our lives. These expectations have been fulfilled in different areas of social sciences, for example, in the degree of freedom in the social sciences. Consequently, advances in theoretical biology, closely correlated with developments in systems thinking, have become a matter of great interest in systems science. The nature of this potential has not been fully realized as yet. It has not been possible, however, to develop any model for organic behavior that does not represent a possible class of behavior. From these simple heuristic considerations, we see immediately why it is that (a) biology and general system theory have been so intimately related, and (b) biology plays such a unique and pivotal role in the sciences. For there is hardly any class of systems which does not either represent a possible class of systems which are not usually regarded as organs or systems which are not usually regarded as organs or systems which are not usually regarded as organs or systems which are not usually regarded as organs or systems which are not usually regarded as organs.
some systems theories that are now well developed, most notably the theory of feedback control. In spite of its rather obvious applicability to the study of economic phenomena, the theory of feedback control has virtually been ignored by most economists. Although the connection between the theory and economics was developed by Oskar Lange [1970], the influence of his work on mainstream economics is not noticeable. Second, under the influence of the cross-disciplinary perspective of systems science, economics could have a chance to liberate itself from its notorious isolationism, which takes it for granted that the economic process can meaningfully be studied as a closed system. That is, orthodox economic models do not involve social, political, ecological, legal, and other relevant aspects, which are considered outside the self-imposed, rigid boundaries of economics. While the study of a phenomenon in isolation is often acceptable in natural sciences, this investigative strategy is totally unrealistic in social sciences, where phenomena that we artificially classify as economic, political, social, ecological, etc., are strongly interconnected. As a consequence, the history of economics is a history of a persistent discrepancy between economic predictions and economic reality. In order to develop more realistic economic models, systems open to relevant phenomena outside economics will have to be considered.

In other areas of social sciences, systems science has been more influential. Systems thinking, for example, is now quite common in political science, management science, and other areas.* At the same time, methodological research in systems science has responded to special methodological needs, often cross-disciplinary, of social sciences.†

To illustrate the great potential of systems thinking in social sciences, let me overview the work of Arvin Aulin [1982, 1989], which focuses on the use of the

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*The purpose of this note is to provide the reader with representative references that describe connections between the various facets of systems science and some other areas. Neither the list of areas nor the references listed under each area are exhaustive.

Philosophy: Laszlo [1971], Bunge [1977], Blansberg, Sadowsky, and Yudin [1977].


Ziegenhagen [1986].


†Methods that emerged from the needs of social sciences are well exemplified by the interpretive structural modeling, developed by Warfield [1976], by the so-called Q-analysis, which was developed by Atkin [1976, 1977], and by the methodology developed by Forrester [1961, 1968, 1969, 1971, 1975], which is usually referred to as Forrester’s systems dynamics.
of requisite variety and the law of requisite hierarchy for global studies of regulating capabilities of human societies.

As previously explained (Chap. 10), the law of requisite variety states that a necessary condition for a successful regulation is that the variety of the regulating system must be at least as large as the variety of the regulated system. The law of requisite hierarchy is due to Aulín himself. It states that the lack of regulatory capability of a simple regulator can be compensated for, to some degree, by conceptualizing the regulator as a hierarchical multilevel structure system. In general, the weaker the regulatory abilities of available regulators, the more hierarchy is needed to achieve the same regulatory performance, if it is possible at all.

Survival of a human society depends on keeping certain essential variables (food, energy sources, medical supplies, etc.) within their survival regions in spite of disturbances. This can be accomplished by organized human production. Production forces of the human society (i.e., labor and production means) can thus be viewed as its regulator. The weaker the production forces, the less capable is this regulator and, using the principle of requisite hierarchy, the higher the degree of hierarchy that is necessary for effective regulation and survival of the society. This means that, given a specific production level of the society, some specific minimum degree of hierarchy is necessary for its survival. When the degree of hierarchy is increased, the regulatory capability of production forces of the society increases until an upper limit of the degree of hierarchy is reached, beyond which further increase has no effect on the regulation. That is, the law of requisite hierarchy establishes, in the context of social systems, a relationship between the production capacity of a human society and the degree of hierarchy associated with the organization of the society that is needed for efficient regulation. For each production level, there is a specific degree of hierarchy for which the regulation is most efficient. In general, the degree of requisite hierarchy increases with decreasing level of production capacity.

Another important concept in Aulín’s global studies of human societies is the concept of steering of human actions (individual or social). Aulín makes a clear distinction between steering of human actions from outside and their self-steering. He views human actions as interactions between subjects and objects, i.e., between conscious actors and parts of the real world. Actions are steered from outside by conditioning the consciousness of the actor, i.e., his cognitive beliefs, values, and norms. On the other hand, actions based on self-steering result from belief, values, and norms that are developed by observations and generalizations.

In general, Aulín characterizes a self-steering system as a goal-oriented system that always expands its states as well as goals, i.e., it never returns to the same state or goal. This means that a self-steering system is constantly producing something new, both in terms of goals and means of achieving them, insofar as it is not forced into a state that is outside the domain of self-steering.
Aulin argues that self-steering in a system increases its regulatory capability. This implies, using the principle of requisite hierarchy, that any increase in the degree of self-steering in a system reduces the degree of hierarchy in its organization demanded for effective regulation. Self-steering and hierarchy are thus reciprocal notions and, consequently, the degree of self-steering can be expressed in terms of the degree of hierarchy.

The lower limit of the degree of hierarchy in each particular society is absolute in the sense that it cannot be exceeded without risking the survival of the population of the society. The upper limit of the degree of hierarchy is not absolute. It can be exceeded without jeopardizing the survival of the population. When exceeded, however, no improvement in the regulation is achieved, while, at the same time, the level of self-steering in the society is proportionally reduced. Since self-steering is associated with freedom, creativity, and progress, its reduction beyond the desirable minimum (corresponding to the upper limit of the degree of hierarchy) restricts unnecessarily the overall progress of the society. Among other things, it restricts the improvement of productive forces, while, at the same time, it evokes social movements resisting severe restriction of freedom and highly visible inequality caused by the unnecessarily high degree of hierarchical organization of the society.

In general, an increase of the degree of self-steering in a human society means progress in human emancipation. The potential for self-steering depends on the level of production. The actual degree of self-steering, however, is not solely the result of the production level, but it depends on other factors (political, administrative, etc.) as well. Unless self-steering is totally suppressed, production level in every human society tends to increase in time. This implies that the maximum and minimum degrees of hierarchy of the society steadily decrease and, consequently, the maximum and minimum degrees of self-steering increase.

The increase of the potential degree of self-steering (i.e., its maximum and minimum values) of a human society in the course of time, due to the increase in production level, and the actual degree of self-steering in the society over the same period of time (determined by political and other factors) is a fundamental macroscopic characteristic of the society. Aulin uses this characteristic for introducing and comparing various types of human societies. He also uses it for describing some historical as well as current societies.

Aulin’s work is important since it clearly demonstrates that the use of modern systems thinking for studying social phenomena is very powerful. Without resorting to any ideological arguments, he can show, for example, that the Marxist theory leads to conclusions that violate the laws of requisite variety and requisite hierarchy, laws that must be satisfied by all systems.

I trust that these few remarks, together with the relevant articles in Part II, will provide the reader with a general but adequate impression of the current status of systems science and its real or potential impact on other areas of science. To
11.3. The Future of Systems Science

There is little doubt that, in the foreseeable future, systems science will continue to be the principal intellectual base for making advances into the territory of organized complexity, a territory that still remains, by and large, virtually unexplored. The challenge offered by organized complexity narrows down fundamentally to one question: how to deal with systems and associated problems whose complexities are beyond our information-processing limits? We are well aware now that the answer to this question lies primarily in the relationship among four key characteristics of systems models: complexity, uncertainty, credibility, and usefulness. However, we are still far from understanding this intricate relationship, in which all the characteristics are themselves multidimensional entities with their own structures. Although some results relevant to this issue have recently been obtained, most notably results regarding the notion of uncertainty [Klir and Wierman, 1999], this only scratches the surface of the whole issue. I believe that basic research concerning this relationship will be the main preoccupation of systems science in the foreseeable future, extending likely well into the Twenty-First Century.

Since the computer is the principal laboratory in systems science, further progress of systems science will undoubtedly be closely correlated with advances in computer technology and science. As far as computer hardware is concerned, its overall progress is usually expressed in terms of several factors such as the number of ordinary instructions that are executed by a single central processing unit (CPU) of the high-end type, the number of transistors on a single CPU chip, the number of bits per chip of a dynamic random access memory, the number of bits per square inch of a magnetic disk storage, and the speed and resolution of input and output devices. Each of these factors has shown a steady exponential growth, with an overall hardware capability increase of approximately 20% each year. This trend is likely to continue until physical limits are reached or the currently predominant electronic hardware is replaced with hardware based on radically different principles such as optical, chemical, biochemical (DNA, bacteria), quantum, or superconductive.

The progress in computer hardware (or “wetware” in the case of DNA computing) is only one factor contributing to increases in the overall capabilities of computing systems. Another factor is the underlying computer architecture, in particular architecture based on massive parallel processing. Computer systems with perhaps as many as several million simple processors are currently under
consideration. Problems in designing such systems are formidable, but there is no doubt that they will be given great attention in the years to come.

In its relationship with computer technology, systems science is not only a beneficiary, but also a benefactor. For example, one of the main issues of current research on parallel computers is to determine the right architectures for the various classes of systems problems. Systems science, one of whose main concerns has been a comprehensive study of systems problems, should be an important resource in this respect. For some problem areas involving computer technology, such as modeling and performance evaluation of large computing systems, software engineering, and knowledge engineering, systems science is also an important methodological resource.

The increasing computing power also influences new developments in mathematics, and these, in turn, produce new methodological types of systems, enlarging thus the inventory of systems categories. The most visible recent trend in mathematics is to generalize. As a rule, each generalization opens new methodological possibilities while, at the same time, it tends to increase the computational complexity involved. To counterbalance this increase in computational complexity, we need an adequate increase in computing power. This explains why some of these generalizations have come to the fore only lately: they became practical only when the available computing power became sufficient to support them.

It is interesting to observe that a generalization of a mathematical theory results usually in a conceptually simpler theory. This is a consequence of the fact that some properties of the former theory are not required in the latter. At the same time, the more general theory has always a greater expressive power, which, however, is achieved only at the cost of greater computational demands. This conceptual simplicity of some mathematical theories whose expressive power is very high allows us to generate patterns of great complexity by simple rules that are repeated many times. Here, the high complexity is manifested on the level of data systems; it is produced, with the help of powerful computer technology, by simple systems on some epistemologically higher level. This phenomenon, which has lately emerged in systems science, is increasingly referred to by the catchy phrase *simple models of complex systems* [Goel and Rozehnal, 1991].

The current trend of generalizing mathematical theories is manifested by the following representative examples of generalizations:

- From quantitative to *qualitative theories*;
- From functions to *relations*;
- From graphs to *hypergraphs*;
- From ordinary geometry (Euclidean as well as non-Euclidean) to *fractal geometry*;
- From ordinary automata to *dynamic cellular automata*;
- From linear to *nonlinear theories*;
- From regular to singular (catastrophe theory);
- From precise analysis to interval analysis;
- From classical probability theory to the various theories of imprecise probabilities;
- From classical set theory and logic to fuzzy set theory and logic;
- From classical measure theory to the theory of monotone measures (nonadditive, in general);
- From regular languages to developmental languages;
- From intolerance to inconsistencies of all kinds to the logic of inconsistency;
- From single objective to multiple objective criteria optimization.

These generalizations have enriched not only our insights but, together with computer technology, extended also our capabilities for modeling the intricacies of the real world. It is not within the scope of this introductory book to cover these recent developments in mathematics, in spite of their importance for systems science.

The following are bibliographical remarks regarding the recent developments in mathematics that are of significance to systems science. The growing interest in qualitative mathematics is well documented by a recent book on systems modeling and simulation [Fishwick and Laker, 1991]. Although thinking in terms of mathematical relations rather than functions is fundamental to systems science, there is no publication oriented to systems science that covers mathematical relations in a comprehensive way. An exception are binary relations on the Cartesian product of one set; these are usually called directed graphs and are covered in the literature under the name graph theory [Harary et al., 1965; Berge, 1973]. Generalizations of graphs are hypergraphs [Berge, 1973], which are important for dealing with structure systems (a hypergraph is a finite set with a family of nonempty subsets that cover the set). A good overview of issues involved in nonlinear systems was prepared by Casti [1985]. Special areas emerging from the theory of nonlinear systems include catastrophe theory [Thom, 1975; Zeeman, 1977; Poston and Stewart, 1978; Casti, 1979; Arnold, 1984], which focuses on the study of singularities of mathematical functions; theory of chaos [Barnsley and Demko, 1986; Holden, 1986; Gleick, 1987; Devaney, 1987; Becker and Dorfler, 1989; Rabin, 1990], under which unpredictable bifurcations in behavior of dynamic deterministic systems, due to sensitive dependence upon initial conditions, are studied; and fractal geometry [Mandelbrot, 1977; Barnsley, 1988; Feder, 1988; Fleischmann et al., 1989], whose descriptive power enables us (with the help of the computer) to generate geometrical objects of most peculiar shapes (e.g., shapes described by functions that are continuous and yet nondifferentiable at every point) by transformations (often quite simple) of similarity and scaling on metric spaces, resulting in patterns that repeat themselves at smaller and smaller scales. Generalizations that emerged from classical automata and formal language theories include Petri nets [Peterson, 1981], dynamic cellular automata [Wolfram, 1986; Triffoli and Margolus, 1987], and developmental systems (or L-systems) [Herman and Rosenfeld, 1975; Prusinkiewicz and Lindenmayer, 1990; Goel and Rokohlal, 1991]. Generalizations in mathematics that attempt to deal with the various types of uncertainty include the following areas: interval analysis [Moore, 1979], fuzzy set theory and logic [Dubois and Prade, 1980; Zimmermann, 1985; Kandel, 1986; Yager et al., 1987; Klir and Yuan, 1995, 1996], fuzzy measure theory [Wang and Klir, 1992] and its various special subtheories, such as evidence theory [Shafer, 1976], possibility theory [Dubois and Prade, 1988] and theories of imprecise probabilities [Walley, 1991]. Especially important for systems science is the notion of fuzzy systems [Negoita and Ralescu, 1975; Negoita, 1981; Pedrycz, 1989; Klir, 1990]. (continued)
The study of relatively new categories of systems—such as cellular automata, developmental systems, neural networks, systems based on fractal geometry, fuzzy systems, and nonlinear chaotic systems—will likely be one of the most active areas of research in systems science in the coming years. Another active area will be, in my opinion, the study of the various types of goal orientation, in particular self-organization, self-preservation, self-reproduction, and autopoiesis.

On a broader scene, the genuine cross-disciplinary and holistic perspective of systems science will undoubtedly continue to counterbalance, by and large indirectly, the one-sided reductionist and discipline-oriented attitude that still prevails in contemporary science. The profound long-term implications of this ongoing influence upon science and even culture are well described by Norman D. Cook [1980]:

The value of general systems theory lies not only in what it can bring to the individual academic disciplines, but also—and more importantly, I believe—in what it can bring in terms of a unified world-view and a fundamental, scientific conception of nature and human life—a world-view which is both open to precise, reductionist analysis and comprehensible to our mundane and poetic common sense. Thus far, Western science has excelled in reductionist analysis and has thereby led us into a world of mass communications, global transportation, computer technology and molecular medicine. But it has also led us into a psychological—philosophical wasteland where, in spite of and often because of stupendous material progress, our sense of the wholeness and unity of human existence and our daily appreciation of it have disappeared. Despite the hopes of the dreaming romantic, however, there is no retreat from modern science—either in practical, technological terms or in terms of the scientific world-view. And yet, there is the real hope of a more unified scientific world-view than that which is currently scientific "common sense"—a world-view which does not leave us with ridiculous, semiscientific odds and ends... aggression centers, death instincts, genes for drug addiction, and magnetic monopoles... without a rational synthesis. Of course, the technological developments of reductionist science are a well-recognized mixed blessing, but the mixed nature of the reductionist's world-view is less widely appreciated. On the one hand, reductionist science has given us the ability to search for (and often find) single-element causes for the malfunction of larger systems, but, on the other hand, we apparently lose the ability to consider whole systems. Disease is traced back to the microorganism or even molecule which "causes" it, despite the fact that life and its irregularities are unquestionably systemic phenomena. The organisms' fundamental "attitude" to the pathogens may indeed determine the effectiveness of the attack by the smaller molecule.

Basic ideas regarding the management of inconsistencies (usually local) in mathematical descriptions of systems were developed by Rescher [1976] and Rescher and Brandon [1980].

Another visible trend in the role of mathematics in systems science is the use of category theory [Padulo and Arbib, 1974; Mesarovic and Takahara, 1975, 1988; Takahara and Takai, 1985].
cell or organism on the larger system. In psychology, the outstanding questions concerned with the nature of consciousness, will, and individuality have been all but abandoned as the search for “aggression centers,” “satiation centers,” etc. proceeds. Without condemning such obviously relevant research, it is nonetheless apparent that the systemic phenomenon of man is not clarified in this type of reductionist psychology. At the social level, again the reductionist approach singles out a few key elements—the man or men central to a social change—and fails to see the development of the social system itself.

Certainly the wholist’s and the reductionist’s approaches to reality differ fundamentally from one another, but there is nothing inherently contradictory between them. As the reductionist continues to define with ever greater precision the elements and few-body interactions of the elements within natural systems, the wholist can—and, indeed, must be able to—put those same elements within ever more comprehensive wholistic, many-bodied frameworks.

Although meaningful wholistic work necessarily must be built upon (and, chronologically, follow) the reductionist elucidation of the elements involved, the wholistic approach is not merely a rephrasing of the questions already answered by the reductionist. To the contrary, the wholist’s questions as well as answers are apt to differ radically from the reductionist’s. By putting the reductionist’s remarkably well-defined bits and pieces within a rational context, we may be able to re-open the wholistic approach to natural phenomena which has long been neglected and even ridiculed by a scientific community devoted to rigorous reductionism.

Most important among the current wholistic modes of thought is “general systems theory.” It offers a potentially more wholistic conception of natural phenomena than is now presented by the scientific world and yet without abandoning traditional scientific precision and verifiability. By dealing with man, society, or any unit of natural organization in terms of its basic properties as an abstract “system,” the fundamentals of the given systems of nature may be discerned not only as the isolated, disconnected units of reductionist science, but also as units coordinated within a system which in turn functions within an environment. That is, individual objects can be rationally studied within their natural contexts.

What is the likely role of systems science in the emerging information society? There seems to be a general agreement that information society will be a fundamentally different environment for organizations than was industrial society. It is expected that the amount of available knowledge, the level of complexity, and the degree of turbulence will be significantly greater in the information society than they were in the industrial society. In addition, it is also expected that even the absolute growth rates of these three factors will be significantly greater than in the past.
In information society, organizations will have no choice but to deal with this radically different environment. To survive, they will have to adjust their structures, processes, and technologies to be able to cope with the new environment. This implies that designs of organizations in information society will be qualitatively different from those in industrial society.

Confronted with an environment that is characterized by more and increasing knowledge, complexity, and turbulence, organizations will be faced with challenging demands on decision making. Not only will decision making be considerably more complex, but decisions will also be required more frequently and will have to be made faster.

To keep organizations compatible with the environment, substantial portions of decision making in information society will be concerned with organizational innovations, i.e., radical changes in produced goods or services, as well as in the technologies, processes, and structures of the organizations themselves. In general, demands on organizational innovations will be more frequent, more extensive, and will have to be implemented faster than in the past.

All these demands on organizations in information society indicate that organizations will be required to function as anticipatory systems, i.e., systems that possess ongoing capabilities of building relevant systems models of their environments and are able to use these models for making decisions and actions that optimize specific goals. This means that an ongoing systems modeling of relevant aspects of the environment will be an essential feature of the decision-making infrastructure of organizations. This implies that expertise in systems science will be in increasing demand by organizations in the information society.*

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*The following are the main journals that publish regularly articles relevant to systems science:

- Behavioral Sciences (Journal of the International Society for the Systems Sciences);
- Complex Systems (Complex Systems Publications, Champaign, Illinois);
- Cybernetics and Systems (Hemisphere, New York);
- Fuzzy Sets and Systems (North-Holland, Amsterdam);
- IEEE Transactions on Systems, Man, and Cybernetics (published by the IEEE Systems, Man, and Cybernetics Society);
- International Journal of General Systems (Gordon and Breach, New York);
- International Journal of Systems Science (Francis & Taylor, London);
- Kybernetes (MCB University Press, Bradford, U.K.);
- Kybernetika (Czech Academy of Sciences);
- Revue Internationale de Systemique (AFCET, Paris);
- Systems Practice (Plenum Press, New York and London);

Other valuable sources of information are:

- International Book Series on Systems Science and Engineering (sponsored by the International Federation for Systems Research and published by Kluwer Academic/Plenum Publishers, New York);
- General Systems Yearbook (published by the International Society for the Systems Sciences);
- Bibliographies [Klir and Rogers, 1977; Trapp, Horn, and Klir, 1985].
References


Ashby, W. R. (1968), Some consequences of Bremsermann's limit for information processing systems.


Ashby, W. R. (1969), Two tables of identities governing information flows within large systems, ASC Communications, 1, 3–8.


Bateson, G. [1967], Cybernetic explanation, American Behavioral Scientist, 10(8), 29–32.
Cartwright, T. J. [1991], Experimental systems research: Towards a laboratory for the general theory, Cybernetics and Systems, 22(1), 135–149.
References


Cavallo, R. E. [1979], The Role of Systems Methodology in Social Science Research. Martinus Nijhoff, Boston.


Devaney, R. L. [1987], An Introduction to Chaotic Dynamical Systems. Addison-Wesley, Reading, Massachusetts.


References


Klir, G. J. [1985b], The emergence of two-dimensional science in the information society, Systems Research, 2(1), 33–41.


References

Leduc, S. [1911], The Mechanism of Life. Re known, London.
Marchal, J. B. [1975], The concept of a system, Philosophy of Science, 42(4), 448–467.
McCulloch, W. S., and Pitts, W. [1943], A logical calculus of the ideas immanent in nervous activity, Bulletin of Mathematical Biology, 5, 115–133.
Mesarovic, M. D. [1972], A mathematical theory of general systems. In Klir [1972a].
Miller, J. G. [1986], Can systems theory generate testable hypotheses? From Talcott Parsons to living systems theory, Systems Research, 3(2), 73–84.
Moore, R. E. [1979], Methods and Applications of Interval Analysis. SIAM, Philadelphia.


Rosen, R. [1978], Biology and systems research: An overview. In Kline [1978].

References

Suuliketo, I. [1982], The Origins and Development of Systems Thinking in the Soviet Union. Annales Academia Scientiarum Fennicae (Finnish Academy of Sciences), Helsinki.
Thom, R. [1975], Structural Stability and Morphogenesis. Addison-Wesley, Reading, Massachusetts.
Thorn, D. C. [1963], An Introduction to Generalized Circuits. Wadsworth, Belmont, California.


References


