

# ANTICIPATORY SYSTEMS

---

Philosophical, Mathematical  
& Methodological Foundations

---

**ROBERT ROSEN**

Dalhousie University,  
Nova Scotia, Canada

IFSR International Series on  
Systems Science and Engineering

Volume 1

PERGAMON PRESS

# ANTICIPATORY SYSTEMS

Philosophical  
Mathematical  
and  
Methodological Foundations

*IFSR International Series on  
Systems Science and Engineering — Volume 1*

**International Federation for Systems Research**

**International Series on Systems Science and Engineering**

**VOLUME 1**

*Editor-in-Chief:* G. KLIR, State University of New York at Binghamton, USA

---

*Other Titles of Interest*

**AKASHI**

Control Science and Technology for the Progress of Society  
(8th Triennial IFAC World Congress)

**ANAND**

Introduction to Control Systems, 2nd edn.

**BROADBENT & MASUBUCHI**

Multilingual Glossary of Automatic Control Technology

**ELLOY & PIASCO**

Classical and Modern Control with Worked Examples

**EYKHOFF**

Trends and Progress in System Identification

**GVISHIANI**

Systems Research: Methodological Problems, Volumes 1 & 2

**HASEGAWA**

Real-Time Programming 1981

**MAHALANABIS**

Theory and Application of Digital Control

**MAHMOUD & SINGH**

Large Scale Systems Modelling

**MILLER**

Distributed Computer Control Systems

**MORRIS**

Communication for Command and Control Systems

**PATEL & MUNRO**

Multivariable System Theory and Design

**SINGH *et al.***

Applied Industrial Control: An Introduction

**SINGH & TITLI**

Systems: Decomposition, Optimization and Control

**TZAFESTAS**

Distributed Parameter Control Systems: Theory and Application

*Pergamon Related Journals (free specimen copies gladly sent on request)*

**AUTOMATICA**

**COMPUTERS & INDUSTRIAL ENGINEERING**

**COMPUTERS & OPERATIONS RESEARCH**

**JOURNAL OF THE OPERATIONAL RESEARCH SOCIETY**

**SYSTEMS RESEARCH**

# ANTICIPATORY SYSTEMS

Philosophical  
Mathematical  
and  
Methodological Foundations

ROBERT ROSEN  
Dalhousie University, Nova Scotia, Canada



PERGAMON PRESS

OXFORD · NEW YORK · TORONTO · SYDNEY · PARIS · FRANKFURT

U.K.	Pergamon Press Ltd., Headington Hill Hall, Oxford OX3 0BW, England
U.S.A.	Pergamon Press Inc., Maxwell House, Fairview Park, Elmsford, New York 10523, U.S.A.
CANADA	Pergamon Press Canada Ltd., Suite 104, 150 Consumers Road, Willowdale, Ontario M2J 1P9, Canada
AUSTRALIA	Pergamon Press (Aust.) Pty. Ltd., P.O. Box 544, Potts Point, N.S.W. 2011, Australia
FRANCE	Pergamon Press SARL, 24 rue des Ecoles, 75240 Paris, Cedex 05, France
FEDERAL REPUBLIC OF GERMANY	Pergamon Press GmbH, Hammerweg 6, D-6242 Kronberg-Taunus, Federal Republic of Germany

---

Copyright © 1985 R. Rosen

*All Rights Reserved. No part of this publication may be reproduced, stored in a retrieval system or transmitted in any form or by any means: electronic, electrostatic, magnetic tape, mechanical, photocopying, recording or otherwise, without permission in writing from the publishers.*

First edition 1985

**Library of Congress Cataloging in Publication Data**

Rosen, Robert, 1934-  
Anticipatory systems.  
(IFSR international series on systems science  
and engineering; v.1)  
1. System theory. I. Title. II. Series.  
Q295.R667 1985 003 84-26626

**British Library Cataloguing in Publication Data**

Rosen, Robert  
Anticipatory systems: philosophical,  
mathematical & methodological foundations.—  
(IFSR international series on systems science  
and engineering; v. 1)  
1. System theory  
I. Title II. Series  
003 Q295  
ISBN 0-08-031158-X

## Foreword

This manuscript was written during the months between January and June in 1979. The writing was done under difficult circumstances. At that time, I had some reason to fear that I would no longer be able to personally pursue a program of active scientific research. Yet at the time, I felt (and still do) that I had arrived upon the threshold of some entirely new perspectives in the theory of natural systems, and of biological systems in particular. The issues involved were of sufficient interest and importance for me to wish them to be pursued by others, if they so desired. On the other hand, my own outlook and development are different from other people's, and have with justification been called idiosyncratic. Thus I resolved to try, while I knew I was still able, to set down a coherent narrative development, beginning with the most elementary matters, which I hoped would permit others to understand why I was doing the things I was doing.

This volume is the result. I have organized it around the concept of anticipation, which is fundamental in its own right, and which connects naturally to a cluster of other concepts lying at the heart of natural science and of mathematics. Strictly speaking, an anticipatory system is one in which present change of state depends upon future circumstances, rather than merely on the present or past. As such, anticipation has routinely been excluded from any kind of systematic study, on the grounds that it violates the causal foundation on which all of theoretical science must rest, and on the grounds that it introduces a telic element which is scientifically unacceptable. Nevertheless, biology is replete with situations in which organisms can generate and maintain internal predictive models of themselves and their environments, and utilize the predictions of these models about the future for purpose of control in the present. Many of the unique properties of organisms can really be understood only if these internal models are taken into account. Thus, the concept of a system with an internal predictive model seemed to offer a way to study anticipatory systems in a scientifically rigorous way.

This book is about what else one is forced to believe if one grants that certain kinds of systems can behave in an anticipatory fashion. I begin with an autobiographical account of how I came to believe

that anticipatory systems were important, and why. I then proceed to an extensive discussion of the central concept of anticipation, namely, the modelling relation between a system and a model, or a system and an analog. In the process of exploring this relation, and by exhibiting manifold and diverse examples of this relation, I try to establish the basic threads between the concepts of science and mathematics. I hope that this discussion will be of interest in its own right, and will also serve to illustrate that the approaches I take are not really as idiosyncratic as might at first appear. Only when this background is laid do I turn to the concept of anticipation itself, and explore some of the elementary properties of systems which can anticipate. Here I well realize, the surface is barely scratched. But this was as far as I had been able to progress at the time, and the point of the book is to guide the reader to the surface.

For a variety of external reasons, the manuscript was not published immediately. However, none of the fundamental material has changed in the intervening five years, and I have not altered the original text. I have, however, added an Appendix. It has turned out that the initial anxieties which generated the book were unfounded, and since that time, I have been able to push some of the fundamental implications of the relation between system and model much further. In the Appendix, some of these more recent developments are sketched, and some of their rather startling implications are pointed out. I hope to enlarge this Appendix into a separate monograph in the near future, giving full details to justify what is merely asserted therein.

I would like to take this occasion to thank the many colleagues and friends who have lent precious moral support as a beacon through dark times. Among them I may mention J. F. Danielli, George Klir, I. W. Richardson, Otto Rössler, Ei Teramoto, and Howard Pattee. I hope that this volume will justify at least some small part of their exertions on my behalf over the years.

# Preface

The present volume is intended as a contribution to the theory of those systems which contain internal predictive models of themselves and/or of their environment, and which utilize the predictions of their models to control their present behavior.

Systems of this type have a variety of properties which are unique to them, just as "closed-loop" systems have properties which make them different from "open-loop" systems. It is most important to understand these properties, for many reasons. We shall argue that much, if not most, biological behavior is model-based in this sense. This is true at every level, from the molecular to the cellular to the physiological to the behavioral. Moreover, model-based behavior is the essence of social and political activity. An understanding of the characteristics of model-based behavior is thus central to any technology we wish to develop to control such systems, or to modify their model-based behavior in new ways.

The essential novelty in our approach is that we consider such systems as single entities, and relate their overall properties to the character of the models they contain. There have, of course, been many approaches to planning, forecasting, and decision-making, but these tend to concentrate on tactical aspects of model synthesis and model deployment in specific circumstances; they do not deal with the behavioral correlates arising throughout a system simply from the fact that present behavior is generated in terms of a predicted future situation. For this reason, we shall not at all be concerned with tactical aspects of this type; we do not consider, for instance, the various procedures of extrapolation and correlation which dominate much of the literature concerned with decision-making in an uncertain or incompletely defined environment. We are concerned rather with global properties of model-based behavior, irrespective of how the model is generated, or indeed of whether it is a "good" model or not.

From the very outset, we shall find that the study of such global aspects of model-based behavior raises new questions of a basic epistemological character. Indeed, we shall see that the utilization of predictive models for purposes of present control confront us with problems relating to causality. It has long been axiomatic that

system behavior in the present must never depend upon future states or future inputs; systems which violate this basic axiom are collectively called *anticipatory*, and are routinely excluded from science. On the other hand, the presence of a predictive model serves precisely to pull the future into the present; a system with a "good" model thus behaves in many ways like a true anticipatory system. We must thus reconsider what is meant by an anticipatory system; the suggestion arising from the present work is that model-based behavior requires an entirely new paradigm, which we call an "anticipatory paradigm", to accommodate it. This paradigm extends (but does not replace) the "reactive paradigm" which has hitherto dominated the study of natural systems, and allows us a glimpse of new and important aspects of system behavior.

The main theoretical questions with which we deal in the present work are the following: (a) What is a model? (b) What is a *predictive* model? (c) How does a system which contains a predictive model differ in its behavior from one which does not? In the process of exploring these questions, starting from first principles, we are lead to a re-examination of many basic concepts: time, measurement, language, complexity. Since the modelling relation plays a central role in the discussion, we provide numerous illustrations of it, starting from models arising entirely within symbolic systems (mathematics) through physics, chemistry and biology. Only when the modelling relation is thoroughly clarified can we begin to formulate the basic problems of model-based behavior, and develop some of the properties of systems of the kind with which we are concerned.

It is a pleasure to acknowledge the assistance of many friends and colleagues who have aided me in developing the circle of ideas to be expounded below. A primary debt is owed to my teacher, Nicolas Rashevsky, who above all set an example of fearlessness in entering territory which others thought forbidden. An equally important debt is owed to Robert Hutchins, and to the Center which he created; it was there that I was first forced to confront the nature of anticipatory behavior. A third debt is to my colleagues at the Center for Theoretical Biology: James F. Danielli, Howard Pattee, Narendra Goel, and Martynas Ycas, for their intellectual stimulation and support over the years. Gratitude must also be expressed to Dalhousie University, where thought and the leisure to think are still valued, and especially to my colleague I. W. Richardson.

# Contents

<b>Chapter 1. Preliminaries</b>	
1.1 General Introduction	1
1.2 The Reactive Paradigm: Its Basic Features	23
<b>Chapter 2. Natural and Formal Systems</b>	
2.1 The Concept of a Natural System	45
2.2 The Concept of a Formal System	55
2.3 Encodings between Natural and Formal Systems	73
<b>Chapter 3. The Modelling Relation</b>	
3.1 The Modelling Relation within Mathematics	89
3.2 Specific Encodings between Natural and Formal Systems	125
3.3 Encodings of Physical Systems	137
3.4 Encodings of Biological Systems: Preliminary Remarks	171
3.5 Specific Encodings of Biological Systems	177
3.6 Models, Metaphors and Abstractions	211
<b>Chapter 4. The Encodings of Time</b>	
4.1 Time and Dynamics: Introductory Remarks	221
4.2 Time in Newtonian Dynamics	225
4.3 Time in Thermodynamics and Statistical Analysis	233
4.4 Probabilistic Time	243
4.5 Time in General Dynamical Systems	249
4.6 Time and Sequence: Logical Aspects of Time	257
4.7 Similarity and Time	265
4.8 Time and Age	271
<b>Chapter 5. Open Systems and the Modelling Relation</b>	
5.1 General Introduction	277
5.2 Open, Closed and Compensated Systems	281
5.3 Compensation and Decompensation	289
5.4 The Main Theorem	293
5.5 Models as Closed Systems	299
5.6 The Concept of Error	307
5.7 Error and Complexity	321
5.8 Order and Disorder	325
5.9 The Stability of Modelling Relations	331

<b>Chapter 6. Anticipatory Systems</b>	
6.1 General Introduction	339
6.2 An Example: Forward Activation	349
6.3 General Characteristics of Temporal Spanning	355
6.4 An Application: Senescence	361
6.5 Adaptation, Natural Selection and Evolution	371
6.6 Learning	385
6.7 Selection in Systems and Subsystems	391
6.8 Perspectives for the Future	399
<b>Chapter 7. Appendix</b>	405
<b>Index</b>	433

## CHAPTER 1

# Preliminaries

### 1.1 General Introduction

The original germinal ideas of which this volume is an outgrowth were developed in 1972, when the author was in residence as a Visiting Fellow at the Center for the Study of Democratic Institutions in Santa Barbara, California. Before entering on the more formal exposition, it might be helpful to describe the curious circumstances in which these ideas were generated.

The Center was a unique institution in many ways, as was its founder and dominating spirit, Robert M. Hutchins<sup>1</sup>. Like Mr. Hutchins, it resisted pigeonholding and easy classification. Indeed, as with the Tao, anything one might say about either was certain to be wrong. Despite this, it may be helpful to try to characterize some of the ambience of the place, and of the remarkable man who created it.

The Center's spirit and *modus operandi* revolved around the concept of the Dialog. The Dialog was indispensable in Hutchins' thought, because he believed it to be the instrument through which an intellectual community is created. He felt that "the reason why an intellectual community is necessary is that it offers the only hope of grasping the whole". "The whole", for him, was nothing less than discovering the means and ends of human society: "The real questions to which we seek answers are, what should I do, what should we do, why should we do these things? What are the purposes of human life and of organized society?" The operative word here is "ought"; without a conception of "ought" there could be no guide to politics, which, as he often said, quoting Aristotle, "is architectonic". That is to say, he felt that politics, in the broadest sense, is ultimately the most important thing in the world. Thus for Hutchins the Dialog and politics were inseparable from one another.

For Hutchins, the intellectual community was both means and end. He said,

The common good of every community belongs to every member of it. The community makes him better because he belongs to it. In political terms the common good is usually defined as peace, order, freedom and justice. These are indispensable to any person, and no person could obtain any one of them in the absence of the community. An intellectual community is one

in which everybody does better intellectual work because he belongs to a community of intellectual workers. As I have already intimated, an intellectual community cannot be formed of people who cannot or will not think, who will not think about anything in which the other members of the community are interested. Work that does not require intellectual effort and workers that will not engage in a common intellectual effort have no place in [the intellectual community].

He viewed the Dialog as a continuation of what he called "the great conversation". In his view,

The great conversation began with the Greeks, the Hebrews, the Hindus and the Chinese, and has continued to the present day. It is a conversation that deals - perhaps more extensively than it deals with anything else - with morals and religion. The questions of the nature and existence of God, the nature and destiny of man, and the organization and purpose of human society are the recurring themes of the great conversation...

More specifically, regarding the Dialog at the Center, he said,

Its members talk about what ought to be done. They come to the conference table as citizens, and their talk is about the common good...It does not take positions about what ought to be done. It asserts only that the issues it is discussing deserve the attention of citizens. The Center tries to think about the things it believes its fellow citizens ought to be thinking about.

The Dialog was institutionalized at the Center. Almost every working day, at 11.00 am, the resident staff would assemble around the large green table to discuss a pre-circulated paper prepared by one of us, or by an invited visitor. At least once a month, and usually more often, a large-scale conference on a specific topic, organized by one or another of the resident Senior Fellows, and attended by the best in that field, would be held. Every word of these sessions was recorded, and often found its way into the Center's extensive publication program, through which the Dialog was disseminated to a wider public.

It might be wondered why a natural scientist such as myself was invited to spend a year at an institution of this kind, and even more, why the invitation was accepted. On the face of it, the Center's preoccupations were far removed from natural science. There were no natural scientists among the Center's staff of Senior Fellows, although several were numbered among the Center's Associates and Consultants; the resident population, as well as most of the invited visitors, consisted primarily of political scientists, journalists, philosophers, economists, historians, and a full spectrum of other intellectuals. Indeed, Mr. Hutchins himself, originally trained in the Law and preoccupied primarily with the role of education in society, was widely regarded as contemptuous of science and of scientists. Immediately on assuming the presidency of the University of Chicago, for instance, he became embroiled in a fulminating controversy on curricular reform, in which many of the faculty regarded his position as anti-scientific, mystical and authoritarian. At an important conference on Science and Ethics, he said, "Long experience as a university president has taught me that professors are generally a little worse than other people, and scientists are a little worse than other professors".

However, this kind of sentiment was merely an expression of the well-known Hutchins irony. His basic position had been clearly stated as early as 1931:

Science is not the collection of facts or the accumulation of data. A discipline does not become scientific merely because its professors have acquired a great deal of information. Facts do not arrange themselves. Facts do not solve problems. I do not wish to be misunderstood. We must get the facts. We must get them all...But at the same time we must raise the question whether facts alone will settle our difficulties for us. And we must raise the question whether...the accumulation and distribution of facts is likely to lead us through the mazes of a world whose complications have been produced by the facts we have discovered.

Elsewhere, he said,

The gadgeteers and data collectors, masquerading as scientists, have threatened to become the supreme chieftains of the scholarly world.

As the Renaissance could accuse the Middle Ages of being rich in principles and poor in facts, we are now entitled to enquire whether we are not rich in facts and poor in principles.

Rational thought is the only basis of education and research. Whether we know it or not, it has been responsible for our scientific success; its absence has been responsible for our bewilderment...Facts are the core of an anti-intellectual curriculum.

The scholars in a university which is trying to grapple with fundamentals will, I suggest, devote themselves first of all to a rational analysis of the principles of each subject matter. They will seek to establish general propositions under which the facts they gather may be subsumed. I repeat, they would not cease to gather facts, but they would know what facts to look for, what they wanted them for, and what to do with them after they got them.

To such sentiments, one could only say Amen. In my view, Hutchins was here articulating the essence of science, as I understand it.

However, I had more specific intellectual reasons for accepting an invitation to spend a year at the Center, as, I think, the Center had for inviting me to do so. It maybe helpful to describe them here. My professional activities have been concerned with the theory of biological systems, roughly motivated by trying to discover what it is about certain natural systems that makes us recognize them as organisms, and characterize them as being alive. It is precisely on this recognition that biology as an autonomous science depends, and it is a significant fact that it has never been formalized. As will be abundantly seen in the ensuing pages, I am persuaded that our recognition of the living state rests on the perception of homologies between the behaviors exhibited by organisms, homologies which are absent in non-living systems. The physical structures of organisms play only a minor and secondary role in this; the only requirement which physical structure must fulfill is that it allows the characteristic behaviors themselves to be manifested. Indeed, if this were not so, it would be impossible to understand how a class of systems as utterly diverse in physical structure as that which comprises biological organisms could be recognized as a unity at all. The study of biological organization from this point of view was pioneered by my Major Professor in my days as a graduate student at the University of Chicago, Nicolas Rashevsky (who, through no coincidence, idolized Robert Hutchins). Rashevsky called this study "relational biology", and we will have much more to say about it below.

The relational approach to organisms is in many ways antithetical

to the more familiar analytic experimental approach which has culminated in biochemistry and molecular biology. Particularly in these latter areas, the very first step in any analytic investigation is to destroy the characteristic biological organization possessed by the system under study, leaving a purely physical system to be investigated by standard physical means of fractionation, purification, etc. The essential premise underlying this procedure is that a sufficiently elaborate characterization of structural detail will automatically lead to a functional understanding of behaviors in the intact organism. That this has not yet come to pass is, according to this view, only an indication that more of the same is still needed, and does not indicate a fault in principle. The relational approach, on the other hand, treats as primary that which is discarded first by physico-chemical analysis; i.e. the organization and function of the original system. In relational biology, it is the structural, physical detail of specific systems which is discarded, to be recaptured later in terms of *realizations* of the relational properties held in common by large classes of organisms, if not universally throughout the biosphere. Thus it is perhaps not surprising that the relational approach seems grotesque to analytic biologists, all of whose tools are geared precisely to the recognition of structural details.

In any case, one of the novel consequences of the relational picture is the following: that many (if not all) of the relational properties of organisms can be realized in contexts which are not normally regarded as biological. For instance, they might be realized in chemical contexts which, from a biological standpoint, would be regarded as exotic; this is why relational biology has a bearing on the possibility of extraterrestrial life which is inaccessible to purely empirical approaches. Or they might be realized in technological contexts, which are the province of a presently ill-defined area between engineering and biology often called Bionics. Or, what is more germane to the present discussion, they may be realized in the context of human activities, in the form of social, political and economic systems which determine the character of our social life.

The exploration of this last possibility was, I think, the motivation behind the extending of an invitation to me to visit the Center, as it was my primary motivation for accepting that invitation. The invitation itself was extended through John Wilkinson, one of the Center's Senior Fellows, whose interest in the then novel ideas embodied in the structuralism of Levi-Strauss clearly paralleled my own concern with relational approaches in biology.

It is plain, on the face of it, that many tantalizing parallels exist between the processes characteristic of biological organisms and those manifested by social structures or societies. These parallels remain, despite a number of ill-fated attempts to directly extrapolate particular biological principles into the human realm, as embodied, for example, in Social Darwinism. Probably their most direct expression is found in the old concept of society as a "super-organism"; that the individuals comprising a society are related to one another as are the constituent cells of a multicellular organism. This idea was explored in greatest detail by the zoologists Alfred Emerson and Thomas Park, who studied insect societies, and who carefully established the striking degree of homology or convergence between social and biological organizations. (Coincidentally, both Emerson and Park were professors of zoology at the University of Chicago).

What would it mean if common modes of organization could be demonstrated between social and biological structures? It seemed to me

that, in addition to the obvious conceptual advantages in being able to effectively relate apparently distinct disciplines, there were a number of most important practical consequences. For instance, our investigation of biological organisms places us almost always in the position of an external observer, attempting to characterize the infinitely rich properties of life entirely from watching their effects without any direct perception of underlying casual structures. For instance, we may watch a cell in a developing organism differentiate, migrate, and ultimately die. We can perceive the roles played by these activities in the generation and maintenance of the total organism. But we cannot directly perceive the casual chains responsible for these various activities, and for the cell's transition or switching from one to another. Without such a knowledge of casual chains, we likewise cannot understand the mechanisms by which the individual behaviors of billions of such cells are integrated into the coherent, adaptive behavior of the single organism which these cells comprise.

On the other hand, we are ourselves all *members* of social structures and organizations. We are thus direct participants in the generation and maintenance of these structures, and not external observers; indeed it is hard for us to conceive what an external observer of our society as a whole would be like. As participants, we know the forces responsible for such improbable aggregations as football games, parades on the Fourth of July, and rush hours in large cities. But how would an external observer account for them?

It is plain that a participant or constituent of such an organization must perceive and respond to signals of which an external observer cannot possibly be aware. Conversely, the external observer can perceive global patterns of behavior which a participant cannot even imagine. Certainly, if we wish to understand the infinitely subtle and intricate processes by which biological organisms maintain and adapt themselves, we need information of both types. Within the purely biological realm, we seem eternally locked into the position of an external observer. But if there were some way to effectively relate biological processes to social ones; if, more specifically, both biological and social behaviors constituted alternate realizations of a common relational scheme, it might become possible to utilize our social experience as a participant to obtain otherwise inaccessible biological insights. Indeed, this capacity for transferring data and information from a system in which it is easy to obtain to a similar system in which it is hard to obtain is a unique characteristic of the relational approach. This was my basic hope; that I as a theoretical biologist could learn something new about the nature of organisms by judiciously exploiting the cognate properties of social systems.

The other side of that coin was equally obvious; that by exploiting biological experience, obtained from the standpoint of an external observer, we could likewise develop entirely new insights into the properties of our social systems. At that time, however, my detailed knowledge of the human sciences was essentially nil; to explore the possibilities raised above would require what appeared to me to be a major educational effort, and one which at first sight seemed far removed from my own major interests and capabilities.

It was at this point that I perceived the benefits of the community of scholars which Robert Hutchins had created. At the Center I could explore such ideas, while at the same time it was possible for me to learn in the most painless possible fashion how the political scientist, the anthropologist, the historian, and the economist each

viewed his own field and its relation to others. In short, the Center seemed to provide me with both the opportunity and the means to explore this virgin territory between biology and society, and to determine whether it was barren or fertile. I thus almost in spite of myself found that I was fulfilling an exhortation of Rashevsky, who had told me years earlier that I would not be a true mathematical biologist until I had concerned myself (as he had) with problems of social organization. At the time, I had dismissed these remarks of Rashevsky with a shrug; but I later discovered (as did many others who tried to shrug Rashevsky off) that he had been right all along.

Thus, I expected to reap a great deal of benefit from my association with the Center. But, as stressed above, the Center was an intellectual community, and to participate in it, I was expected to contribute to the problems with which the other members of that community were concerned. Initially, it appeared that I would have no tangible contribution to make to such problems as the constitutionalization of the oceans, the role of the presidency, or the press as an institution. Gradually, however, I perceived the common thread running through these issues and the others under intense discussion at the Center, and it was a thread which I might have guessed earlier. As I have noted, Hutchins' great question was: what should we do now? To one degree or another, that was also what the economists, the political scientists, the urban planners, and all the others wanted to know. However different the contexts in which these questions were posed, *they were all alike in their fundamental concern with the making of policy, the associated notions of forecasting the future and planning for it.* What was sought, in each of these diverse areas, was in effect a technology of decision-making. But underlying any technology there must be a substratum of basic principles; a science, a theory. What was the theory underlying a technology of policy generation?

This was the basic questions I posed for myself. It was a question with which I could feel comfortable, and through which I felt I could make an effective, if indirect, contribution to the basic concerns of the Center. Moreover, it was a question with which I myself had had extensive experience, though not in these contexts. For the forecasting of the future is perhaps the basic business of theoretical science; in science it is called *prediction*. The vehicle for prediction must, to one degree or another, comprise a *model* for the system under consideration. And the making of models of complex phenomena, as well as the assessment of their meaning and significance, had been my major professional activity for the preceding fifteen years. In some very real sense, then, the Center was entirely concerned with the construction and deployment of predictive models, and with the use of these predictive models to regulate and control the behaviors of the systems being modelled. Therefore, the basic theory which must underlie the technologies of policy making in all these diverse disciplines is the theory of modelling; the theory of the relation between a system and a model of that system.

This in itself was a pleasing insight. And it led to some immediate consequences which were also pleasing. For instance: why did one need to make policies in the first place? It was clear that the major purpose of policy-making, of planning, was to eliminate or control *conflict*. Indeed, in one form or another, much attention at the Center was devoted to instances of conflict, whether it be between individuals or institutions; that was what the Law, for example, was all about. In each specific case, it appeared that the roots of conflict lay not so much in any particular objective situation, but rather in the fact that differing *models* of that situation

had been adopted by the different parties to the conflict; consequently, different predictions about that situation were made by these parties, and incompatible courses of action adopted thereby. Therefore, a general theory of policy making (or, as I would argue, a general theory of modelling) would have as a corollary a theory of conflict, and hopefully of conflict resolution.

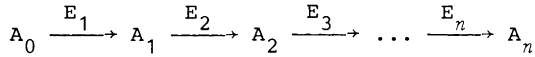
I proceeded by attempting to integrate these thoughts with my overall program, which as I noted above was to establish homologies between modes of social and biological organization. Accordingly, I cast about for possible biological instances of control of behavior through the utilization of predictive models. To my astonishment, I found them everywhere, at all levels of biological organization. Before going further, it may be helpful to consider a few of these examples.

At the highest level, it is of course clear that a prominent if not overwhelming part of our own everyday behavior is based on the tacit employment of predictive models. To take a transparent example: if I am walking in the woods, and I see a bear appear on the path ahead of me, I will immediately tend to vacate the premises. Why? I would argue: because I can *foresee* a variety of unpleasant consequences arising from failing to do so. The stimulus for my action is not *just* the sight of the bear, but rather the output of the model through which I predict the consequences of direct interaction with the bear. I thus change my *present* course of action, in accordance with my model's prediction. Or, to put it another way, my present behavior is not simply *reactive*, but rather is *anticipatory*.

Similar examples of anticipatory behavior at the human level can be multiplied without end, and may seem fairly trivial. Perhaps more surprising is the manifestation of similar anticipatory behavior at lower levels, where there is no question of learning or of consciousness. For instance, many primitive organisms are negatively phototropic; they move towards darkness. Now darkness in itself has no physiological significance; in itself it is biologically neutral. However, darkness can be *correlated* with characteristics which are not physiologically neutral; e.g. with moisture, or with the absence of sighted predators. The relation between darkness and such positive features comprises a *model* through which the organism predicts that by moving towards darkness, it *will* gain an advantage. Of course this is not a conscious decision on the organism's part; the organism has no real option, because the model is, in effect, "wired-in". But the fact remains that a negatively phototropic organism changes state in the present in accord with a prediction about the future, made on the basis of a model which associates darkness (a neutral characteristic in itself) with some quality which favors survival.

Another example of such a "wired-in" model may be found in the wintering behavior of deciduous trees. The shedding of leaves and other physiological changes which occur in the autumn are clearly an adaptation to winter conditions. What is the cue for such behavior? It so happens that the cue is not the ambient temperature, but rather is day length. In other words, the tree possesses a model, which *anticipates* low temperature on the basis of a shortening day, regardless of what the present ambient temperature may be. Once again, the adaptive behavior arises because of a wired-in predictive model which associates a shortening day (which in itself is physiologically neutral) with a *future* drop in temperature (which is not physiologically neutral). In retrospect, given the vagaries of weather, we can see that the employment of such a model, rather than a direct temperature response, is the clever thing to do.

A final example, this one at the molecular level, illustrates the same theme. Let us consider a biosynthetic pathway, which we may represent abstractly in the form



in which each metabolite  $A_i$  is the substrate for the enzyme  $E_{i+1}$ . A common characteristic of such pathways is a *forward activation* step, as for example where the initial substrate  $A_0$  activates the enzyme  $E_n$  (i.e. increases its reaction rate). Thus, a sudden increase in the amount of  $A_0$  in the environment will result in a corresponding increase in the activity of  $E_n$ . It is clear that the ambient concentration of  $A_0$  serves as a *predictor*, which in effect "tells" the enzyme  $E_n$  that there *will be* a subsequent increase in the concentration  $A_{n-1}$  of its substrate, and thereby pre-adapts the pathway so that it will be competent to deal with it. The forward activation step thus embodies a model, which relates the present concentration of  $A_0$  to a subsequent concentration of  $A_{n-1}$ , and thereby generates an obviously adaptive response of the entire pathway.

I remarked above that I was astonished to find this profusion of anticipatory behavior at all levels of biological organization. It is important here to understand why I found the situation astonishing, for it bears on the developments to be reported subsequently, and raises some crucial epistemological issues.

We have already seen, in the few examples presented above, that an anticipatory behavior is one in which a change of state in the present occurs as a function of some predicted future state, and that the agency through which the prediction is made must be, in the broadest sense, a model. I have also indicated that obvious examples of anticipatory behavior abound in the biosphere at all levels of organization, and that much (if not most) conscious human behavior is also of this character. It is further true that organic behaviors at all of these levels have been the subject of incessant scrutiny and theoretical attention for a long time. It might then be expected that such behavior would be well understood, and that there would indeed be an extensive body of theory and of practical experience which could be immediately applied to the problems of forecasting and policy-making which dominated the Center's interests. But in fact, nothing could be further from the truth. The surprise was not primarily that there was no such body of theory and experience, but rather that almost no systematic efforts had been made in these directions; and moreover, almost no one recognized that such an effort was urgently required. In retrospect, the most surprising thing to me was that I myself had not previously recognized such a need, despite my overt concerns with modelling as a fundamental scientific activity, and despite my explicit involvement with biological behavior extending over many years. Indeed, I might never have recognized this need, had it not been for the fortuitous chain of circumstances I have described above, which led me to think seriously about apparently alien problems of policy-making in a democratic society. Such are the powers of compartmentalization in the human mind.

In fact, the actual situation is somewhat worse than this. At its deepest level, the failure to recognize and understand the nature of anticipatory behavior has not simply been an oversight, but is the necessary consequence of the entire thrust of theoretical science since earliest times. For the basic cornerstone on which our entire scientific enterprise rests is the belief that events are not arbitrary, but obey definite laws which can be discovered. The search for such laws is an expression of our faith in causality. Above all, the development of theoretical physics, from Newton and Maxwell through the present, represents simultaneously the deepest expression and the most persuasive vindication of this faith. Even in quantum mechanics, where the discovery of the Uncertainty Principle of Heisenberg precipitated a deep re-appraisal of causality, there is no abandonment of the notion that microphysical events obey definite laws; the only real novelty is that the quantum laws describe the statistics of classes of events rather than individual elements of such classes.

The temporal laws of physics all take the form of differential equations, in which the rate of change of a physical quantity at any instant is expressed as a definite function of the values of other physical quantities at that instant. Thus, from a knowledge of the values of all the relevant quantities as some initial instant  $t_0$ , the values of these quantities at the succeeding instant  $t_0 + dt$  are determined. By iterating this process through an integration operation, the values of these quantities, and hence the entire behavior of the system under consideration, may be determined for all time. Carrying this picture to its logical conclusion, Laplace could say,

An intelligence knowing, at a given instant of time, all forces acting in nature, as well as the momentary position of all things of which the universe consists, would be able to comprehend the motions of the largest bodies of the world as well as the lightest atoms in one single formula...To him nothing would be uncertain; both past and future would be present in his eyes.

This picture of causality and law, arising initially in physics, has been repeatedly generalized, modified and extended over the years, but the basic pattern remains identifiable throughout. And one fundamental feature of this picture has remained entirely intact; indeed itself elevated to the status of a natural law. That feature is the following: *in any law governing a natural system, it is forbidden to allow present change of state to depend upon future states.* Past states perhaps, in systems with "memory"; present state certainly; but *never* future states. It is perfectly clear from the above discussion why such a commandment is natural, and why its violation would appear tantamount to a denial of causality in the natural world.

A denial of causality thus appears as an attack on the ultimate basis on which science itself rests. This is also the reason why arguments from final causes have been excluded from science. In the Aristotelian parlance, a final cause is one which involves a purpose or goal; the explanation of system behavior in terms of final causes is the province of *teleology*. As we shall see abundantly, the concept of an anticipatory system has nothing much to do with teleology. Nevertheless, the imperative to avoid even the remotest appearance of telic explanation in science is so strong that all modes of system analysis conventionally exclude the possibility of anticipatory behavior from the very outset.<sup>2</sup>

And yet, let us consider the behavior of a system which contains a predictive model, and which can utilize the predictions of its model

to modify its present behavior. Let us suppose further that the model is a "good" model; that its predictions approximate future events with a sufficiently high degree of accuracy. It is clear that such a system will behave *as if it were* a true anticipatory system; i.e. a system in which present change of state does depend on future states. In the deepest sense, it is evident that this kind of system will not in fact violate our notions of causality in any way, nor need it involve any kind of teleology. But since we explicitly forbid present change of state to depend on future states, we will be driven to understand the behavior of such a system in a purely *reactive* mode; i.e. one in which present change of state depends only on present and past states.

This is indeed what has happened in attempting to come to grips theoretically and practically with biological behavior. Without exception (in my experience), all models and theories of biological systems are reactive in the above sense. As such, we have seen that they *necessarily* exclude all possibility of dealing directly with the properties of anticipatory behavior of the type we have been discussing.

How is it, then, that the ubiquity of anticipatory behaviors in biology could have been overlooked for so long? Should it not have been evident that the "reactive paradigm", as we may call it, was grossly deficient in dealing with systems of this kind? To this question there are two answers. The first is that many scientists and philosophers have indeed repeatedly suggested that something fundamental may be missing if we adopt a purely reactive paradigm for consideration of biological phenomena. Unfortunately, these authors have generally been able only imperfectly to articulate their perception, couching it in terms as "will", "Geist" "élan", "entelechy", and others. This has made it easy to dismiss them as mystical, vitalistic, anthropomorphic, idealistic, or with similar unsavory epithets, and to confound them with teleology.

The other answer lies in the fact that the reactive paradigm is *universal*, in the following important sense. Given any mode of system behavior which can be described sufficiently accurately, *regardless of the manner in which it is generated*, there is a purely reactive system which exhibits precisely this behavior. In other words, any system behavior can be *simulated* by a purely reactive system. It thus might appear that this universality makes the reactive paradigm completely adequate for all scientific explanations, but this does not follow, and in fact is not the case. For instance, the Ptolemaic epicycles are also universal, in the sense that any planetary trajectory can be represented in terms of a sufficiently extensive family of them. The reason that the Copernican scheme was considered superior to the Ptolemaic lies not in the existence of trajectories which cannot be represented by the epicycles, but arises entirely from considerations of *parsimony*, as embodied for instance in Occam's Razor. The universality of the epicycles is regarded as an extraneous mathematical artifact irrelevant to the underlying physical situation, and it is for this reason that a representation of trajectories in terms of them can only be regarded as a simulation, and not as an explanation.

In fact, the universality of the reactive paradigm is not very different in character from the universality of the epicycles. Both modes of universality ultimately arise from the mathematical fact that any function can be approximated arbitrarily closely by functions canonically constructed out of a suitably chosen "basis set" whose members have a special form. Such a basis set may for

instance comprise trigonometric functions, as in the familiar Fourier expansion; the polynomials  $1, x, x^2, \dots$  form another familiar basis set. From this it follows that if any kind of system behavior can be described in functional terms, it can also be *generated* by a suitably constructed combination of systems which generate the elements of a basis set, and this entirely within a reactive mode. But it is clear that there is nothing unique about a system so constructed; we can do the same with *any* basis set. All these systems are different from one another, and may be likewise different from the initial system whose behavior we wanted to describe. It is in this sense that we can only speak of simulation, and not of explanation, of our system's behavior in these terms.

*Nevertheless, I believe that it is precisely the universality of the reactive paradigm which has played the crucial role in concealing the inadequacy of the paradigm for dealing with anticipatory systems.*

Indeed, it is clear that if we are confronted with a system which contains a predictive model, and which uses the predictions of that model to generate its behavior, we cannot claim to understand the behavior unless the model itself is taken into account. Moreover, if we wish to construct such a system, we cannot do so entirely within the framework appropriate to the synthesis of purely reactive systems.

On these grounds, I was thus led to the conclusion that an entirely new approach was needed, in which the capability for anticipatory behavior was present from the outset. Such an approach would necessarily include, as its most important component, a comprehensive theory of models and of modelling. The purpose of the present volume in fact, is to develop the principles of such an approach, and to describe its relation to other realms of mathematical and scientific investigation.

With these and similar considerations in mind, I proceeded to prepare a number of working papers on anticipatory behavior, and the relation of this kind of behavior to the formulation and implementation of policy. Some of these papers were later published in the *International Journal of General Systems*.<sup>3</sup> The first one I prepared was entitled, "Planning, management, policies and strategies: Four fuzzy concepts", and it already contained the seeds of the entire approach I developed to deal with these matters. For this reason, and to indicate the context in which I was working at the Center, it may be helpful to cite some of the original material directly. The introductory section began as follows

It is fair to say that the mood of those concerned with the problems of contemporary society is apocalyptic. It is widely felt that our social structure is in the midst of crises, certainly serious, and perhaps ultimate. It is further widely felt that the social crises we perceive have arisen primarily because of the anarchic, laissez-faire attitude taken in the past towards science, technology, economics and politics. The viewpoint of most of those who have written on these subjects revolves around the theme that if we allow these anarchies to continue we are lost; indeed, one way to make a name nowadays is to prove, preferably with computer models, that an extrapolation of present practices will lead to imminent cataclysm. The alternative to anarchy is management; and management implies in turn the systematic implementation of specific plans, programs, policies and strategies. Thus it is no wonder that the circle of ideas centering around the concept of *planning* plays a dominant role in current thought.

However, it seems that the net effect of the current emphasis on planning

has been simply to shift the anarchy we perceive in our social processes into our ideas about the management of these processes. If we consider, for example, the area of "economic development" of the underdeveloped countries (a topic which has been extensively considered by many august bodies), we find (a) that there is no clear idea of what constitutes "development"; (b) that the various definitions employed by those concerned with development are incompatible and contradictory; (c) that even among those who happen to share the same views as to the ends of development, there are similarly incompatible and contradictory views as to the means whereby the end can be attained. Yet in the name of developmental planning, an enormous amount of time, ink, money and even blood is in the process of being spilled. Surely no remedy can be expected if the cure and the disease are indistinguishable.

If it is the case that planning is as anarchic as the social developments it is intended to control, then we must ask whether there is, in some sense, a "plan for planning" or whether we face an infinite and futile anarchic regress. It may seem at first sight that by putting a question in this form we gain nothing. However, what we shall attempt to argue in the present paper is that, in fact, this kind of question is "well-posed" in a scientific sense: that it can be investigated in a rigorous fashion and its consequences explored. Moreover, we would like to argue that, in the process of investigating this question, some useful and potentially applicable insights into planning itself are obtainable.

After a brief review of the main technical concepts to be invoked (which was essential for the audience at the Center) I then proposed a specific context in which anticipatory behavior could be concretely discussed:

We are now ready to construct our model world, which will consist of a class of systems of definite structure, involving anticipation in an essential way, and in which the fuzzy terms associated with "planning" can be given a concrete meaning.

Let us suppose that we are given a system  $S$ , which shall be the system of interest, and which we shall call the *object system*.  $S$  may be an individual organism, or an ecosystem, or a social or economic system. For simplicity we shall suppose that  $S$  is an ordinary (i.e. non-anticipatory) dynamical system.

With  $S$  we shall associate another dynamical system  $M$ , which is in some sense a *model* of  $S$ . We require, however, that if the trajectories of  $S$  are parameterized by real time, then the corresponding trajectories of  $M$  are parameterized by a time variable which goes faster than real time. That is, if  $S$  and  $M$  are started out at time  $t = 0$  in equivalent states, and if (real) time is allowed to run for a fixed interval  $T$ , then  $M$  will have proceeded further along its trajectory than  $S$ . In this way, the behavior of  $M$  *predicts* the behavior of  $S$ ; by looking at the state of  $M$  at time  $T$ , we get information about the state that  $S$  will be in at some time later than  $T$ .

We shall now allow  $M$  and  $S$  to be coupled; i.e. allow them to interact in specific ways. For the present, we shall restrict ourselves to ways in which  $M$  may affect  $S$ ; later we shall introduce another mode of coupling which will allow  $S$  to affect  $M$  (and which will amount to updating or improving the model system  $M$  on the basis of the activity of  $S$ ). We shall for the present suppose simply that the system  $M$  is equipped with a set  $E$  of *effectors*, which allow it to operate either on  $S$  itself, or on the environmental inputs to  $S$ , in such a way as to change the dynamical properties of  $S$ . We thus have a situation of the type diagrammed in Figure 1.