CYBERNETICS AND ECONOMICS

A Thesis

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by

Arthur S. Gillman

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PREFACE

This thesis attempts to collect certain contemporary work from fields not often regarded by the economist as belonging to his professional discipline, and to limn some topics of economics with this synthetic view. The resulting synthesis, as presented here, is but a preface to the vastly more detailed project of systematic exploration that will have to be done before even a meaningful evaluation of cybernetics' impact upon the social sciences can emerge.

The author is especially grateful to the chairman of his Examining Committee, Professor H. C. Pentland, for encouragement and helpful discussion. But as is customary (and, of course, right), the author accepts full responsibility for whatever defects the present work may contain.

A. S. Gillman

The University of Manitoba,
Winnipeg, Manitoba, Canada.

September, 1966.

CYBERNETICS AND ECONOMICS

<u>Thesis</u>:

The thesis of the present work is that the problem of <u>control</u> is of central importance to the scientific study of socio-economic phenomena; that the control problem is at bottom a problem about the efficient use of constrained resources; and that these propositions have some important consequences for social-science theory generally and economics particularly.

TABLE OF CONTENTS

CYBERNETICS AND ECONOMICS

". . . the development of adequate cybernetic mechanisms for a free economy, which will not achieve stability only at the cost of tyranny or stagnation, is a project of the first priority, exceeded in urgency perhaps only by the necessity of developing similar cybernetic mechanisms for the stabilization of peace."

—Kenneth Boulding[1]

"I am a human being; do not fold, bend, or mutilate."

—Max Ways[2]

CHAPTER I

PREDICTIVE POWER AND THE LOGIC OF CONTROL

1. What is Cybernetics?

In the summer of 1947 the word cybernetics came into the English language.[3] It was coined by a group of academicians around Norbert Wiener, the eclectic professor of mathematics at the Massachusetts Institute of Technology, who had over the preceding four years organized an interdisciplinary effort to unify under a distinct discipline certain important problems and results common (albeit in different jargons) to several fields as diversely separate as "electrical engineering, neurophysiology, physics, biology, with even a dash of economics."[4] Wiener writes of the occasion:[5]

> . . . the group of scientists about Dr. Rosenblueth and myself had already become aware of the essential unity of the set of problems centering about communication, control, and statistical mechanics, whether in the machine or in living tissue. On the other hand, we were seriously hampered by the lack of unity in the literature concerning these problems, and by the absence of any common terminology, or even of a single name for the field. After much consideration, we have come

> to the conclusion that all existing terminology has too heavy a bias to one side or another to serve the future development of the field as well as it should; and as happens so often to scientists, we have been forced to coin at least one artificial neo-Greek expression to fill the gap. We have decided to call the entire field of control and communication theory, whether in the machine or in the animal, by the name Cybernetics, which we form from the Greek . . . steersman.

On Wiener's definition, cybernetics is thus "the entire field of control and communication theory, whether in the machine or in the animal." Apart from the special sense in which the terms "control" and "communication" are construed here, the question that Wiener's definition might provoke for a reader of the present work is: "What has cybernetics to do with economics, which is neither a machine nor an animal?"

This question is of course natural and legitimate. It has two answers. First, the economy is a machine. This is not to say merely that an economic system behaves like a machine, is analogous to a machine, lends itself to a machine metaphor, etc.--but that an economic system is a machine. That proposition is an important part of the present thesis. And, though it may be bad form to anticipate later expository material (particularly the content of Chapter II) it is surely germane to quote here the words of Sir Stafford Beer explaining what a "machine" is:[6]

"The machine is a set of states undergoing transformations . . ."

Second, Wiener's definition of cybernetics, though obviously authoritative, appears not to have settled usage of the term "cybernetics." In 1966, fully 19 years after Wiener's definition, it is still possible to find passages like this in a respected scientific publication:[7]

> In the United States there is no general agreement among scientists about what the subject of cybernetics should include, and it is not likely that a student could obtain an advanced degree in "cybernetics".

And Beer says,[8] accurately if amusingly:

> Some people think that cybernetics is another word for automation; some that it concerns experiments with rats; some that it is a branch of mathematics; others that it wants to build a computer capable of running the country.

To the extent that usage of the term "cybernetics" has not yet crystallized it is possible (and even legitimate) to place economic phenomena within the purview of cybernetics. However, in the present paper "cybernetics" will be employed more or less in Wiener's sense, with perhaps a stronger emphasis on the _control_ aspect as distinguished from the _communication_ aspect of cybernetic processes, though these are certainly not dichotomous. Wiener writes:[9]

> In giving the definition of Cybernetics . . . I classed communication and control together. Why did I do this? When I communicate with another person, I impart a message to him, and when he communicates back with me he returns a related message which contains information primarily accessible to him and not to me. When I control the actions of another person, I communicate a message to him; although this message is in the imperative mood, the technique of communication does not differ from that of a message of fact. Furthermore, if my control is to be effective I must take cognizance of any messages from him which may indicate that the order is understood and has been obeyed.

No bald definition of "cybernetics" is apt at this point to convey the full meaning of that term as it will be employed in the present work. Yet this question of full meaning is very important. It is well, therefore, to dwell at length on cybernetics' _usage_ by way of analogy to that technique of definition which the philosopher characterizes as "ostensive."[10] It is in this sense instructive to note that the Greek word _steersman_ (or _helmsman_) which yields _cybernetics_, yields also the English _governor_, _government_, etc. Further, is it instructive to learn from Cherry[11] that Wiener and Rosenblueth

> referred to this general study as cybernetics, from the word κυβερνήτης (a "steersman") a word first used by André Ampère in the form cybernétique, in his "Essai sur la philosophie des sciences," 1834, to mean the "science of government or control."

The Nerves of Government is what Deutsch, the eminent political scientist, titles a work devoted mainly to exploring cybernetics' impact upon political science.[12]

> This book concerns itself less with the bones or muscles of the body politic than with its nerves--its channels of communication and decision.
>
> . . . .
> It suggests that it might be profitable to look upon government somewhat less as a problem of power and somewhat more as a problem of steering; and it tries to show that steering is decisively a matter of communication.

Later in the same work[13] Deutsch says:

> Cybernetics, the systematic study of communication and control in organizations of all kinds, is a conceptual scheme on the "grand scale," in J. B. Conant's sense of the term. Essentially, it represents a shift in the center of interest from drives to steering, and from instincts to systems of decisions, regulation, and control, including the non-cyclical aspects of such systems.

"Cybernetics, the systematic study of communication and control in organizations of all kinds," is surely what Kenneth Arrow is doing when he writes:[14]

> An organization is a group of individuals seeking to achieve some common goals, or, in different language, to maximize an objective function. Each member has objectives of his own, in general not coincident with those of the organization. Each member also has some range of decisions to make within limits set partly by the environment external to the organization, and partly by the decisions of other members. Finally, some but not all observations about the workings of the organization and about the external world are communicated from one member to another.
>
> . . . I wish to set forth some considerations on one aspect of the workings of an organization--how it can best keep its members in step with each other to maximize the organization's objective function. This may be referred to as the problem of organizational control.

One can add a further dimension to the notion of cybernetics by placing it in historical perspective. If one examines the broad unfolding of human intellectual history, one readily discerns alternating periods of synthesis and analysis. Aristotle was a prodigious unifier; and the Aristotilean synthesis elicited a centuries-long campaign of analysis that continued to the verge of sterility. Then came the Copernican revolution, which culminated in the monumental synthesis of Isaac Newton. In physics, Newton's followers analyzed and analyzed until they came to the impasse that triggered a new, Einsteinian synthesis. From such a perspective cybernetics represents simply contemporary synthesis—in this case built around the unifying notions of control and (in a special sense of the word, presently to be explained) communication.

The introduction of these notions, and of cybernetics generally into the social sciences is of great importance, historically. To understand why, we may turn next to the question, "How does economic cybernetics differ from traditional economics?"

2. How Cybernetics-*cum*-Economics Differs from Traditional Economics.

An entirely satisfactory definition of traditional economics does not exist. The venerable *Oxford Dictionary*[15] defines *economics* as "the science of household, rural, and *esp.* political economy," where economy means "house management," and *political economy* is "the art of managing the resources of a people and of its government (Adam Smith); later, "the theoretical science of the laws of production and distribution of wealth." Marshall writes:[16]

> Political Economy or Economics is a study of mankind in the ordinary business of life; it examines that part of individual and social action

> which is most closely connected with the use of the material requisites of well being.

And Samuelson says:[17]

> Economics is the study of how men and society choose, with or without the use of money, to employ scarce productive resources to produce various commodities over time and distribute them for consumption, now and in the future, among various people and groups in society.

Practically the only generalization warranted by these diverse attempts to define economics is that a distinct discipline, economics, does indeed exist. This may be considered the least controversial statement, and the broadest, which it is possible to make about economics from the traditional viewpoint. By contrast, the contemporary view is not so certain that the old fences which divided the social sciences into the various fields of psychology, political science, economics, sociology, etc., were wisely placed; or even that their sheer existence is helpful. Ironically, this latter view results from the very multiplication of fences that social scientists have wrought over the past, say, fifty years.

Uncertainty about the boundaries of the social sciences has numerous sources, each offering unique insight into the very phenomena that social scientists explore. One source is the realization that, to a substantial degree, the apparent orderliness of empirical phenomena is as much due to man's perceptual apparatus as to a property inhering in these phenomena. This insight is generally attributed to Kant,[18] was importantly extended from empirical to ideal systems (e.g. mathematics) by Whitehead and Russell,[19] and appears even to have attracted experimental confirmation, according to Kenneth Boulding:[20]

> Alexander Bavelas, a social psychologist at Stanford University, has reported orally on an experiment in which he gave to a number of subjects

> sets of random data—random, that is, from the experimenter's point of view—and asked them to find the rule in them. Almost without exception, the subjects were able to find rules and order in the random data given them; and what is more, when they were informed after the experiment that there were, in fact, no rules, they became quite angry and insisted that the rules that they thought they had discovered must, in fact, be true.

Well might one heed Ackoff's advice to "stop acting as though nature were organized into disciplines in the same way that universities are."[21]

A second source for contemporary uncertainty about the boundaries of the autonomous discipline, economics, is the accelerating fragmentation of knowledge. This has led naturally to the need of synthesis. This need is no less prominent in the social sciences than elsewhere, and (as mentioned earlier) furnishes the basis for cybernetics. Inevitably in any such new synthesis the geography of the social sciences is bound to change. Old landmarks will be merged, and in many cases disappear.

Of the present fragmentation of knowledge, Wiener has written:[22]

> 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. A century ago there may have been no Leibniz, but there was a Gauss, a Faraday, and a Darwin. 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 the 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.
>
> These specialized fields are continually growing and invading new territory. The result is like what occurred when the Oregon country was being invaded simultaneously by the United States settlers, the British, the Mexicans, and the Russians—an inextricable tangle of exploration, nomenclature, and laws. There are fields of scientific work, as we shall see in the body of this book, 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.

Wiener's illustrations mainly come from the biological and the natural sciences; but these have no monopoly on the process by 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." Fragmentation of knowledge plagues the social sciences, too. Evidence abounds. The problem of operations control in the firm, with which traditional accountancy wrestles afresh (and more or less fruitlessly) when each new generation of accountants takes it up, was for all practical purposes solved in the now classical papers of Abraham Wald;[23] but accountants lack the theoretical equipment to understand the solution, while mathematicians lack the training and experience to recognize this important application of Wald's work, and the twain simply have not met. The same remarks apply to the intellectual barrier that separates the industrial cost-accountants' endless debate about how to allocate overhead burden, from certain of the economists' results in distribution theory, particularly the marginal opportunity-cost concept which has in linear models of constrained resource allocation been colourfully named "shadow pricing." Doubtless the reader could multiply these two illustrations many times over.

A third source for contemporary uncertainty about the boundaries of economics is the movement towards synthesis ascribed earlier to the problems of fragmentation. This synthetic tendency envelopes all the social sciences, urging their unification. Boulding writes:[24]

> It is a sympton of the way in which the social sciences are moving toward unification that these days it is often quite hard to tell whether a book such as, for instance, Kenneth Arrow's Social Choice and Individual Values, is economics or political science. We could almost say that the division of the social sciences according to fields of study or according to the types of institutions studied--with, for instance, a political scientist studying states, an economist, corporations, and a sociologist, families and churches--is now breaking down, indeed has broken down. It has become clear that each social science concentrates on a certain aspect of the social system which cuts across virtually all forms of social organization, even though it may be particularly relevant to some of them. Thus the processes of decision-making are quite similar, whether they take place in a corporation, in the government, in a labor union, in a church, or in a family. If the decision has to be reached in a group or has to be accepted by a group, there are problems of compromise, accommodation, reformulation, and development of new positions which likewise take place no matter what the organization . . . we seem to be getting a specialization according to certain functional processes such as decision-making, the resolution of conflict, processes of exchange, processes of threats and coericion, and so on.

An economist might be excused for perceiving, in the proliferation described by Boulding, not so much the dissolution of economics as an autonomous discipline, as the enrichment of economics on a near-renaissance scale. But this does indeed seem to be the case. It is natural in these circumstances that the scope and content of economics should be changing, rapidly and radically, metamorphizing "traditional" economics (on whatever definiton) almost beyond recognition. This metamorphosis comprises the broadest contrast between the traditional economics and the new. The economist in the age of cybernetics contemplates a wider professional domain than his traditional predecessor would have dared. Each of the emerging social-science specialties (and more) named by Boulding lies in this domain. Each is amply, even brilliantly represented in the contemporary literature of economics: Decision-making, by the now classic work of Luce and Raiffa, Games and Decisions,[25] or the more recent work by Fishburn, sponsored by the Operations Research Society of America, Decision and Value Theory;[26] conflict-resolution, by von Neumann and Morgenstern's Theory of Games and Economic Behavior,[27] and Boulding's own Conflict and Defense;[28] processes of exchange, by Buchanan and Tullock's The Calculus of Consent,[29] or Blau's Exchange and Power in Social Life; processes of threats and coercion by Schelling's Strategy of Conflict;[31] and so on.

In summary: the "new" economics differs from economics traditionally construed, first of all, on the very notion of how the social sciences ought to be partitioned, and particularly on the question of which social phenomena do, and which do not, legitimately comprise the economist's professional responsibility. More and more, strategic areas in the non-economics social sciences are being absorbed into concepts under which a new synthesis of the social sciences is emerging; and these concepts belong, at least potentially, to economics.[32]

This first point of contrast is rather general, and while true of the difference between cybernetic economics and the traditional economics, it is true equally of certain contemporary developments which are not characteristically cybernetic, when these latter are contrasted with traditional economics.[33] This can hardly be said of the second point of contrast: the abandonment, in cybernetic economics, of the Newtonian determinism that dominated traditional economics.

Newton's stunning success in physics inevitably influenced the social sciences, and led to their largely uncritical acceptance of the philosophy behind Newtonian methodology. Here it is important to distinguish between the Newtonian methodology on one hand, and its philosophical foundations on the other. Economists' use of the Calculus, which Newton co-discovered with Leibniz, and which played the major role in Newton's system of physics, led by von Thünen,[34] sought to rationalize their discipline with Newton's powerful tools. From technical considerations, the differential (i.e., "infinitesimal") and the integral Calculus have very important short-comings in dealing with the problems of economic analysis, and have accordingly been largely displaced, in the cybernetic approach to the social sciences, with the so-called finite methods.[35] This is an important change, to be sure. But of far deeper importance is the change at the philosophical level.

> There was, actually, an important statistical reservation implicit in Newton's work, though the eighteenth century, which lived by Newton, ignored it. No physical measurements are ever precise; and what we have to say about a machine or other dynamic system really concerns not what we must expect when the initial positions and momenta are given with perfect accuracy (which never occurs) but what we are to expect when they are given with attainable accuracy. This merely means that we know, not the complete initial conditions, but something about their distribution. The functional part of physics, in other words, cannot escape considering uncertainty and the contingency of events.[36]

Similarly, no measurement of socio-economic phenomena is ever precise. Therefore, the prescriptions that the economist may deduce from some resource-allocation paradigm or other apply not to a (socio-economic) system whose "initial conditions" and "momenta" are given with perfect accuracy--i.e., the kind of system alone for which prescriptions are <u>logically</u> valid--but instead to a system whose initial conditions, momenta, etc., are given <u>probabilistically.</u> This probabilistic picture comes from a stream of data that the economist collects according to some formal scheme which reflects his willingness to tolerate a particular mixture of Type I and Type II error (cf. sec. 4, below); and as the "real" phenomena underlying this picture change significantly, the economist must be in a position to detect such changes promptly, to amend his picture accordingly, and as necessary also to amend his prescriptions. (This process is discussed in greater detail below, sec. 3 and sec. 4). Thus while the economy on the traditional view is a deterministic machine in the sense normally associated with Newton,[37] the economy on the cybernetic view is a probabilistic machine, contrasting with the traditional view in much the same way that the statistical mechanics of Gibbs and his successors contrasts with the Newtonian mechanics of physics.

A further, important contrast between the Newtonian-oriented traditional models of economics and the newer, cybernetic models, is this: whereas the former are invariant with respect to changes in the direction of time, the latter are not, and thus come much nearer to a realistic description of socio-economic phenomena. Cherry makes this point[38] with admirable lucidity:

> Communication provides an example of a process which we regard as proceeding from the past into the future; time, we say, "has a direction." Phonograph records played backward sound as senseless gibberish. A movie, in reverse, produces comic results--a diver rising from the water, landing on tiptoe; torn scrap paper coming together into folded news sheets; a drinker regurgitating a pint of beer into a glass. The world, run backward, looks ludicrous.
>
> Yet Newton's laws of motion--the backbone of physical science--are reversible; time can have a positive or negative sign. We appear then to regard time in two distinct ways, reversibly and irreversibly. On one hand, if we study, say, the properties of some simply frictionless machine containing relatively few moving parts, we can calculate its precise motions, in detail; we may learn all about it and predict its future behavior with accuracy. In the equations of such mechanical motions, the sign of time may be everywhere reversed, with complete consistency. On the other hand there are whole realms wherein the "direction" of time is of major importance. . . . Mechanical analogy forms a basis for a great deal of our thinking. In the social field, "forces" are not the forces of mechanics, nor are social groups to be

> compared with machines in the Newtonian sense. For, in simple mechanics, time can be reversed; but we cannot reverse the course of history.

On the traditional, Newtonian-oriented view, the economy is like an exceedingly complex clockworks. Various shortcomings of this view—or at least contrasts between it and the cybernetic view—have been discussed above. They lead to a further contrast which is at once a consequence of the earlier ones, and yet an important characteristic of its own. This is a difference on the question of what comprises a system, and frequently is referred to as the mechanism-*versus*-functionalism controversy.[39] Krupp's remarks[40] on this controversy are a useful first approximation to a clear contrast between (Newtonian-oriented) mechanism and its contemporary successor:

> In mechanical theories the parts are assumed to be independent entities which are combined according to special rules to yield aggregates. These aggregates obey the same general laws that apply to the parts. Functional analysis, on the other hand, starts with the unit that goals give to the system. Functional theories postulate a general purpose for the system, and proceed to discriminate the component parts and subgoals of the parts. The goal of the system acts to bind the components in the same way that the general law and the rules of composition act to relate the variables of mechanistic systems. Thus, the goal of a functionalist theory is more than one postulate among other postulates. It gives the system a general direction and exerts a pulling force on all the components and functions within the system. The parts are related to each other through their goal-fulfilling properties. Changes in the units will usually be analyzed in terms of their differential consequences to the attainment of the system's goal.
> . . . .
> Functionalist theory focuses on the unity and directedness of a total system, while mechanistic theory tends to concentrate on the precise determination of the relationship between parts of a system. Functionalist theory assumes a system to have a basic organizing principle of goals and self-regulating mechanisms. Mechanistic theory takes a system to be derived from the relationships between the parts.

Lest the characterization of functionalism that emerges from Krupp's pen appear somewhat metaphysical, the following more precise statement by Nagel[41] is of value:

> . . . once a system S and a state G supposedly maintained in it are adequately specified, the task of the functionalist is to identify a set of state variables whose operations maintain S in the state G, and to discover just how these variables are related to each other and to other variables in the system or its environment.

On the functionalist view of systems, in contradistinction to the mechanistic view, the economy is a system which, qua system, is something more than just the sum of its parts. In the first place, the economic system is so complex (i.e., consists of so very many parts) that any method of studying the economy which demands keeping track of all the parts in order to "grasp" the system is a practical impossibility. Hence the modern emphasis on statistical, as opposed to Newtonian mechanics.

> In view of the necessary abstraction, and of the great residue of uncertainties facing us in analysis of material so varied and so numerous as human populations, it would seem that statistical mechanics may be more relevant and applicable than ordinary (determinate) mechanics; . . . Ordinary mechanics deals with simple rigid bodies like levers, wheels, frameworks, and with their motions and the various forces in equilibrium which act upon them. . . . On the other hand, statistical mechanics deals with the properties of systems consisting of such enormous assemblages of component elements (such as a volume of gas) that exact determinate calculations become impossible. It abstracts certain macroscopic properties and ignores other data entirely, so that the life history of the system cannot be specified precisely, but only statistically. . . . Today the principal concepts are finding application in many fields where vast assemblages of "systems" are studied.[42]

In the second place, the economic system cannot, as can the naive mechanical systems upon whose pattern most traditional models in economics rest, be run in reverse. In it change is not generally reversible. In the third place, an economic system may not, as in the case of a clockworks system, be disassembled and then reassembled with the expectation that it will function after reassembly much as it did before dismemberment. In the fourth place, the economic system is intrinsically subject to random disturbances that leave in their wake a residual pattern frequently denoted as evolution or "growth" in some progressive sense, while the mechanistic system and the economic models it spawns are free from such aberrations. Clearly, then, the functional view of systems is radically different from the traditional view on which, too, the economy is nominally a "system."

Functionalism dominates cybernetic economics. It is, therefore, necessary to dwell at some length on this topic, and in particular to address three problems which, ignored, might impair succeeding material. The first problem concerns a clear delineation between (a) any particular function, and (b) an instrument through which that function operates; the second problem concerns the defence of functionalism against the charge of intrinsic "status-quo-ism"; the third problem concerns empirical evidence in the literature of economics in support of functionalism.

With regard to the first problem, it would be hard to improve upon this statement of Nagel:[43]

> It is . . . crucial . . . to distinguish between the function or type of activity exercised by a particular variable in a system, and the variable that exercises this function. Thus, one of the functions of the thyroid glands in the human body is to help preserve the internal temperature of the organism. However, this is also one of the functions of the adrenal glands, so that in this respect there are at least two organs in the body that perform (or are capable of performing) a similar function. Accordingly, although the maintenance of a steady internal temperature may be indispensable for the survival of human organisms, it would be an obvious blunder to conclude that since the thyroid glands contributed to this maintenance they are for this reason indispensable for the continuance of human life. Indeed, there are human beings who, as a consequence of surgical intervention, do not have thyroid glands, but nevertheless remain alive. An identical point requires to be made in the context of social inquiry. Let us assume that one of the functions of a church organization in a given society is to foster religious sentiments and religious activities. However, this function may also be exercised by other institutionalized groups in that society, for example, by individual families or by schools. Moreover, even if

these other organizations did not actually perform this function at a given time, they might acquire it at some later time under appropriate circumstances. In consequence, even if it were beyond dispute that religious attitudes and activities are essential for the welfare of human societies, it would not follow that church organizations are indispensable for that welfare.

With regard to the second problem, it is again convenient to quote Nagel:[44]

> It is . . . quite easy to overlook the requirement that the system S and the state G with which the analysis presumably deals must be carefully delimited, and in consequence to omit explicit mention, in the teleological explanation finally proposed, of the specific system within which the variable allegedly maintains a specific state. It is then also easy to forget that even if the variable does have the function attributed to it of preserving G in S (e.g., the performance of a religious ritual having the function of maintaining the state of emotional solidarity of <u>each</u> primitive tribe in which the ritual takes place), it may not have this role in some <u>other</u> system S' (e.g., in a confederation of tribes, where the ritual may have a divisive force) to which the variable may also belong; or that it may not have the function of maintaining in the same system S some <u>other</u> state G' (e.g., an adequate food supply), with respect to which it may perhaps be dysfunctional by obstructing the maintenance of G' in S.
>
> But however this may be, it is hardly possible to overestimate the importance for social sciences of recognizing that the imputation of a teleological function to a given variable must always be <u>relative</u> to some particular state in some particular system, and that, although a given form of social behavior may be functional for certain social attributes, it may also be dysfunctional (or even nonfunctional, in the sense of being causally irrelevant) for many others. Failure to recognize this point, obvious though it is when stated formally, is undoubtedly a major source for the not uncommon confusion of questions of fact with questions of desirable social policy, as well as the frequent accusation that a functional approach in social science is <u>necessarily</u> committed to the values embodied in the social <u>status quo</u>. With this point in mind, however, even if individual functionalists are so committed, it will be evident that the accusation that such commitment is inherent to functionalism is baseless.

The third problem concerns empirical evidence in the literature of economics in support of functionalism. This problem may reasonably be construed in two different senses. In the first sense, it is the problem of showing that the contemporary literature of economics is indeed much concerned with functionalist-oriented economics. In the second sense, it is the problem of showing that the latter genre of economic theory enjoys empirical confirmation.

In the first sense, the problem is hardly a problem; and to anyone even slightly familiar with the contemporary literature a mere allusion to works of pure mathematical economics (which, being entirely formal and without content, are *ipso facto* paradigms of functionalism in economics) will suffice.

In the second sense, many economists' preoccupation with methodological functionalism has led to an emphasis on behaviourism whose models have been spectacularly successful on empirical test. One rather colourful name for this approach is *black box* theory (*cf*. Ch. IV, below). Dantzig, for example, writes:[45]

> Suppose that the system under study . . . is a complex of machines, people, facilities, and supplies. It has certain overall reasons for its existence. For the military it may be to provide a striking force, or for industry it may be to produce certain types of products.
>
> The linear programming approach is to consider a system as decomposable into a number of elementary functions, the *activities*. An activity is thought of as a kind of "black box" (fn.: "Black box: any system whose detailed internal nature one willfully ignores.") into which flow tangible inputs, such as men, material, and equipment, and out of which may flow the products of manufacture, or the trained crews of the military. What happens to the inputs inside the "box" is the concern of the engineer or the educator; to the programmer, only the rates of flow into and out of the activity are of interest.

Similarly, in the behaviouristic school, behaviour is what counts, as do behavioural relationships (which are generally represented as either mathematical or computer-program models); while the internal nature of the system responsible for these behavioural patterns is, in Dantzig's succinct phrase, "willfully ignored." As to the success of this approach, on empirical testing, Clarkson's model of portfolio selection may be cited as an almost astonishing example.[46] Clarkson attempted a computer simulation of investment trust officer behaviour in selecting securities for clients' portfolios.

> This process involves decision making under uncertainty. Our model, written as a computer program, simulates the procedures used in choosing investment policies for particular accounts, in evaluating the alternatives presented by the market, and in selecting the required portfolios. The analysis is based on the operations at a medium-sized national bank and the decision maker of our model is the trust investment officer. We require our simulation model to select portfolios using the same information that is available to the trust officer at the time his decisions are made.[47]

As to the achievements of Clarkson's model, he presents the following results.[48]

<u>Simulation of Account 1 (1/8/60)</u>

<u>Growth Account</u>

Funds available for investment: $22,000.

The PROGRAM selected the following portfolio	The portfolio selected by the TRUST OFFICER was
60 General American Transportation	30 Corning Glass
50 Dow Chemical	50 Dow Chemical
10 I.B.M.	10 I.B.M.
60 Merck and Company	50 Merck and Company
45 Owens Corning Fiberglas	50 Owens Corning Fiberglas

<u>Simulation of Account 2 (6/10/60)</u>

<u>Income and Growth Account</u>

Funds available for investment: $37,500.

The PROGRAM selected	The TRUST OFFICER selected
100 American Can	100 American Can
100 Continental Insurance	100 Continental Insurance
100 Equitable Gas	100 Equitable Gas
100 Duquesne Light	100 General Public Utilities
100 Libbey Owens Ford	100 Libbey Owens Ford
100 International Harvester	50 National Lead
100 Philadelphia Electric	100 Philadelphia Electric
100 Phillips Petroleum	100 Phillips Petroleum
100 Socony Mobil	100 Socony Mobil

Simulation of Account 3 (7/8/60)

Income and Growth Account

Funds available for investment: $31,000.

The PROGRAM selected	The TRUST OFFICER selected
100 American Can	100 American Can
100 Continental Insurance	100 Continental Insurance
100 Duquesne Light	100 Duquesne Light
100 Equitable Gas	100 Equitable Gas
100 Pennsylvania Power and Light	100 General Public Utilities
100 International Harvester	100 International Harvester
100 Libbey Owens Ford	100 Libbey Owens Ford
100 Socony Mobil Oil	100 Socony Mobil Oil

Simulation of Account 4 (8/26/60)

Income Account

Funds available for investment: $28,000.

The PROGRAM selected	The TRUST OFFICER selected
100 American Can	100 American Can
100 Continental Insurance	100 Continental Insurance
100 Duquesne Light	100 Duquesne Light
100 Equitable Gas	100 Equitable Gas
100 Pennsylvania Power and Light	100 General Public Utilities
100 International Harvester	100 International Harvester
100 Phillips Petroleum	100 Phillips Petroleum

3. Predictive Power, and Control Processes.

It is the thesis of this section that demand for "predictive power" in scientific models--which is a legitimate demand--is in fact a demand that scientific models should facilitate "control" of the modelled processes.

Nowadays it is a methodological platitude to say that predictive power is the touchstone of scientific worth in economic models. If ones model can predict, it is scientifically acceptable; otherwise it is not. Dissent from this view is exceptionally difficult psychologically, for it gives the dissenter the appearance of not desiring predictive power in scientific models.

In this sense no dissent is intended here: if the power of foretelling events could somehow descend from the gods into models of economic processes one should rejoice indeed.

Consider the implications of predictive power literally construed. Literally, from its Latin root, to predict means to say beforehand, i.e., to make a statement about the way something will happen before it has happened. Such a statement has the logical form of an antecedent-consequent coupling--"If (a specified set of antecedent conditions, then (a set of specified consequent conditions)." In turn, this requires a deterministic causal connection between the antecedent and consequent conditions. Therefore, predictive power, literally construed, depends on a species of deterministic causality, as just described.

But deterministic causality of this kind is logically insupportable. To see this, consider how one could tell whether any statement possessing the form of an antecedent-consequent coupling--"If A, then B"--were true or false. For expository convenience the letter "A" here can stand for a set of statements which comprise some economic model, while the letter "B" stands for a set of consequences--or predictions--which have been deduced from, and which, therefore, are implied by, the model, "A".

Truth or falsity can be taken in two distinct senses here: logical, and empirical. In the logical sense a statement of the form, "If A, then B" is false just when its antecedent member is true and its consequent member false; and it is true in every other case.[49] Thus, if "A" is true by way of being a set of logically consistent theorems in (say) welfare economics, and "B" is a particular set of deductions about the distribution of national income which is self-contradictory (e.g., that income distribution both is and is not Pareto-optimal), then the statement "If A, then B" is formally false, and represents defective theory. In the literature of economics, theory which proves defective on this test is by no means either rare or unimportant (in the sense that mere quibbling is often unimportant). For example, much traditional welfare theory appears to have been irretrievably compromised by Arrow's demonstration that community preference behaviour (e.g., as reflected in voting) can lack transitivity.[50]

In the empirical sense a statement of the form "If A, then B" is, as in the logical sense, false just when its antecedent member is true and its consequent member false, and true in every other case. But when the consequent "B" depends for its truth on empirical evidence, and not just on the rules of formal logical consistency, then the statement "If A, then B" is a prediction in the sense of "prediction's" general usage, and the model "A" is opened to empirical test. If "B", the set of predictions, turns out to be false, then one must either amend or scrap ones model, for in these circumstances the only way of preserving the truth of the statement "If A, then B" is to falsify the antecedent "A", i.e., to acknowledge that the model which "A" represents is false.[51]

Suppose an economist makes a prediction statement of the form "If A, then B", and "B", on empirical test, turns out to be true. Does this establish the truth of his model, "A"? Clearly it does not. For, recall that, by the established results of formal logic, any statement of the family "If A, then B" is false just when it has both a true antecedent and a false consequent, and is true in every other case. It is helpful at this point to array all possible truth-value combinations of "A" and "B", to give the expression, "in every other case," greater clarity.

TABLE I

ALL POSSIBLE TRUTH-VALUE COMBINATIONS
FOR THE STATEMENT: "IF A, THEN B."

Case (1): "A" is true, and "B" is true

Case (2): "A" is true, and "B" is false

Case (3): "A" is false, and "B" is true

Case (4): "A" is false, and "B" is false

In terms of Table I, case (2) is the only combination that renders "If A, then B" false, cases (1), (3), and (4), being considered true (i.e., non-false).[52] It is instructive to invent a concrete interpretation of Table I. Let "A" stand for the statement, "There is unemployment."[53] Let "B" stand for the statement, "Economic growth is retarded." Then, in case (1) a hypothetical economist is saying: "If there is unemployment, then economic growth is retarded," in circumstances where unemployment does indeed exist and economic growth is indeed retarded; and in these circumstances his utterance, "If there is unemployment, then economic growth is retarded" is a true proposition. In case (2), where unemployment coexists with an economic growth rate that all agree is not retarded, it is false to say: "If there is unemployment, then economic growth is retarded." In case (3), where full employment coexists with retarded growth, clearly it is not false to assert that "If there is unemployment, then economic growth is retarded", for retarded growth may have many causes of which unemployment is but one; and since (3) is not false it must be true. Finally, in case (4) full employment and non-retarded growth rate coexist--a conjunction which clearly does not falsify the proposition, "If there is unemployment, then economic growth is retarded;" so that proposition must be true.

Recall now the question that triggered the foregoing explication: If an economist makes a prediction statement of the form "If A, then B," and then demonstrates empirically that "B" is true, has he thereby proved the truth of "A"? The answer, of course, is "no." He has, at best, merely failed to falsify "A". But, suppose he confirms the truth of "B" a second time, from a further empirical test that is independent of the first. Has he now established "A's" truth? On the preceding argument exactly, he has not; he has merely further failed to falsify "A", and to that extent has rendered "A" somehow more probable than before. Suppose he confirms "B" empirically a third time? A fourth? . . . A fiftieth? . . . A thousandth? . . . Is there any finite number of empirical confirmations of "B" that will once and for all establish the truth of "A"? In general, the answer is "no." As the independent empirical confirmations of "B" accumulate, "A's" truth becomes successively more probable just in the obverse sense that "A's" falseness grows increasingly improbable. As to the point beyond which one may "safely" take "A" as true, this is at bottom a value judgement depending on what risk of error one is prepared to tolerate.[54] The literature of mathematical statistics--particularly of statistical inference--offers guidance here[55] in making inferential judgements with ones eyes open, so to speak. But the important point to note is the obverse style of scientific inference, and the utter impossibility *a priori* of "proving" the truth of predictive models empirically.

Clearly, therefore, the question of predictive power in economic models, conventionally construed, is immune from proof. And so is the principle of causal determinism, which is an integral part of predictive power on the conventional interpretation of "predictive power." Nagel's opinion is germane here:[56]

> Whether the occurrence of every discriminable event is determined, whether for every event there is a unique set of conditions without whose presence the event would not take place, and whether if conditions of a specified kind are given an event of a certain type will invariably happen, are variant forms of a question that cannot be settled by *a priori* arguments. Nor do I think the question can be answered definitively and finally, even on the basis of factual evidence; for . . . the question is best construed as dealing with a rule of procedure for the conduct of cognitive inquiry, rather than with a thesis concerning the constitution of the world.

Since there is no secure logical foundation for construing literally, or indeed even in the sense of common usage, the predictive-power requirement of scientific models in economics, these interpretations may be abandoned. Yet, whatever the notion that is commonly designated "predictive power" really is, it is clearly a legitimate, integral requirement--the *sine qua non*, some would say--of any scientific model of economic phenomena. Therefore, it is necessary to give some interpretation or other to predictive power. The interpretation recommended here is based on the view that a central problem--*the* central problem, perhaps--of economics is the problem of *control*.

No sense of oppression is intended in the notion of control recommended here, though oppression indeed can be a technique of control. In Chapter II below a rigorous model of control will be presented. Meanwhile, control may be thought of as a *process* whereby a *system* is maintained in a designated state. A control process is, of course, independent of any particular system or state. The biologist presumably is interested in keeping various biological systems in states of survival; the ballistics engineer wishes to keep a ballistic system (a missile in flight, say) in the state of being on a given trajectory; the economist wishes to keep an economic system in a state defined by criteria like employment rate, growth rate, balance of payments, etc. All in common wish to maintain a particular set of parameters, belonging to some system or other, within a designated range. So long as these parameters remain each with its designated range, the system of which they are part is said to be <u>in control</u>. If a parameter goes beyond its designated range, its parent system is said to be <u>out of control</u>. A <u>control process</u> is, in part at least, the process whereby it is sought to maintain a system in control.

The system with which the economist is chiefly concerned is a set of assets (or resources) which are deployed by various means in order to achieve a set of objectives. Generally, the assets and the rules according to which they may be deployed are under a set of constraints (physical, cultural, etc.). The economist's task is (1) adequately to describe (e.g., by utilizing prose, mathematical, physical, etc. models) the system of assets-<u>cum</u>-deployment-rules-<u>cum</u>-deployment-techniques-<u>cum</u>-constraints-<u>cum</u>-objectives; (2) to prescribe an appropriate resource-deployment pattern; and (3) to monitor the modelled system, keeping alert for changes which make it necessary to change his prescribed resource-deployment pattern.

Now in describing the system of resources and so forth--in fashioning the model which will be the basis for his prescriptions--the economist must estimate the value of certain parameters contained in the modelled system. Depending on the structure of his models, these parameter values may lie anywhere within designated ranges (sometimes within a zero range, in which case the modelled system would be extraordinarily sensitive to the parameter concerned) without impelling a revision of the prescriptions which the economist deduces from his model. When an economist, using the various resource-allocation paradigms which are products of the *formal* part of his science, prescribes a particular pattern of resource deployment, then so long as his model's parameter values remain within their designated ranges, the economic process for which he has prescribed is *in control*. Otherwise, it is *out of control*, and the economist must refashion his prescriptions at least, and perhaps his model, too.

Thus a central problem--*the* central problem, perhaps--of economics is the problem of *control*. And the characteristic of economic models that goes by the name "predictive power" is simply the capacity for control. To say that a particular model possesses a high degree of predictive power is to say that it orders a great deal of uncertainty about the phenomenon it represents, and puts one in a good position to tell quickly when its client phenomenon has changed significantly. To the degree that an economic model has predictive power, its information content--an exceedingly important concept, which will be defined rigorously in Chapter III--is economically summarized (i.e., in the sense of "summarized with but little effort"), and significant changes in its information content readily detected.

4. *The Logic of Control*

In the preceding section reference was made to "significant" changes in modelled economic phenomena. It is well to elaborate the meaning of "significant" in this context. In an important sense, "significance" represents the point at which descriptive and prescriptive economics meet--the analogue, one might say, of the human pituitary gland in Descartes' explanation of how body and soul communicate with each other. And just as the logic of the soul's control over the body is embodied (for Descartes) in the human pituitary, so is the logic of economic control bound up with this notion of significance.

Once a particular economic process has been described by a model, whether subsequent changes in that process shall be regarded as significant depends ultimately on the model's author. It is entirely a question of value. It depends first of all on the author's--the controller's, the economist's: these designations will be used interchangeably in the present disccusion--intention toward the subject process. Secondly, it depends on the author's stock of a priori judgements and information. And finally, it depends on his willingness to risk being wrong.

As to the controller's intention, few publications in the contemporary literature can match Charles Hitch's Decision-Making for Defense[57] as a practical example of how rational resource allocation can be hobbled by unclear aims. Hitch, a distinguished economist, became Assistant Secretary of Defense (Comptroller) under Robert McNamara, and was a key figure in McNamara's controversial re-organization of the United States Defense Department. As Controller, Hitch's responsibilities naturally included assessing the significance of intra-Department changes upon the Department's resource allocation policies, the interaction between resource allocation patterns and military strategy, and so forth. He concluded that the whole problem of optimal resource allocation for this $50-billions-plus per annum enterprise would remain intractable so long as the system's desired outputs lacked unequivocal definition. Obviously there was a degree of arbitrariness about the output-taxonomy that Hitch thereupon introduced into Defense Department microeconomic analysis; and that is precisely the point of the present illustration. Hitch's output categories went something like this:[58]

(1) Deterrence or Fighting of All-Out War

(2) Deterrence or Fighting of Limited Wars

(3) Research and Development

(4) General Administration

(5) Miscellaneous

Subsumed under each general output program was a set of program elements which defined each output in greater detail. For example:

(1) <u>Deterrence or Fighting of All-Out War</u>

Nuclear Striking Force (Air Force, Navy)

B-47

B-52

Atlas

Polaris

. . .

Active Defense (Army, Navy, Air Force)

Early Warning

Interceptors

F 102

Bomarc

. . .

Local Defense

Nike

. . .

Passive Defense (Office of Civil Defense Mobilization

Dispersal

Shelters, Evacuation

Recuperation Planning

Conjoined with the various output categories were an array of quantitative measures (e.g., a 0.95 probability of 80 per cent destruction of a designated target area's economic capacity following "our" absorption of a nuclear first strike, given existing weapons' technology, intelligence data, etc.). Now corresponding to Hitch's output taxonomy, and *ceteris paribus*, a particular pattern of Department resource allocation becomes the theoretically optimal pattern. To alter his taxonomy would likely also be to alter the theoretical prescription for optimal resource allocation. Similarly, by altering Hitch's output taxonomy one alters also the very bases upon which the Defense Department's actual performance is measured, and hence an important subset of the criteria on which empirical developments are judged significant or non-significant. And clearly, the question, "What is the 'best' output taxonomy?" has no meaning except with reference to a particular set of prior, non-economic aims (e.g., political, social, military, etc.). This is what was meant by the earlier statement: Once a particular economic process has been described by a model, whether subsequent changes in that process shall be regarded as significant depends first of all on the controller's intention toward the subject process.

It depends, secondly, on the controller's stock of *a priori* judgements and information. Suppose a person is given an urn containing ten balls, some white and some red. He then draws at random a single ball from this urn and notes its colour. Clearly his a priori belief as to the true proportion of white versus red balls in the urn will determine his "degree of surprise" at whatever the result of his random selection. For example, let his random selection be a white ball. Suppose (from arbitrary motivation--the exact reason is not here germane) his a priori belief has been that his urn contained nine white balls and a single red one. On that hypothesis he began with a 9/10 chance of selecting a white ball; so the actual outcome will hardly surprise him. Now by contrast, suppose his a priori hypothesis had been that the true proportion of white balls in his urn was 1/10: then he should be quite surprised at his chance selection of a white ball.

Now the work of controlling an economic process bears this similarity to the work of drawing balls at random from an urn: the various outcomes of the subject economic process will surprise the economist (or controller) more or less in accordance with his a priori hypotheses about that process. And he will accordingly attach more or less significance to these outcomes, and apply remedies to the target system's inputs with greater urgency or less, depending on his stock of a priori information. This stock of information Kenneth Arrow calls the controller's "signal from Nature":[59]

> Each member of the organization is in possession of a signal from Nature, and his probability distribution of states of the world is the conditional distribution given that signal. (The "signal" is understood to be his knowledge based on learning and experience.)

As he learns, as he acquires experience, his signal, his stock of a priori information, his a priori probability distribution of states of the world, changes. Indeed, as will presently be seen (Chapter II), it is quite possible to define learning, experience, etc., in terms of ones signal.

But for now the discussion centres upon "significance" with respect to economic models, and particularly upon how the significance of economic information depends on the economist's stock of _a priori_ information.

Significance depends finally, as stated earlier, on the economist's willingness to risk being wrong. This criterion flows from error theory in mathematical statistics, and is conceptually quite simple and elegant. Given a particular economic model and its concomitant set of aims, objects, taxonomy, etc., the judgements which proceed from the controller's information stock will sometimes be right and sometimes be wrong, depending on the truth or falsity of his hypotheses. Assuming a basically stochastic universe (as will be elaborated in Chapter IV), chance fluctuations of economic phenomena, which get translated into random data variances, prevent the economist from ever knowing with certainty when an hypothesis that is part of his "signal" has been confirmed or falsified. Sometimes, for example, a correct hypothesis will appear contradicted by a datum which an ideal observer would recognize as a mere random fluctuation; and at other times this same phenomenon of random fluctuation will give bogus confirmation to an hypothesis that is not true. Professional statisticians call an error of the first kind simply an error of Type I, and an error of the second kind a Type II error. The two basic Types of error are ubiquitous in any decision process that occurs in the empirical world, and are logically so related that depressing the risk from one _ipso facto_ increases the risk from the other. For the controller concerned with spotting significant developments in his subject process, there is no question of escaping error; he can only choose what mixture of Type I and Type II errors he thinks fit; and though at some lower stage in his reckoning this is a cost-benefit matter, ultimately the economist must make a value judgement on this question. It may be useful here to emphasize this point with an illustration.

Suppose a particular economist is planning the economy of Canada, using a linear programming model. As is well known, the linear programming model, while permitting diminishing productivity of individual productive factors, assumes constant returns to scale. Therefore, by choosing a linear programming model, the economist claims the hypothesis that the Canadian economy exhibits constant returns to scale. Now that is a pretty important hypothesis, and he will naturally wish to test it empirically. Consider the value question that arises at this next step. When he tests his hypothesis, four distinct outcomes are possible:

(1) The Canadian economy is indeed characterized by constant returns to scale, and his empirical evidence confirms this.

(2) The Canadian economy is indeed characterized by constant returns to scale, but owing to random data fluctuations his empirical evidence is inconsistent with constant returns to scale.

(3) The Canadian economy is <u>not</u> characterized by constant returns to scale, and his empirical evidence shows that it is not.

(4) The Canadian economy is <u>not</u> characterized by constant returns to scale, but owing to random data fluctuations his empirical evidence is quite consistent with constant returns to scale.

If he lets his judgement about his returns-to-scale hypothesis be decided by the outcome of his empirical investigation, then in cases (1) and (3) the economist's judgement will be correct while in cases (2) and (4) it will be wrong. In case (2) he would be led to reject an hypothesis which is in fact correct--a Type I error. In case (4) he would be led to accept an hypothesis which is in fact false--a Type II error. Now for the value question: Since Type I and Type II errors are inescapably related, and assurance against one can be purchased just with greater risk from the other, how much of each Type should he buy?

If he were a very conservative person, disinclined to change things unless the need of change is overwhelmingly apparent, and fearful of the grave social and economic harm that might attend poor economic planning, the economist might choose a considerable risk of Type I error for the sake of more assurance against an error of Type II. On the other hand, if he were a radical reformer for whom a Canadian economy unplanned is much worse an evil than a Canadian economy planned even with a faulty model, then he might gladly tolerate a large risk of Type II error for the sake of holding down the risk from error of Type I. In either case (or in any intermediate case) the economist would design his statistical schema to reflect his outlook; and from that point onward the economic processes under his scrutiny--as represented by inflows of data--would be significant or otherwise according to his willingness to risk being wrong.

Near the start of the present section it was said that the question of "significance" was the bridge between prescriptive and descriptive economics, and the focus of control in the economist's control process. Now perhaps it is appropriate to recapitulate this claim. For given an economic model with a particular information content and a particular objective function--i.e., some set of things to be maximized or minimized--the economist may then turn to an appropriate-resource allocation paradigm and grind out a prescription for optimal behaviour. As the next step in the process of economic control, he monitors the empirical processes which gave him his model's parameters, assessing these data inflows according to some appropriate mixture of Type I and Type II risks, watching for significant behavioural changes. Generally, such changes will occur for two reasons: (1) due to the empirical impact of the resources-deployment activities that he has set going or (2) due to exogenous environmental changes. In either case these disturbances are significant in that they compel various parameter changes, thereby signalling the possibility of prescriptive changes for the process being controlled. In this way the (ongoing) act of describing his subject phenomena becomes an integral part of the economist's prescriptive procedure, with the exact place of joining determined by how he has chosen to define "significance."

5. The Problem of Control in the Social Sciences.

The social sciences deal with social systems. (In Chapter II, below, the notion of "systems" is discussed more precisely). Boulding writes:[60]

> The social sciences are seeking knowledge about society, that is, about social systems. A social system may be defined as any pattern of events that involves the interaction of two or more persons. The study of the internal constituion and behavior of persons, with which psychology is principally engaged, is, of course, an essential pre-requisite to the study of social systems, and there is clearly a continuum between, say, physiology at one end of the scale and sociology at the other, within which any boundary that we draw to define the social sciences will be somewhat arbitrary.

In pragmatic terms, the first aim of control in socio-economic phenomena is the social system's sheer survival. When the system's critical parameters lie securely within the ranges that ensure the continuing existence, the endurance, of the system over time, certain further aims are generated by the system—a function, perhaps, of its "learning" activity—which becomes imbedded in its control criteria. This learning process reflects what was once thought to be the overriding problem in the scientific analysis of socio-economic phenomena.

> Insofar as the social sciences change our images of social systems, they will also change our behavior and, hence, the social systems themselves. The social system does not, as it were, "stay put" while we investigate it. It changes, sometimes profoundly, under the impact of our investigation. The social scientist, therefore, unlike the physical scientist, cannot regard himself as a detached outside observer of nature but must regard himself as part of the system that he is studying.[61]

The present work cannot deal at length with this subtle, complex problem. But, before passing to the second, equally important problem of socio-economic control, the general outline for a solution will be described.[62] It is based on Gödel's notion of Incompleteness.

Consider the statement S: "This statement is false." Is S true or false? A moment's reflection reveals that if S is true, then it is false; and if S is false, then it is true. Therefore, S is true if and only if it is false. But it is logically meaningless to say that a proposition is true if and only if it is false, and so we must designate the class C of propositions which S represents as formally undecidable. Now C belongs to a language <u>system</u> which, like every logical, consistent system (e.g. arithmetic, geometry, etc.), possesses <u>ipso facto</u> certain regions of undecidability comprising the characteristic of (Gödelian) Incompleteness. (Gödel has shown this to be a necessary consequence of consistency.) In the example just cited, S has no referent except itself, so that the Incompleteness characteristic appears of little consequence. But consider a further example. Suppose there is a barber in Winnipeg whose job is to shave every Winnipeg man who does not shave himself. Now, does this Winnipeg barber shave himself? Again, he does if and only if he does not. At first glance it might seem that this Winnipeg barber is truly a remarkable person, or that Winnipeg is indeed a remarkable city, or that the act of shaving possesses intrinsically a strange, paradoxical property. But the phenomenon of the Winnipeg barber is due solely to the "texture" of the language that describes it, and is merely a pseudo-problem in any other sense. To deal with Incompleteness in a particular language is impossible except with the use of a metalanguage, i.e., a language for talking about the first-order language. But the metalanguage, too, *ipso facto* possesses the Incompleteness property, and requires a *meta-metalanguage* for the same reason that the first-order language required a metalanguage. And this same requirement holds through an infinite regress of meta-meta-meta- . . . languages.

Now these principles have their counterpart in social systems. Any particular social system comprehends an information network that is, as it were, the language of the system. It therefore, possesses the property of Incompleteness, which manifests itself as an impotency with respect to information which enters the system from exogenous sources. To deal with this exogenous information, it is necessary to invoke a metalanguage, in the form of a higher-order system of which the first-order system is a (mere) component. And this procedure may be continued through an infinite regress of "higher-order" systems. In this context, the problem of the observer who is, and whose observations are, a part of an observed economic system, is seen to be not a substantial problem of economics, but a "syntactical" problem instead, imbedded in the logical bricks and mortar that the economist perforce uses to build his economic models.[63]

A further, ubiquitous control problem which plagues the social sciences, and which is not unrelated to the foregoing problem, might aptly be dubbed the Queen's Croquet Game phenomenon. Wiener, being a mathematician, understandably defines this problem by focussing on mathematical economics; but his remarks surely apply well beyond this narrow field, and comprise an excellent summary of the classic difficulty of model construction for socio-economic phenomena.[64]

> I have found mathematical sociology and mathematical economics or econometrics suffering under a misapprehension of what is the proper use of mathematics in the social sciences and of what is to be expected from mathematical techniques, ...
> . . . .
>
> The success of mathematical physics led the social scientist to be jealous of its power without quite understanding the intellectual attitudes that had contributed to this power. The use of mathematical formulae had accompanied the development of the natural sciences and became the mode in the social sciences. Just as primitive peoples adopt the Western modes of denationalized clothing and of parliamentarism out of a vague feeling that these magic rites and vestments will at once put them abreast of modern culture and technique, so the economists have developed the habit of dressing up their rather imprecise ideas in the language of the infinitesimal calculus.
>
> In doing this, they show scarcely more discrimination than some of the emerging African nations in the assertion of their rights. The mathematics that the social scientists employ and the mathematical physics that they use as their model are the mathematics and the mathematical physics of 1850. An econometrician will develop an elaborate and ingenious theory of demand and supply, inventories and unemployment, and the like, with a relative or total indifference to the methods by which these elusive quantities are observed or measured. Their quantitative theories are treated with the unquestioning respect with which the physicist of a less sophisticated age treated the concepts of the Newtonian physics. Very few econometricians are aware that if they are to imitate the procedure of modern physics and not its mere appearances, a mathematical economics must begin with a critical account of these quantitative notions and the means adopted for collecting and measuring them.

> Difficult as it is to collect good physical data, it is far more difficult to collect long runs of economic or social data so that the whole of the run shall have a uniform significance. The data of the production of steel, for instance, change their significance not only with every invention that changes the technique of the steelmaker but with every social and economic change affecting business and industry at large, and, in particular, with every technique changing the demand for steel or the supply and nature of the competing materials. For example, even the first skyscraper made of aluminum instead of steel will turn out to affect the whole future demand for structural steel, as the first diesel ship did the unquestioned dominance of the steamship.
>
> Thus the economic game is a game where the rules are subject to important revisions, say, every ten years, and bears an uncomfortable resemblance to the Queen's croquet game in Alice and Wonderland. . . . Under the circumstances, it is hopeless to give too precise a measurement to the quantities occurring in it. To assign what purports to be precise values to such essentially vague quantities is neither useful nor honest, and any pretense of applying precise formulas to these loosely defined quantities is a sham and a waste of time.

What Wiener describes is the social scientist's--the economist's--problem of determining (1) what empirically-derived, or at least empirically-testable assumptions, postulates, parameters, etc., he can prudently rest his models on, and (2) when the time has come to amend such existing bases. Wiener says that (a) socio-economic phenomena inherently lack the stability (i.e., the stationarity) of physical phenomena, and (b) extant statistical techniques are impotent to deal with the very short runs of data which alone, and particularly due to (a), are available to the social scientist.

It is important to note that (1) and (2) are at bottom control problems, as discussed earlier in the present paper. Thus Wiener's authority supports the control-oriented view of socio-economic phenomena advocated here. It is further important to note that (1) and (2) derive from (a) and (b), so that any effective solution of the latter can in principle be translated into the solution of the former.

Now statement (a) is by no means a proven Law of Nature, as the sheer success of the commercial insurance industry should suggest. But given (a), the essence of the social scientist's problem would appear to lie in (b). This makes it particularly noteworthy that the thrust of contemporary developments in statistical theory has been almost precisely by way of remedy for Wiener's complaint. Such work as Wald's,[65] and the resurrection of Bayes' Theorem[65] comprise impressive evidence of this. There is a movement away from the traditional emphasis on static point and interval estimation of parameters, towards emphasis instead on the dynamic process of ongoing, sequential testing (and continuous revision) of hypotheses-about-parameters. This is the answer to the inherent instability of socio-economic phenomena. And, when brought to bear upon a control-oriented interpretation of economic models, it may have a profound impact on economic theory.

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[1]Kenneth Boulding, A Reconstruction of Economics (New York: John Wiley & Sons, Inc.; Science Editions, Inc., 1965), p. 303.

[2]Max Ways, "Tomorrow's Management," Fortune, 1 July, 1966, p. 65.

[3]Norbert Wiener, Cybernetics (New York: The Technology Press of the Massachusetts Institute of Technology, and John Wiley & Sons, Inc., 1948), p. 19.

[4]"Cybernetics . . . comes out of electrical engineering, neuro-physiology, physics, biology, with even a dash of economics." Kenneth E. Boulding, "General Systems Theory--The Skeleton of Science," Management Science, Vol. 2, No. 3, April, 1953.

[5]Wiener, op. cit., p. 19.

[6]Stafford Beer, Cybernetics and Management (New York: John Wiley & Sons, Inc.; Science Editions, 1964), p. 91. Italics (i.e., underlined words, here) are in the original text.

[7]Lee B. Lusted, "Cybernetics and Medicine," review of Masturzo, Aldo, Cybernetic Medicine (Springfield, Ill: Thomas, 1965) in Science, Vol. 151, 4 February, 1966, p. 559.

[8]Beer, op. cit., p. xii.

[9]Norbert Wiener, The Human Use of Human Beings: Cybernetics and Society (2nd ed., rev.; New York: Doubleday & Company, Inc., 1954) p. 16.

[10]See, for example, John Hospers, An Introduction to Philosophical Analysis (Englewood Cliffs: Prentice-Hall, Inc., 1960) pp. 57-62.

[11]Colin Cherry, On Human Communication (New York: M.I.T. Press and John Wiley & Sons, Inc.; Science Editions, Inc., 1961), p. 56. Last italics supplied.

[12]Karl W. Deutsch, The Nerves of Government: Models of Political Communication and Control (New York: The Free Press of Glencoe, 1963). The immediately following passages appear at p. ix.

[13]Ibid., p. 76.

[14]Kenneth J. Arrow, "Control in Large Organization," Management Science, Vol. 10, No. 3, April, 1964, p. 398.

[15]The Oxford Universal Dictionary (3rd ed.; revised; Oxford: The Clarendon Press, 1955).

[16]Alfred Marshall, Principles of Economics (8th ed.; London: Macmillan & Co. Ltd., 1961), p. 1

[17]Paul A. Samuelson, Economics: An Introductory Analysis (6th ed.; New York: McGraw-Hill Book Company, 1964), p. 5.

[18]Immanuel Kant, Critique of Pure Reason (transl. Norman Kemp Smith; London: Macmillan & Co. Ltd., 1963).

[19]Alfred North Whitehead and Bertrand Russell, Principia Mathematica (Cambridge: Cambridge University Press, 1962).

[20]Kenneth E. Boulding, "Social Sciences," The Great Ideas Today (Chicago: Encyclopaedia Britannica, Inc., 1965), p. 257.

[21]Russell L. Ackoff, "Systems, Organizations, and Interdisciplinary Research," General Systems, Vol. V, 1960, p. 6, quoted by A. Kuhn, op. cit., p. vii.

[22]Norbert Wiener, Cybernetics (2nd ed.; New York: The M.I.T. Press and John Wiley & Sons, Inc., 1961), p. 2.

[23]See, for example, Abraham Wald, Sequential Analysis (New York: John Wiley & Sons, inc., 1947). Strictly speaking Wald's work does not belong to the social sciences. But, as will presently be shown, it has begun to have a considerable, radical influence on the social sciences; and this influence

will grow, very greatly. Also, see Boulding's comment about the arbitrariness of "any boundary that we draw to define the social sciences," at p. 34 of the present text.

[24]Kenneth E. Boulding, "Social Sciences," The Great Ideas Today (Chicago: Encyclopaedia Britannica, Inc., 1965), pp. 274-5.

[25]R. Duncan Luce and Howard Raiffa, Games and Decisions (New York: John Wiley & Sons, Inc., 1957).

[26]Peter C. Fishburn, Decision and Value Theory (New York: John Wiley & Sons, Inc., 1964).

[27]John von Neumann and Oskar Morgenstern, Theory of Games and Economic Behavior (New York: John Wiley & Sons, Inc., Science Editions, 1964).

[28]Kenneth E. Boulding, Conflict and Defense (New York: Harper & Row, 1963).

[29]James M. Buchanan and Gordon Tullock, The Calculus of Consent (Ann Arbor: The University of Michigan Press, 1962).

[30]Peter M. Blau, Exchange and Power in Social Life (New York: John Wiley & Sons, Inc., 1964).

[31]Thomas C. Schelling, The Strategy of Conflict (Cambridge: Harvard University Press, 1963).

[32]These concepts may broadly be subsumed under the notion of "control," as the present thesis will try to show.

On the matter of collapsing diverse parts of the non-economics social sciences into economics, a remarkable illustration is Braybrooke and Lindblom's "Strategy of Disjointed Incrementalism"—an important recent contribution to the political science literature which, in essence, is but the economist's marginal analysis applied to political decision-making. Here is how Braybrooke and Lindblom describe Disjointed Incrementalism:

> . . . we note first that only those policies are considered whose known or expected consequent social states differ from each other incrementally. But one can imagine that a set of policies meeting this condition might be expected to bring about some social state differing drastically and nonincrementally from the status quo. Hence we add a second feature: that only those policies are considered whose known or expected consequences differ incrementally from the status quo. The third feature of incremental choice, however closely it seems to follow from the first two, is logically independent of them; that examination of policies proceeds through comparative analysis of no more than the marginal or incremental difference in the consequent social states rather than

through an attempt at more comprehensive analysis of the social states. To this list we add a final feature, again logically independent but implicit in our exposition of incremental choice: choice among policies is made by ranking in order of preference the increments by which social states differ.

See David Braybrooke and Charles E. Lindblom, A Strategy of Decision (Glencoe: The Free Press of Glencoe, 1963), pp. 85-6.

Conversely, as an illustration of economics reaching into political science:

Economics, as the writer sees it, is ultimately a policy science. In order to formulate workable public policies . . . economics must first develop appropriate models. These, though necessarily simplified, must be as accurate as prevailing methodology and knowledge permit. Among other things, they must take into account political and social reality, as well as economic efficiency. The following comments point to a necessary change in the traditional emphasis for economics . . .

See Hans H. Jenny, "Operations Research: Its Nature and Scope, with Some Comments Concerning Its Impact on the Smoothing of Cyclical Fluctuations," in Kenneth E. Boulding and W. Allen Spivey, Linear Programming and the Theory of the Firm (New York: The Macmillan Company, 1961), p. 158.

That the traditional economics somehow failed to attract the other social-science disciplines as an apt conceptual apparatus-for-integration is suggested by the remarks of Krupp:

It seems probable that social scientists in other disciplines would find that the methods of economic theory can be fruitfully applied to the special substantive problems of their own sciences. The interchange between sociology, anthropology, and psychology has provided a major source of stimulation in the social sciences; economics, in contrast, has had practically no influence on the other social sciences. This is especially lamentable because the development of analytic and systematic methods has probably been more advanced in economics than in any of the other social sciences.

See Sherman Roy Krupp, "Equilibrium Theory in Economics and in Functional Analysis as Types of Explanation," in Don Martindale, (ed.) Functionalism in the Social Sciences (Philadelphia: The American Academy of Political and Social Sciences, 1965) pp. 82-3.

[33]It is cumbersome to refer repeatedly to traditional economics, economics on the traditional view, the "old" economics, etc., on the one hand; and cybernetic economics, economic cybernetics, economics in the age of cybernetics, the "new" economics, etc., on the other. Far easier would it be simply to contrast economics with cybernomics, were it not for a

certain presumptiousness attached to the coining of a new word in present circumstances. Still, it is worth considering whether such a new word is not indeed merited.

[34]"Thünen's contributions may be summed up as follows. (1) He was the first to use the calculus as a form of economic reasoning. . . ." Joseph A. Schumpeter, History of Economic Analysis (New York: Oxford University Press, 1963), p. 466.

[35]Cf., for example, John G. Kemeny, et al, Finite Mathematical Structures (Englewood Cliffs: Prentice-Hall, Inc., 1960). Setting aside such finite mathematical structures, and relying simply on classical calculus, would virtually eliminate such modern developments in economic theory as Game Theory, the various programming paradigms (Linear Programming, Dynamic Programming, etc.), and Markovian decision processes--to name but a few.

[36]Norbert Wiener, The Human Use of Human Beings: Cybernetics and Society (2nd ed., rev.; New York: Doubleday & Company, Inc., 1954), p. 8.

> [37]The science of economics developed in the nineteenth century when the public had become familiar with mechanical principles. Both classical and Marxist economics leaned heavily on mechanical analogies. The economist's model still in service today is a machine. Since humanity is that element in economics least susceptible to mechanical treatment and prediction, economics tends to suppress the human factors. "Economic man" is the most oversimplified of all views of our otherwise interesting species.

Max Ways, "Tomorrow's Management: A More Adventurous Life in a Free-Form Corporation," Fortune, 1 July, 1966, p. 85. Evidently it will take a long time for the newer ideas to replace the old.

[38]Colin Cherry, On Human Communication (New York: M.I.T. Press and John Wiley & Sons, Inc.; Science Editions, Inc., 1961), pp. 212-13; the second passage comes from ibid., p. 23.

[39]In the present paper--drawing as it does perhaps more extensively than is usual from diverse, non-economics fields--the risk of communications impairment (between author and reader) from terminological accident is especially great. This is a natural risk, of course. It is notorious how jargon in one discipline can mean quite the opposite of its homonymous counterpart in a second discipline. For example, to the economist, "rationalization" is generally a good thing, while to the psychologist, "rationalization" may be an unhealthy activity indeed. Again, the sociologist's "discrimination" is generally reprehensible, the psychologist's commendable. A like caveat applies to such terms as "system," "mechanism," "functionalism," and so forth. There is no easy solution to the problem. But in the circumstances, the present author feels bound to take note of it, at least.

[40]Krupp, op. cit., p. 70; the second passage is drawn from p. 65.

[41]Ernest Nagel, The Structure of Science: Problems in the Logic of Scientific Explanation (New York: Harcourt, Brace & World, Inc., 1961), p. 532.

[42]Cherry, op. cit., p. 24.

[43]Ernest Nagel,op. cit., p. 532.

[44]Ibid.

[45]George B. Dantzig, Linear Programming & Extensions (Princeton: Princeton University Press, 1963), p. 32.

[46]G.P.E. Clarkson, "A Model of Trust Investment Behavior," in Richard M Cyert and James G. March, A Behavioral Theory of the Firm (Englewood Cliffs: Prentice-Hall, Inc., 1963), pp. 253-267.

[47]Ibid., p. 254.

[48]Ibid., pp. 265-6

[49]This is a standard result of elementary logic. See, e.g., Willard Van Orman Quine, Methods of Logic (rev. ed.; New York: Henry Holt and Company, Inc., 1959), pp. 12-17.

[50]See Kenneth J. Arrow, Social Choice and Individual Values (2nd ed.; New York: John Wiley & Sons, Inc., 1951).

[51]This way of testing the truth of economic models leads to rather deep philosophical questions which professional philosophers themselves still find controversial. So, too, do the professional economists whose empirical work has caused them to ponder methodological issues, e.g, Geoffrey P. Clarkson, The Theory of Consumer Demand: A Critical Appraisal (Englewood Cliffs: Prentice-Hall, Inc., 1963), esp. Ch. 2, "Some Notes on Scientific Deductive Systems," andCh. 6, "The Economist's Dilemma." Cf. Richard S. Rudner, Philosophy of Social Science (Englewood Cliffs: Prentice-Hall, Inc., 1966); and the anthologies, Sidney Hook (ed.), Determinism and Freedom in the Age of Modern Science (New York: Collier Books, 1961); and Herbert Feigl and Grover Maxwell (eds.) Current Issues in the Philosophy of Science (Proceedings of Section L of the American Association for the Advancement of Science, 1959; New York: Holt, Rinehart and Winston, 1961).

[52]This result, which can easily appear mistaken at first glance, follows rigorously from DeMorgan's Theorem. See Morris R. Cohen and Ernest Nagel, An Introduction to Logic and Scientific Method (New York: Harcourt Brace & World, Inc., 1934), pp. 69-70, and p. 125. Ultimately this result is a consequence of relying, as Western culture generally and the scientific method particularly do, on a two-value logic, under which every proposition must be either true or false. Of course, there is no immutable need to so do

and this system does indeed lead to such paradoxes as statements which are true if and only if they are false (e.g., Russell's Paradox of the class of all classes which are not members of themselves, which oddly is a member of itself if and only if it is not a member of itself); see Ernest Nagel and James R. Newman, Gödel's Proof (New York: New York University Press, 1960). One alternative might be a three-value logic, on which every proposition must be either true, false, or neither. It is interesting to speculate on the results for economic theory of abandoning the traditional two-value logic. For one thing, it is possible that certain normative questions which presently lie outside of economic theory but nevertheless touch economics importantly (e.g., How ought income to be distributed?) may ultimately be anomalies of the Russell's Paradox family, which would disappear with the abandonment of two-value logic. Cf. Sec. 5 of the present Chapter.

[53]Strictly speaking, the statement, "There is unemployment" is not an economic model, though this simplification is not germane to the point which the present paragraph (of text) is trying to make. Some readers may nevertheless prefer to think of "A" as standing for a particular economic model possessing unemployment as a prominent characteristic.

[54]There is a growing literature which would treat even this question from a cost-benefit calculus, instead of from the conventional value-judgement viewpoint. The present thesis has much in common with this newer approach, though a detailed reconciliation would go too far afield now. But cf. Howard Raiffa and Robert Schlaifer, Applied Statistical Decision Theory (Boston: Division of Research, Graduate School of Business Administration, Harvard University, 1961).

[55]The contemporary explosion in the literature of statistical inference amounts surely to a renaissance in this important branch of science. It has a strong impact on economic theory. This impact occurs in modern Decision Theory, which the present author believes is logically identical to the constrained-resource-allocation problem in its general form, ie., the central problem of all economics. No proof will be attempted here, but the connection between the two will be suggested by an illustration that appears later in the present paper.

A seminal influence in Decision Theory was Abraham Wald, Sequential Analysis (New York: John Wiley & Sons, Inc., 1947). Wald's work was done during World War II; its dissemination was unfortunately inhibited by security restrictions; and his brilliant conversation with his colleagues in the open literature ended abruptly with his death in 1950, aged 48, in an airplane crash. For an encyclopaedic treatment of classical statistical inference side by side with a discussion of sequential analysis, see M. G. Kendall and A. Stuart, The Advanced Theory of Statistics (London: Charles Griffin & Company Limited, 1961), Vol. 2, "Inference and Relationship." For economists, a good introduction to Decision Theory is Herman Chernoff and Lincoln E. Moses, Elementary Decision Theory (New York: John Wiley & Sons, Inc., 1959). Considerably more advanced, but extremely lucid, lively, and more explicitly relevant to the economist's professional work is the now classic Duncan R. Luce and Howard Raiffa, Games and Decisions (New York: John Wiley & Sons, 1957). A more recent wrinkle in the Decision Theory cloth comes from Savage's embroidery upon the 1763 Theorem of the Reverend Thomas Bayes. In the hands of Savage and his followers Bayesian Decision Theory is rapidly becoming a powerful influence. See L. J. Savage et al, The Foundations of Statistical Inference (London: Methuen & Co. Ltd., 1962).

[56] Ernest Nagel, "Some Notes on Determinism," in Sidney Hook (ed.) Determinism and Freedom (New York: New York University Press, 1958), p. 183.

[57] Charles J. Hitch, Decision-Making for Defense (Berkeley and Los Angeles: University of California Press, 1965).

[58] Ibid., p. 29; and Charles J. Hitch and Roland N. McKean, The Economics of Defense in the Nuclear Age (Cambridge: Harvard University Press, 1963), p. 56.

[59] Kenneth J. Arrow, "Control in Large Organizations," Management Science, Vol. 10, No. 3, April, 1964, p. 404.

[60] Kenneth E. Boulding, "Social Sciences," in The Great Ideas Today (Chicago: Encyclopaedia Britannica, Inc., 1965), p. 256.

[61] Boulding, loc. cit.

[62] To be accurate, no single "solution" to this problem exists, but an infinite regress of classes of solutions. An adequate discussion of this matter would require a major work (a doctoral dissertation, say) all by itself. It is simply impossible to claim precision in a sketch as brief as that appearing in the present text.

[63] A very interesting, practical illustration of this hypothesis, drawn from his experience of industrial operations research, and explicitly related by its author to Gödelian Incompleteness, appears in Beer, op. cit., pp. 76-81.

[64] Norbert Wiener, God & Golem, Inc., (Cambridge: The M.I.T. Press, 1966), pp. 88-91.

[65] Please fn. 55, above.

CHAPTER II

A MODEL OF CONTROL

We may find ourselves, indeed, with two bodies of rather integrated social theory--one revolving around the concept of the transaction and decision theory, the other revolving around the nature of media and the communications process. The task of bringing these together remains to be done.

--Kenneth Boulding[1]

Now the elements of the art of war are: first, measurement of space; second, estimation of quantities; third, calculations; fourth, comparisons; fifth, probability of victory. Quantities derive from measurements, numbers from quantities, and victories from comparisons.

--Sun Tzu, c. 400 B.C.[2]

Mathematicians are like Frenchmen: whatever you say to them they translate into their own language, and forthwith it is something entirely different.

--Goethe[3]

1. The Economy as a System

A particular set of variables comprise a particular economic system. The variables define the system. (This is analogous to defining the universe of discourse, in logic.) As a rule in problems of control, "economic systems" will mean here "systems-for-doing-something-or-other," and not just systems *in vacuo*. Thus speaking generally, an economic system is a system-for-optimum-resource-allocation. Particular systems can have narrower designations, as e.g., the system-for-providing-community-health-services-at-least-cost.

Clearly, the choice of which variables shall comprise a particular economic system is rather an arbitrary matter. As discussed earlier, it depends on (1) what one wants the system to do, (2) one's a priori information stock, and (3) how tight a degree of control one desires (ie., what risk of being wrong one is prepared to tolerate.

Given any economic system, its ingredient variables at any particular time t, will be in some particular pattern, which is determined by a particular conjunction of values; and these define the system's state at time t. For example, consider the economic system defined by the single variable, "Brown's employment." On each successive day (t = 1, 2, . . .) this variable can take one of two values, viz. "employed" (= 1.), or "unemployed" (= 0.) Thus on the first day, when Brown has no work, the system is in state zero. On the second day, when Brown has found work, the system is in state one. A protocol of the system's states over time might look like this:

t	1	2	3	4	5	6	. . .
State	0	1	1	1	0	0	. . .

Consider a further illustration of the state-defined behaviour of an economic system. Let this be the monetary system of a national economy. In this economy it is possible to distinguish m separate lines of economic activity by their m different, characteristic outputs. To each such output (or line) assign a distinct number, so that the number "one" represents (say) "steel," the number "two" represents "wood pulp," the number "3" represents "labour," . . . , and the number "m" represents "automobiles." These numbers represent the states of the system. Now at a specified time t, each unit of money in this system will reside in a particular state. For example, at midnight each Sunday, each dollar in the system will be in the possession of some firm or individual belonging to one and only one system-state. Each representative dollar thus traces out a path about the system. The path of one particular such dollar might yield a protocol like this:

Week	1	2	3	. . .	t
State	5	12	2	. . .	i

Likewise, the paths of the two distinct representative dollars might yield a protocol like this:

	Week	1	2	3	. . .	t
State	\$ no. 1	5	12	2	. . .	i
	\$ no. 2	7	18	97	. . .	j

Indeed, any number of distinct, representative dollars could be considered in this way:

	Week	1	2	3	. . .	t
State	\$ no.					
	1	5	12	2	. . .	i
	2	7	18	97	. . .	j
	3	5	11	1	. . .	k
	.	.	.	.	. . .	.
	.	.	.	.	. . .	.
	.	.	.	.	. . .	.
	N	118	1	17	. . .	r

One might expect that the pattern produced by these interindustry money flows somehow reflects the pattern of interindustry economic activity imposed upon the subject economy by its technology, institutions, and so on. If this were indeed the case, then one might be glad of some succinct way to describe this pattern--some way perhaps to quantify it economically. Such an index could tell a great deal. It would be like having the economy's fingerprint, except that this fingerprint would change over time, thereby yielding even more information than human fingerprints do. In Chapter IV, a way of obtaining such an economic fingerprint is described which is quite simple both conceptually and computationally. For now, the state-oriented model is presented as an illustration of a certain conceptual richness that is pregnant in the cybernetic method; and as an example of an economic system.

2. Isomorphism and Homomorphism in Models.

A model of a system is a representation of the system. This representation may take many forms, e.g., verbal or written prose, mathematical symbols, physical hardware, and so forth. The main idea is that each element of the system is somehow related to each element of the model. Generally such a relationship can be formalized by a set of rules for transforming the system's elements into the model's, i.e., for identifying any particular element of the system with a particular element of the model.

If each element of the system is uniquely associated with one and only one element of the model, the transformation that defines this association is called a one-one transformation. If more than one element of the system may be associated with a particular element of the model, the defining transformation is called a many-one transformation. And finally, if a particular system element may be associated with more than one model element, the defining transformation is called a one-many transformation.

In the example of sec. 1., above, where the elements of each system were sets of states, particular states were identified with particular integers. The written symbols for these integers, characteristically arrayed on paper, comprised the models used. Thus in the first illustration the system-state of "being employed" was transformed (i.e., related by the rules governing how the model represented its parent system) into the integer, "one"; and the system-state of "being unemployed" was likewise transformed into the integer, "0".

For convenience, let the symbol $\rightarrow$ stand for the occurrence of a transformation, so that the statements

"being employed" $\rightarrow$ "1." and

"being unemployed" $\rightarrow$ "0."

express the system-model connections just described. As a further notational convenience, these same two statements may be represented as follows:

$\rightarrow$	
being employed	1.
being unemployed	0.

Now strictly speaking, the operation of transforming a system into a model, which allegedly occurred above, is not really what happened. In fact, one set of symbols was merely transformed into another set of symbols. This is rather an important point. Any empirical (or even conceptual) economic system that one wishes to transform into a model can after all be viewed as a collection of symbols, instead of as "a system"; and conversely, any model of an economic system can itself be viewed as a kind of system, instead of as a collection of symbols. Either viewpoint is arbitrary. So, instead of talking inflexibly about economic systems and their models, one may speak more generally about <u>operands</u> and <u>transforms</u>--retaining, of course, one's system-model language whenever its usage is clear. An operand[4] is simply a thing that a particular transformation relates to a transform: the operand is what gets transformed; the transform is what the operand gets transformed into. In the earlier example, "being employed" and "being unemployed" are both operands, and "1." and "0." are their respective transforms.

Whenever a transformation exists which can uniquely relate the elements of one system with the elements of a second system in a one-one transformation, the two systems are said to be <u>isomorphic</u>. The relationship of isomorphism is quite general: the two systems which are isomorphic with each other may be a process-model pair, a process-process pair, a model-model pair, or a pair of any other kind. The key consideration is whether one can relate whatever pair one is studying by a one-one transformation under which they are formally identical.

This notion may be generalized (and perhaps sharpened) by symbolic representation. Suppose there are two separate transformations, ∂ and β each governing its own operand-transform sets as follows:

$\partial \rightarrow$	
g	k
h	j
j	h
k	g

$\beta \rightarrow$	
a	b
b	a
c	d
d	c

The transformation ∂ may be represented graphically as follows:

$g \rightarrow k$

$h \rightarrow j$

Similarly, the transformation β may be represented graphically:

$$a \rightrightarrows b$$

$$c \rightrightarrows d$$

By visual inspection, the formal identity between the systems ∂ and β is apparent; this identity can be expressed by any of the one-one transformations Q, R, S, or T, as follows:

Q: $\rightarrow$	
β	∂
a	h
b	j
c	g
d	k

R: $\rightarrow$	
β	∂
a	g
b	k
c	h
d	j

S: $\rightarrow$	
∂	β
h	a
j	b
g	c
k	d

T: $\rightarrow$	
∂	β
g	a
k	b
h	c
j	d

Hence the systems (or transformations) ∂ and β are isomorphic.

In principle, there is no end to the complexity of phenomena that one can represent by isomorphic models. The economist, for example, who created an isomorphic macro-model of the Canadian economy could, if he wished, refine his model into isomorphism with (say) every separate firm and family in Canada; and after he had done this, it would remain open to him further to refine his model into isomorphism with every separate person in Canada, . . . and so on endlessly through every neuron, every molecule, every atom, every electron, etc., etc. The principle on which he might proceed can be illustrated by lifting one notch upward the level of complexity of the systems ∂ and β.

Let β be a subsystem of a larger transformation M, which contains as well the subsystem ϵ such that

M: →	ϵ	β
a	a	b
b	c	a
c	d	d
d	c	c

Similarly, let ∂ be a subsystem of a larger transformation N, which contains as well the subsystem ϵ such that

N: →	ϵ	∂
g	k	k
h	h	j
j	g	h
k	g	g

Then M and N are isomorphic, by the one-one transformation P, viz.

P: →	
ϵ	α
∂	β
g	c
h	a
j	b
k	d

For example, M might be the Canadian economy, M(α) the Canadian economy under boom conditions, and M(β) the Canadian economy under conditions of slump. Then N would be an isomorphic model of M.

But, while in principle there is no end to the complexity of phenomena that one could represent with isomorphic models, in practice by sticking just to isomorphic models one proscribes from investigation the more interesting of the real-world processes that are the social scientist's responsibility. This is due to the sheer complexity of such processes, and the computational burden they accordingly impose. A theoretical apparatus bound by the rules of strict isomorphism leads to sterile theory; it can never come to grips with real problems--the engine and fuel of theoretical power. Hence the need for homomorphism.

If two systems M and N are related to each other in such a way that a many-one transformation T performed on M, and yielding (say) M', makes M' isomorphic with N, then N is called an homomorphism of M.

This definition may be illustrated symbolically.

Suppose M is a system described by the following transformation:

M: →	i	j	k	l
a	b	a	a	b
b	a	b	b	c
c	b	c	b	a
d	c	b	e	e
e	a	c	d	e

while N is a system described by the following transformation:

N: →	α	β
g	g	h
h	h	h

Clearly M and N are not isomorphic, M being a more complex process than N. The question is: Can they be made isomorphic? Is there a transformation T which, when applied to M, will yield some M' that is isomorphic with N?

As it happens, the answer is "yes," the required transformation being

T: →	
a	h
b	h
c	h
d	g
e	g
i	β
j	β
k	α
l	α

Applying T to M gives M', as follows:

M': →	β	β	α	α
h	h	h	h	h
h	h	h	h	h
h	h	h	h	h
g	h	h	g	g
g	h	h	g	g

Removing redundant rows yields:

M': →	β	β	α	α
h	h	h	h	h
g	h	h	g	g

Removing redundant columns yields:

M': →	β	α
h	h	h
g	h	g

Transposing rows then gives:

M': →	β	α
g	h	g
h	h	h

Finally, transposing columns produces:

M': →	α	β
g	g	h
h	h	h

which is isomorphic with N.

Thus N is a homomorphism of M.

Homomorphism is ubiquitous in scientific models, and indeed in all organized thought. When Kant said that percepts without concepts are blind,[5] he was referring to the need of imposing system upon--of ordering--the multitude of data that impinge on human sensory apparatus from the physical world. Without some such ordering we humans would be overwhelmed by the sheer complexity of our environment. With such an ordering we perceive not the detailed, fantastic diversity of these empirical processes' data-outputs, but instead various homomorphic representations of them. These homomorphisms are our concepts, our language, our models, and so forth. And our grasp of reality--our ability to understand, to manipulate, and to control the environmental processes which in sum comprise reality--turns largely on the aptness of our homomorphisms.

In an important sense the business of science is to devise useful homomorphisms. This is what is meant by the generality of scientific "laws," or scientific "power." The scientific approach to an exceedingly complex system like a national economy does not entail making every possible distinction--that would be slavish isomorphism--but entails instead skillful homomorphism. The professional mathematician could not claim much power for his discipline if he had to say, "2 x 2 = 4; 2 x 4 = 8; 4 x 2 = 8; 2 x 6 = 12; . . ." every time he wanted to express the idea that, "Even x Even = Even." In the first case he would be attempting to construct an isomorphism with the infinite set of even numbers; while his second statement is homomorphic with the same set.

An exceedingly large, complex system like a national economy must be dealt with homomorphically if one is to understand and control it. The trouble with this dictum is that with respect to any such economic process, many different homomorphisms are possible. Disagreement among economists about how the economy "really" works is perhaps always due to disagreement about what particular homomorphism is "best." Ultimately this is a question of values and not just economic values; but it is possible to delineate this question with somewhat greater precision that one customarily finds in discussion of this sort. To do this, attention is next focussed on the topic of model hierarchies.

3. Homomorphic Hierarchies and Tradeoff Compartments

Consider a system possessing four distinct states, a, b, c, and d. Suppose the system operates so that state a is always succeeded by state b, state b remaining unchanged thereafter; state c is always succeeded by state d, which in turn is itself succeeded by state c, and so on indefinitely.

To anchor this rather abstract system in mind, give it a concrete economic interpretation. Let the system be the Canadian monetary system. Suppose that it is desired to record the state of each dollar in this system as at Sunday midnight each week, where state a means being in the Bank of Canada's vaults, soiled, and ready for burning; state b is the "destroyed" state; state c means being in the ownership of any consumer; and state d means being in the ownership of any investor. Then under the system described, each dollar bill that lies soiled in the Bank of Canada's vaults this Sunday midnight will be in the "destroyed" state next Sunday midnight, and will remain "destroyed" thereafter. And each dollar bill that is owned by a consumer this week will be owned by an investor next week, a consumer on the week following, and so on endlessly.

If this were exactly how the Canadian monetary system operated, one could form an isomorphic model of it with the following transformation K:

K: $\rightarrow$	
a	b
b	b
c	d
d	c

(1)

Suppose, however, that K's complexity forced adoption of an homomorphic model of K under which the model-builder decided not to discriminate state _a_ from state _b_, but to denote either as state _h_ instead. Under such an homomorphism, the system K should be described by the following transformation:

$\rightarrow$	
h	h
h	h
c	d
d	c

which, after removal of either redundant row h $\rightarrow$ h would be:

$\rightarrow$	
h	h
c	d
d	c

(2)

Of course other, different homomorphisms of K are possible, and might be recommended by different economists. For example, instead of blinding oneself to the difference between state a and state b, one could cease to distinguish between state c and state d, denoting either simply as state i. Then one should model the system K as follows:

→	
a	b
b	b
i	i
i	i

or, removing either redundant row i → i,

→	
a	b
b	b
i	i

(3)

A further possible homomorphism entails both the simplification of model (2) and that of model (3), i.e., lumping states a and b together under state h, and simultaneously lumping states c and d together under state i. This, (after the usual removal of redundant rows) gives the homomorphic model:

→	
h	h
i	i

(4)

Two further, different homomorphisms of K are possible. The first of these entails treating states b, c, and d simply as a new state, j. It gives the homomorphic model:

→	
a	a
j	j

(5)

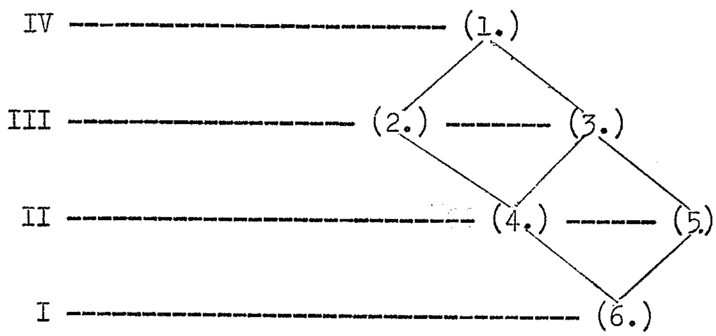
And finally, one may choose not to distinguish at all between the various states a, b, c, and d, but instead to treat them all as state m. Then one would get the simple homomorphism:

→	
m	m

(6)

The different homomorphisms presented above exhaust the possibilities for constructing meaningful models of the given system, (1).[6] Intuitively, one can readily grasp that some of these homomorphisms are "nearer" to their parent system than others. The models (1) through (6) may indeed be ranked in a lattice or hierarchy that shows which model entails which. For example, (1) entails (2), (3), (4), (5), and (6), since each and all of the homomorphisms (2) through (6) can be got from model (1). Similarly, since models (4) and (6) can both be obtained from (2), (2) is said to entail (4) and (6). By contrast, (3) cannot be obtained from (2), so that (2) does not entail (3); and for the same reason (3) does not entail (2).

All the hierarchial relationships among the six models are illustrated in the following diagram:



Here there are four <u>orders</u> of models, <u>viz</u>. I, II, III, and IV. Where a line connects two models of two different orders, the higher-order model represents a finer partitioning of the lower-order model. <u>E.g.</u>, (3) is a finer partitioning of (6) than either (4) or (5) is, while (1) is an even finer partitioning of (6); but (2) and (3) are not likewise refinements of each other, nor are the pairs (4) and (5). Higher-order models <u>entail</u> the lower-order models to which they are connected--and only those to which they are connected--even when this connection is more than one order removed; but the opposite does not hold: entailment does not pass from lower-order models to higher-order models. <u>E.g.</u>, the process represented by model (1) can be "perceived" by (say) model (2), but the process represented by model (2) cannot be "perceived" by model (1); thus, while either of the transformations $a \rightarrow b$ or $b \rightarrow b$ in model (1) entails the model (2) transformation $h \rightarrow h$, this latter transformation entails no definite transformation in model (1): there is no way that (1) can "know" whether to translate (2)'s "message," $h \rightarrow h$, into $a \rightarrow b$ or into $b \rightarrow b$. (In the terminology of Chapter III, higher-order homomorphisms contain more <u>information</u> that lower-order homomorphisms; information is lost as one descends the model hierarchy.)

If the construction of scientific models turns importantly on selecting "good" homomorphisms of the modelled phenomena, lack of a compelling criterion for identifying "good" homomorphisms is much to be deplored. The concept of model-hierarchies offers no panacea to the economist here, but it may furnish insight into at least one pervasive problem in economic theory, <u>viz</u>. the problem of Utility. For the notion of Utility in economics is but a species of homomorphism. It represents the economist's attempt to bring diverse elements of an economic problem (e.g., a problem of choice) under a single common denominator, so to speak. Symbolically, this is like the homomorphism under which the model

$\rightarrow$	
a	b
b	b
c	d
d	c

(1)

was represented simply as

$\rightarrow$	
m	m

(6)

In the model-hierarchy on page 48, each numbered node represents a <u>tradeoff compartment</u>. Each tradeoff compartment represents an exhaustive partitioning of any other compartment that is of a lower order than the first compartment, provided they are linked (i.e., connected by a line). For the economist, every problem of control is ultimately a problem of rational choice (see section 5, below), and may be represented by a homomorphism possessing a unique hierarchy address. (i.e., for any hierarchy upon whose lattice one's model-of-choice lies, it lies in one and only one compartment.) And every such problem is solved by a rather standard ritual of optimization according to some set of optimization criteria (see section 4, below). The ritual optimization procedure is based ultimately on an attempt to equalize the marginal Utility derived from the model's various sources of Utility (viz, its "outputs"), a procedure whose logic is sanctioned by the differential Calculus' first-order condition for maximizing (or minimizing) a continuous function. This procedure generally consists in a sequence of tradeoffs, whereby the impact is determined of incremental changes of a resource-allocation pattern upon the Utility derived therefrom, with the aim of "trading-off" resources from one employment to another until an optimum deployment results. Tradeoffs may properly occur only

(1) within the boundaries of a single compartment, or

(2) between two different compartments which

(a) lie on the same order, and

(b) do not entail any common lower-order compartment.

Otherwise, the Utility homomorphism is defective, and leads to invalid comparisons (and hence to invalid choice-decisions) of the well-known "apples versus oranges" type.

(Nothing in the foregoing discussion is intended to suggest that the role of Utility in model-construction is particularly unique, or uncommonly important. Utility stands here simply as one kind of homomorphism in economic theory; and its treatment as such is intended perhaps to illuminate a small part of this controversial notion.)

An example may be appropriate. The field of community health services shares, for the planner-administrator-economist, the vexing problem of most non-commercial enterprises; lack of a dominant criterion (i.e., profit) to orient choice in resource allocation. No one doubts that the aim of resource commitments to community health services is to secure community Utility of some sort, but practically speaking the Utility notion has little merit here. It is hard to tell, for example, whether the marginal Utility yielded by an expensive electro-mechanical resuscitation device, which, though rarely employed, can save a life when it is required, is greater than the marginal Utility to be obtained by using that machine's cost instead to secure an extra nurse for a year's service as an instructor of student nurses. Only in the vaguest sense are the nurse and the machine "equal" competitors for the same marginal expenditure. They do not belong in the same tradeoff compartment, so that the Utility homomorphism used to choose between them is unsatisfactory.

Now suppose instead that the activity, "community health services," were partitioned into the following mutually exclusive and exhaustive[7] categories:

(1) Control and prevention

(2) Treatment and restoration

(3) Long-term care and domiciliary maintenance

(4) Training

(5) Research

(6) Other

This partitioning comprises a tradeoff compartment. The resuscitation-machine may belong in category (2), while the nurse-teacher may belong in category (4). Thus they are competitors for a marginal dollar spent on community health services just in the indirect sense that

(2) Treatment and restoration, and

(3) Training

are distinct competitors. More directly, the resuscitation machine must compete with other alternative contributors to

(2) Treatment and restoration

(e.g., a heart-lung machine, a new operating room) for each marginal dollar that becomes available to that category of community health services, while the nurse-teacher must compete with other alternative contributors to

(4) Training

(e.g., programmed-learning texts, visual aids, etc.) for each marginal dollar that becomes available in that category.

4. Some Different Optimization Criteria.[8]

The task of economic control pre-supposes designation of a range within which it is desired to keep a specified set of variables. In turn, this requires some criterion by which one outcome of an economic process is deemed "better" than some other outcome; a criterion of optimality.

Broadly speaking, the putative controller of an economic process faces a problem of rational choice. He must choose a course of action (or a policy, or a strategy) A_i from a set of alternatives $A_1, A_2 \ldots\ldots A_m$ which are open to him. He does this knowing that his chosen A_i will result in some outcome a_{ij} which depends on (1) the particular A_i selected, and

(2) the "state of nature" S_j which happens to coincide with his A_i, where S_j belongs to a set of alternative possible states $S_1, S_1, \ldots, S_n$. Associated to each outcome a_{ij} is its utility u_{ij} to the controller.

If the controller knew with certainty which S_j would be associated with his A_i, then he could tell unfailingly which u_{ij} would result from his choice, and the most desirable A_i would in principle be clearly apparent. Moreover, if the controller knew the probability p_j associated to the occurrence of each S_j, he could still predict the outcome of each A_i statistically, and therefore know which A_i would be his best choice in some long-run, average sense. The controller's problem of choosing the best A_i stems, however, from his uncertainty about the S_j's. This uncertainty is the rule of economic phenomena, and is due to (1) basic ignorance of nature's true parameters, or, where these parameters are estimated statistically, (2) the lack of stationarity in such estimates (cf. Ch. I, sec. 5), and (3) the difficulty of interpreting any p_j on each separate, single occasion when a decision is required. (In the latter case, it may be simply meaningless to talk of probabilities.)

Luce and Raiffa[9] quote this simple example:

> Your wife has just broken five good eggs into a bowl when you come in and volunteer to finish making the omelet. A sixth egg, which for some reason must be either used for the omelet or wasted altogether, lies unbroken beside the bowl. You must decide what to do with this unbroken egg. Perhaps it is not too great an over-simplification to say that you must decide among three acts only, namely, to break it into the bowl containing the other five, to break it into a saucer for inspection, or to throw it away without inspection. Depending on the state of the egg, each of these three acts will have some consequence of concern to you, say that indicated by [the following table:]

Act	Good	Rotten
Break into bowl	Six-egg omelet	No omelet, and five good eggs destroyed
Break into saucer	Six-egg omelet and a saucer to wash	Five-egg omelet and a saucer to wash
Throw away	Five-egg omelet, and one good egg destroyed .	Five-egg omelet

If the controller is exceedingly pessimistic, he will assume that "nature" behaves like a malevolent opponent, always seeking to confront him with the particular S_j which minimizes the u_{ij} that he may enjoy for any A_i he happens to choose. Thus he will select the particular A_i which secures for him the maximum u_{ij} over the set of minimum u_{ij} associated with his set of possible choices, $A_1, A_2, \ldots, A_m$, viz.

$$A_i = \max_i \min_j u_{ij}$$

This criterion of choice is known as the Wald (or maximin) criterion. The chief difficulty of the Wald criterion is its extreme pessimism. If nature is anything better than a malevolent opponent the controller will forego utility needlessly. To prevent such waste he must adopt a different decision criterion.

Savage's minimax regret criterion is one such alternative, which, however, is still very conservative. Here, the aim is to minimize the maximum regret r_{ij} which the decision maker may suffer when nature's S_j is conjoined with his chosen A_i. For any state of nature S_j, the maximum regret r_{ij} is simply the difference between (1) the greatest u_{ij} that the controller could have enjoyed had he (with suitable prescience) selected the correct A_i for that particular S_j, and (2) the u_{ij} that he actually did enjoy. (Thus if the controller's A_i happened indeed to be the right choice, then the greatest u_{ij} he could have enjoyed would equal the u_{ij} he actually did enjoy, and his regret r_{ij} would be zero.). Symbolically, Savage's min-max regret criterion prescribes selection of

$$A_i = \min_i \max_j r_{ij}$$

where

$$r_{ij} = \max_i u_{ij} - u_{ij}$$

Hurwicz's pessimism-optimism index offers a somewhat more sophisticated approach to the criterion problem. Here the controller assigns a probability β to the worst outcome a_{ij} (or, in terms of its value to him, u_{ij}) associated with each A_i in the set $A_i, A_2, \ldots, A_m$. It, therefore, follows that the probability of any outcome except the worst is $(1 - \beta)$. The controller then chooses the A_i such that

$$\max_i A_i = \beta \min_j u_{ij} + (1 - \beta) \max_j u_{ij}$$

When $\beta = 1$, the controller fully expects the worst outcome, and the Hurwicz criterion is the same as Wald's maximin criterion. Thus the Hurwicz criterion embraces the Wald criterion as a special case, yet offering more flexibility to the controller. The chief shortcoming of the Hurwicz criterion is the estimate it requires of β. However, this weakness is not so objectionable as might first appear: the mere introduction of a pessimism index β permits the controller to trap β within certain bounds which, though the exact value of β be unknown, can decidedly assist his choice. For simple algebraic manipulation of the criterion

$$\max_i A_i = \beta \min_j u_{ij} + (1 - \beta) \max_j u_{ij}$$

leads to the expression

$$\beta = \frac{\max_i A_i - \max_j u_{ij}}{\min_j u_{ij} - \max_j u_{ij}}$$

This permits alternative A_i's to be compared with each other hypothetically, thereby generating an inequality relationship which shows the bounds within which β must lie if certain A_i's are to be preferred to others. (This general statement will be illustrated by a numerical example presently).

A further decision criterion, called the <u>principle of insufficient reason</u>, is associated with the name of <u>Laplace</u>.[10] It says simply that where, for a particular A_i, there are <u>n</u> distinct possible states of nature S_{i1}, S_{i2}, . . . , S_{in}, with associated outcomes a_{i1}, a_{i2}, . . . , a_{in}, and utilities u_{i1}, u_{i2}, . . . , u_{in}; and the controller has no reason to expect that each outcome a_{ij} is <u>not</u> equally probable; then he should assume them indeed equiprobable, and choose the action A_i which maximizes his (average) expected value, <u>viz</u>.

$$\max_i A_i = 1/n \sum_{j=i}^{n} u_{ij}$$

An important difficulty with the principle of insufficient reason is described by Luce and Raiffa:[11]

> Suppose we are confronted with a real problem in decision making under uncertainty, then our first task is to give a mutually exclusive and exhaustive listing of the possible states of nature. The rub is that many such listings are possible, and in general these different abstractions of the same problem will, when resolved by the principle of insufficient reason, yield different real solutions. For instance, in one listing of the states we might have: <u>s_1</u>, the organism remains fixed; <u>s_2</u>, the organism moves. In another equally good listing we

might have $\underline{s}_1$, the organism remains fixed; $\underline{s}_2$, the organism moves to the left; $\underline{s}_3$, the organism moves to the right. We can further complicate our description of the possible states of nature by noting which leg moves, whether the animal raises its head or not, etc.

The foregoing, different optimization criteria may be illustrated.[12]

Suppose the economist must choose one A_i among four possible policies (or acts), A_1, A_2, A_3, and A_4, in circumstances where four distinct states of nature, S_1, S_2, S_3, and S_4, whose respective probabilities are unknown to the economist, may occur. Let the utility u_{ij} associated with each possible outcome a_{ij} be according to the following payoff table:

	S_1	S_2	S_3	S_4
A_1	2,500.	3,500.	0.	1,500.
A_2	1,500.	2,000.	500.	1,000.
A_3	0.	6,000.	0.	0.
A_4	1,500.	4,500.	0.	0.

On the Wald criterion, the minimum payoff u_{ij} associated with

A_1 is 0.,

A_2 is 500.,

A_3 is 0.,

A_4 is 0.,

the maximum of these minima being 500., which is associated with A_2. Therefore, A_2 is the prescribed act.

On the minimax regret criterion, the given payoff table is converted to a maximum-regret table, column by column ($j = 1,2,3,4$), and row by row (i = 1,2,3,4), according to the procedure

$$r_{ij} = \max_i u_{ij} - u_{ij}$$

Thus when j = 1, clearly

$$\max_i u_{i1} = 2{,}500.$$

yielding the following entries for each row's intersection (i = 1,2,3,4) with column one (j = 1):

$$r_{11} = 2{,}500. - 2{,}500. = 0.$$

$$r_{21} = 2{,}500. - 1{,}500. = 1{,}000.$$

$$r_{31} = 2{,}500. - 0. = 2{,}500.$$

$$r_{41} = 2{,}500. - 1{,}500. = 1{,}000.$$

A similar procedure applied over the rest of the u_{ij} gives the regret matrix:

	S_1	S_2	S_3	S_4
A_1	0.	2,500.	500.	0.
A_2	1,000.	4,000.	0.	500.
A_3	2,500.	0.	500.	1,500.
A_4	1,000.	1,500.	500.	1,500.

Now the maximum regret associated with

A_1 is 2,500.,

A_2 is 4,000.,

A_3 is 2,500.,

A_4 is 1,500.,

of which the minimum is 1,500., i.e., that associated with A_4, which hence is the prescribed act.[13]

Application of the Hurwicz pessimism-optimism index begins by arraying the minimum and maximum payoffs u_{ij} associated with each A_i:

A_1: 0., 3,500.

A_2: 500., 2,000.

A_3: 0., 6,000.

A_4: 0., 4,500.

Now the pair A_2, A_3 are said to dominate the set A_1, A_2, A_3, A_4, since between them they possess better outcomes, and none worse than the outcomes associated with their complements A_1, A_4. Therefore, the decision-maker's choice is between A_2 and A_3. On the Hurwicz criterion, A_2 is preferred to A_3 if and only if there exists a measure of the probability of the worst payoff u_{ij} occurring, such that

$$\beta \min_j u_{2j} + (1 - \beta) \max_j u_{2j} > \beta \min_j u_{3j} + (1 - \beta) \max_j u_{3j}$$

viz.

$$\beta(500.) + (1 - \beta)(2,000.) > \beta(0.) + (1 - \beta)(6,000.)$$

viz.

$$\beta > 8/9$$

Similarly, A_3 is preferred to A_2 if and only if $\beta < 8/9$, while indifference between A_2 and A_3 is prescribed if and only if $\beta = 8/9$.

On the principle of insufficient reason, the expected value E associated with each act A_i is $E(A_i)$, as follows:

$$E(A_1) = \frac{1}{4}(2{,}500. + 3{,}500. + 1{,}500.)$$
$$= \frac{1}{4}(7{,}500.)$$
$$= 1{,}875.$$
$$E(A_2) = \frac{1}{4}(1{,}500. + 2{,}000. + 500. + 1{,}000.)$$
$$= 1{,}250.$$
$$E(A_3) = 1{,}500.$$
$$E(A_4) = 1{,}500.$$

of which 1,875., the expected value of A_1, is maximum. Therefore A_1 is the prescribed act.

5. A Model of Control

Let the set A of alternative acts open to the putative controller consist of the single act A_1, and the setS of alternative possible states of the environment consist of the four distinct states, S_1, S_2, S_3, and S_4, the distinct payoffs a_{ij} assocated with each S_j being determined by the payoff table:

	S_1	S_2	S_3	S_4
A_1	a_{11}	a_{12}	a_{13}	a_{14}

Here the quantity of variety V_S due to nature (in an impersonal sense, i.e., emanating from the environment) is 4, the quantity of variety V_A due to the controller is one, and the quantity of variety V_a present in the outcome is $4(= 4 \times 1)$. In these circumstances, it should be noted, the "controller" has no control over the outcome.

Now enlarge the set A by the addition of a further alternative act, A_2, which is open to the controller, producing a new payoff table:

	S_1	S_2	S_3	S_4
A_1	a_{11}	a_{12}	a_{13}	a_{14}
A_2	a_{21}	a_{22}	a_{23}	a_{24}

In these new circumstances,

$V_S = 4$

$V_A = 2$

$V_a = 8$ (= 4 x 2 = $V_S V_A$).

Now the controller does have some control over the outcome a_{ij}. But his control is not so great as nature's, in that the outcome a_{ij} is more determinable by nature's wider range of selection (4 alternatives) than by the controller's narrower range of selection (two alternatives).

Suppose the last payoff table is part of a (quasi-von Neumann-) Game for the survival of some economic system E. The controller seeks to ensure the survival of E against the working of an impersonal environment. The Game proceeds by a series of steps. At the first step, the environment selects some S_j, which is immediately known to the controller; at the second step the controller selects some A_i, which together with the S_j determines an outcome a_{ij}. If (i + j) is even, the system survives for a further two-step cycle; if (i + j) is odd, the system perishes and the Game ends.

Clearly, for this Game, the controller's degree of control is adequate to ensure E's survival. To do this he must act in accordance with the transformation

$\rightarrow$	
S_1	A_1
S_2	A_2
S_3	A_1
S_4	A_2

In the general case, the system E has associated to itself a set of outcomes a_{ij} which are determined jointly by the action A_i of a controller selecting his acts from a set of possible acts $A_1, A_2, \ldots, A_m$, and the state S_j of an environment which may assume any state in the set $S_1, S_2, \ldots, S_n$. At each successive step <u>t</u> in the operation of E, an output $a_{ij}(t)$ results. The controller's object is to ensure that E's output $a_{ij}(1), a_{ij}(2), \ldots$, belongs to a designated subset of the set of all possible outputs $a_{ij}(t)$, and only to that designated subset. To the extent that he succeeds in this object, he is said to <u>control</u> E.

6. <u>The Law of Requisite Variety</u>

In the preceding section it happened that E's total variety was equal to the product of the variety present in the environment and that at the controller's disposal. Though <u>a fortiori</u> E's total variety could not have been greater, it might have been less. For example, if, for all <u>i</u> and all <u>j</u>

$$a_{ij} = a^*$$

then the total variety V_a present in E would be one (i.e., unity). Thus the product $V_A V_S$, instead of determining the total amount of variety V_a present in E, sets an upper bound to it, i.e.,

$$V_a \leq V_A V_S$$

or

$$V_a/V_A \leq V_S \qquad (2.1)$$

(2.1) is an extremely important relationship. From the model of control presented in the preceding section, the essence of the control problem consists in coping with environmental variety. The controller, by appropriate selection of acts A_i, attempts to limit the outputs a_{ij} to some designated subset of the larger set from which they would come in the absence of his, and the presence only of the environment's, influence; which is to say, he tries to lessen the amount of variety present in the system's output. In terms of (2.1) the controller's object is to lessen the value of V_a/V_A. Now suppose that the variety V_A at the controller's disposal is unity, while that present in the environment is fixed at 100., which, suppose again, is also the fixed amount of variety present in the full set of outputs a_{ij}. Thus

$$V_A = 1.$$

$$V_S = 100.$$

$$V_a = 100.$$

which is consistent with (2.1), *viz*.

$$V_a/V_A = V_S \ (= 100.) \qquad (2.1)'$$

Here the controller's aim is to reduce the magnitude of the left-hand-side of (2.1)'. As V_a and V_S both are fixed, the only way he can do this is by increasing the amount of variety at his disposal, i.e., the magnitude of V_A. And since V_a is fixed, the controller, in adding to the number of A_i which are open to him, may use no a_{ij} which are not already present in the system. (An illustration of this point will be given presently.) Thus if he doubles the variety at his disposal, the controller reduces the magnitude of the left-hand-side of (2.1)' from 100., to 50.; which is consistent with the inequality (2.1). If he doubles V_A again, the left-hand-side of (2.1)' falls further, to 25.; and so on.

For any controller, _the only way of coping with environmental variety is by possessing, or acquiring, sufficient variety of his own. Only variety can overcome variety._ That is the Law of Requisite Variety.

To illustrate the Law of Requisite Variety, let E_1 be an economic system whose unemployment rate can fluctuate between one _per cent_ and 3 _per cent_. To start with, suppose that the economist who desires to control E_1's unemployment rate (e.g., to keep it at one _per cent_ or less) has open to him just the single course A_1 of allowing the environment to take whatever state S_j it will, and to suffer the outcome. This situation is represented by the following table:

	S_1	S_2	S_3	S_4
A_1	0	1	2	3

If E_1's succession of states-of-nature for (t = 1, 2, 3) is S_1, S_4, S_3, then the corresponding A_i over the same interval will be A_1, A_1, A_1, and E_1's output will be (0, 3, 2). All of E_1's variety is due to its environment, the economist having no control over its output.

Clearly the economist's task is to control E_1's output enough to limit its variety from 4, (i.e., 0, 1, 2, 3) to two, (i.e., 0, 1). By the Law of Requisite Variety, since here

$V_A = 1$

$V_a = 4$

$V_S = 4$

V_A must be doubled at least to secure his control for the economist.[14] For example, adding the vector $A_2 = (2, 3, 0, 1)$ to his options gives the economist the following payoff table:

	S_1	S_2	S_3	S_4
A_1	0	1	2	3
A_2	2	3	0	1

Now, by responding to any environmental state S_j according to the transformation

$\rightarrow$	
S_1	A_1
S_2	A_1
S_3	A_2
S_4	A_2

the economist is assured of control over E_1; so that if, as earlier, E_1's succession of states-of-nature for (t = 1, 2, 3) is S_1, S_4, S_3, then the corresponding A_i over the same interval will be A_1, A_2, A_2, and E_1's output will be (0, 1, 0).

It cannot be too strongly emphasized that the Law of Requisite Variety lies at the heart of all control problems, including, of course, the problem of controlling socio-economic systems. For the economist, be he concerned with stabilizing employment at a high level, or sustaining the growth rate of *per capita* national income, or distributing income according to a given pattern, or keeping balance of payments parameters within fixed limits, or in general with the optimum allocation of scarce resources--the essence of his task is to reduce the quantity of variety in the economic system's output. If all of this variety is due to environmental influences, and none of it is due to actions which lie within the economist's domain of choice, then he has no control over the subject system. *Only variety can "kill" variety*.

7. The Model and Conflict: Theory of Games

The thesis of the present section is: that the general problem of control of socio-economic systems is ultimately an *economic* problem, *viz*., of optimally allocating scarce, constrained resources in achievement of a stated set of ends.

It will surely by now have struck the reader that the model of control presented here resembles the paradigm of conflict-resolution in von Neumann and Morgenstern's *Theory of Games and Economic Behavior*.[15] Shubik, describing Game Theory, writes:[16]

> Game theory is a method for the study of decision making in situations of conflict. It deals with human processes in which the individual decision-unit is not in complete control of other decision units entering into the environment. It is addressed to problems involving conflict, cooperation, or both, at many levels. The decision-unit may be an individual, a group, a formal or an informal organization, or a society. The stage may be set to reflect primarily political, psychological, sociological, economic, or other aspects of human affairs.

The economist-controller of the present paper is the rational decision-maker of Game Theory; the environmental influences which confront the economist-controller with the various states of nature S_j, comprise in sum his "opponent." Instead, however, of the economist and his opponent making alternate moves, Game Theory requires that they move simultaneously on each play of the Game—an amendment to the model of control earlier presented that may now be made, as by placing that model at a higher-order address in its homomorphic hierarchy, it renders it a closer representation of the empirical world.[17]

That the problem of socio-economic control is formally a von Neumann-Morgenstern Game against the environment may not seem an unreasonable proposition. That this *ipso facto* makes it logically identical with the problem of optimally allocating constrained, scarce resources, is perhaps surprising. Yet this does appear to be the case.

Let two players confront each other in the usual kind of two-person, non-zero-sum Game, player I having the alternative strategies (p_1, p_2, p_3, and p_4) at his disposal, and player II the strategies (q_1, q_2, and q_3). Let this Game be played for some value *v*, governed by the following pay-off table:

Player II

		q_1	q_2	q_3
	p_1	4	2	3
	p_2	5	4	1
Player I	p_3	1	1	0
	p_4	2	2	1

There is no saddle-point.[18] Therefore player II's problem[19] is to select a mixed strategy that will hold player I's winnings to the Game's inherent value $\underline{v}$, at most. What player II seeks is a solution for:

$$
\begin{aligned}
&q_1 + q_2 + q_3 = 1. \\
&4q_1 + 2q_2 + 3q_3 \leq v \\
&5q_1 + 4q_2 + q_3 \leq v \\
&q_1 + q_2 \leq v \\
&2q_1 + 2q_2 + q_3 \leq v
\end{aligned}
\tag{7.1}
$$

Dividing each line in (7.1) by $\underline{v}$, yields:

$$\bar{q}_1 + \bar{q}_2 + \bar{q}_3 = 1/v \tag{7.2}$$

$$4\bar{q}_1 + 2\bar{q}_2 + 3\bar{q}_3 \leq 1. \tag{7.3}$$

$$5\bar{q}_1 + 4\bar{q}_2 + \bar{q}_3 \leq 1. \tag{7.4}$$

$$\bar{q}_1 + \bar{q}_2 + 0\bar{q}_3 \leq 1. \tag{7.5}$$

$$2\bar{q}_1 + 2\bar{q}_2 + \bar{q}_3 \leq 1. \tag{7.6}$$

So player II's problem amounts to the need of maximizing the objective function (7.2)--recall that player II seeks to minimize $\underline{v}$ for player I, which is logically equivalent to maximizing the value of the objective function, (7.2)--subject to the constraints described by the inequalities (7.3), (7.4), (7.5), and (7.6). That II's optimum course turns out to be the random, 50-50 mixture of q_2 and q_3 (thereby holding player I down to $v = 2.5$, which the latter may assure himself by randomly mixing p_1 and p_2 in the proportions 3/4 to 1/4) is almost beside the point in this instance. The important point to note here is that player II's problem is but a particular instance of a linear programming problem, viz.

Maximize $\sum_j c_j q_j$

Subject to $\sum\sum_{ij} a_{ij} q_j = v$

which in turn is a paradigm of the general economic problem of optimum resource allocation.[20] Moreover, the logical equivalence in general of matrix Games and linear programs has been proved mathematically.[21] Therefore, the general problem of control of socio-economic systems is ultimately an economic problem, viz. of optimally allocating scarce, constrained resources in achievement of a stated set of ends.

8. The Effect of Time: The Prisoner's Dilemma

Hiawatha, mighty hunter
He could shoot ten arrows upwards
Shoot them with such strength and swiftness
That the last had left the bowstring
Ere the first to earth descended.
This was commonly regarded
As a feat of skill and cunning.

One or two sarcastic spirits
Pointed out to him, however,
That it might be much more useful
If he sometimes hit the target.

Why not shoot a little straighter
And employ a smaller sample?

Hiawatha, who at college
Majored in applied statistics
Consequently felt entitled
To instruct his fellow men on
Any subject whatsoever,
Waxed exceedingly indignant
Talked about the law of error,
Talked about truncated normals,
Talked of loss of information,
Talked about his lack of bias
Pointed out that in the long run
Independent observations
Even though they missed the target
Had an average point of impact
Very near the spot he aimed at . . .

--Maurice G. Kendall[22]

The model of control presented so far is a static model. It will now be rendered dynamic.

Suppose that tomorrow morning at 3 a.m, the police come to Mr. Smith's home and arrest him. They take him to the city jail, where they hold him incommunicado, and tell him:

"We have just arrested your best friend, Mr. Jones, and are holding him, too, incommunicado in another part of this jail. We strongly suspect that you and your friend are responsible for the Great Train Robbery, but we do not have quite enough evidence to secure a conviction. We need one of you to testify against the other, to cinch our case. If you sign this statement implicating your friend, then we can certainly get him convicted and sentenced to ten years. That will satisfy us, and we will set you free on account of your co-operation. However, if you refuse to co-operate we will book you on some petty, trumped-up charge (like carrying an offensive weapon--your pocket knife is an offensive weapon, you know), which is sure to cost you a year. It's only fair to add that our colleagues are at this moment offering the same 'deal' to your friend, Jones. If each of you implicates the other, then we are bound to proceed against you both in the Great Train Robbery case--political pressure, you understand; but you will then be sentenced to nine years, instead of ten, owing to your joint co-operation with the authorities. You have ten minutes to think it over."

What should Mr. Smith do?

If he had studied Game Theory, Smith would recognize his plight as a two-person, non-zero-sum Game;[23] and he might accordingly draw up the following payoff matrix:

Smith \ Jones	Silence	Talk
Silence	(-1, -1)	(-10, 0)
Talk	(0, -10)	(-9, -9)

Then he might reason as follows:

"If I am silent and Jones is silent, the ensuing outcome brings us the least collective harm; but if I choose silence, Jones would be best off to talk, and he might indeed choose that course: then I would get ten years and he would be freed. On the other hand, if Jones does talk and I, too, talk, at least he will not get off scot free; and there is always the chance that he will remain silent, setting me scot free. So the worst that can happen to me if I am silent is a ten year prison sentence; the worst that can happen to me if I talk is a nine year prison sentence; in order to protect myself from Jones' betrayal--he knows Game Theory, too, after all, and doubtless feels compelled to reason exactly as I feel compelled to reason--I must choose the 'least-worst' course. I will talk."

So will Jones, if he is rational. And both will draw merely a nine year term (instead of a ten year term) as the fruit of their rationality. Ironically, by being irrational--or co-operative--together, each would have fared far better.

The foregoing illustration[24] is of a particular kind of Game in the literature of Game Theory: the Prisoner's Dilemma.[25] The "correct" solution to the Prisoner's Dilemma is indeed to "talk." Since the correct strategy is not affected by adding an equal amount to every payoff, let the number six be added to each payoff in the table on page 86 above. The resulting payoff matrix may be further generalized by replacing Smith's "Silence" strategy with the label "A_1" and his "Talk" strategy with the label "A_2." Similarly Jones' strategies may be transformed into "B_1" and "B_2." The following payoff matrix results:

	B_1	B_2
A_1	(5, 5)	(-4, 6)
A_2	(6, -4)	(-3, -3)

It will now be given a particular economic interpretation.

Perhaps the most abused character in economic literature is Robinson Crusoe. He stands for the antithesis to division of labour. At risk of swelling this martyr's martyrdom, suppose that two Robinson Crusoes share the same remote, South Pacific island. Designate them Crusoe-A and Crusoe-B respectively. Let each be equally an independent chap, fully motivated by the kind of ethos that so sturdily supports classical economic theory.

One fine day Crusoe-A and Crusoe-B are confronted by an economic task which furnishes them their first opportunity of mutual co-operation. By the nature of this task, an instant of time is at hand when each of them must simultaneously--and just once--act either co-operatively (strategy one) or selfishly (strategy two). Their payoffs are shown in the table of page 87, above.[26] This is essentially the Prisoner's Dilemma Game already alluded to, with the rational strategy for each player being to decline co-operation with the other. If Crusoe-A and Crusoe-B could trust each other with certainty, then they could profit mutually by choosing A_1B_1. But they are playing a one-shot Game, with no preplay communication,[27] so that each player simply cannot risk being a "sucker" for the other. So the two Crusoes continue to live without the blessings that co-operative trust could bring (e.g., division of labour).

What would happen if this were a two-shot Game? Could the players then afford to trust each other? The answer is, No. For each player, the reasoning behind this answer runs somewhat as follows: "The final iteration of this Game is simply a one-shot Game all over again, with my (non-co-operative) strategy clearly dictated as in any one-shot Game. With iteration number two thus strictly determined, I have only iteration number one left to consider. But this, too, has now become a one-shot Game, with my course (i.e., non-co-operation) again clearly determined. Hence I must play strategy two on both iterations." And this reasoning is generally correct for any finite number of iterations of the same game.

But note that by dynamizing the Crusoes' Game--by expanding the time dimension from a single iteration to n iterations--one opens up the possibility that one shrewd Crusoe may teach the other to co-operate, and thereby secure a mutually greater payoff over the whole course of the Game.[28] Suppose, for example, that n = 1,000. Crusoe-A, preferring (if he can get it) a 1,000 x 5 = 5,000 utils' gain to a 1,000 x (-3) = 3,000 utils' loss, might start by playing A_1 instead of A_2, as a hopeful gesture to Crusoe-B. (Crusoe-A thus attempts to control the Game's outcome by a form of communication with Crusoe-B). Crusoe-B may or may not get the message at first, but at least the fact of a greater-than-one-period time horizon creates the opportunity for a form of communication. Rebuffed, Crusoe-A might "punish" Crusoe-B by a deliberate run of ten A_2's before again attempting A_1. He might indeed implement the policy of trying A_1 on every 10th iteration, to be continued whenever Crusoe-B responds in kind; and of punishing Crusoe-B with 10 A_2's each time Crusoe-B plays B_2. This would be precisely the sort of teaching by operant-conditioning that Pavolv made famous, and that has been the basis of the successful "programmed-learning" teaching technique of Skinner[29] and the impressive mathematical simulations of human learning modelled (for example) by Feigenbaum.[30]

If the control model put in a (temporally) dynamic form sets the stage logically for development of social co-operation, what can be said about human psychological propensity to exploit this setting? What has been said on this question takes the form of (1) authoritative conjecture, and (2) experimental result. For authoritative conjecture one may fairly turn to Luce and Raiffa,[31] who write:

> We feel that in most cases an unarticulated collusion between the players will develop, much in the same way as a mature economic market often exhibits a marked degree of collusion without any communication among the participants. This arises from the knowledge that the situation will be repeated and that reprisals are possible.

For experimental evidence, there is the brilliant work of Anatol Rapoport, director of the University of Michigan's Mental Health Research Institute. Rapoport's experiments[32] aimed explicitly at the question of co-operative-competitive psychological interaction in a Prisoner's Dilemma situation. His results amply confirm the conjecture of Luce and Raiffa:

> We have examined the correlation coefficients in the various runs. A positive value of this coefficient would indicate that cooperative responses of one subject tend to elicit cooperative responses of the other. A negative value would indicate that opposite responses tend to be elicited. A value of the correlation coefficient near zero would indicate that the responses are essentially independent of each other. . . . It turns out that the values of the correlation coefficient are strongly biased toward the positive end. . . . Moreover, the coefficient increases with time, starting about 0.2 and reaching values of 0.7 and 0.8. The players not only play like each other; they tend to play more and more like each other as plays are repeated.
>
> . . . Once a pair has "locked in" on cooperation . . . , it is highly unlikely (one chance in twenty) that one of them will defect on a particular play.

As the Crusoes thus learn to co-operate, and proceed to fashion their economic and political institutions on this basis, the value of continuing to trust each other, and the penalty for reverting to the strategy of untrust, both grow apace. Moreover the passage of time increases the players' explicit awareness of the payoff matrix, whether in the sense of the material evidence of co-operation's rewards pressing itself upon their attention, or in the sense of their growing sophistication and insight into the theoretical aspects of their Game. This, too, adds impetus to the strategy of co-operation,[33] at once enabling and encouraging the players to further lengthen their planning horizon.

The rationale for the kind of economic co-operation described here depends on the additivity of payoff matrix utils over time. Since for the average person reared in the traditions (and the prejudices) of Western culture the relevant time-stream extends indefinitely into the future, it is legitimate to raise the question how a (divergent) infinite series of future payoffs could be meaningfully evaluated, for the purpose of choosing (as any Crusoe eventually, and perhaps repeatedly, must) between a static and a dynamic Game (or, given a dynamic setting, between a selfish and a co-operative policy). Historically, the method that appears to have evolved is the method of <u>discounting</u>. An util now is worth more than an util on some future iteration of the Game. This convention serves to make the series of future payoffs convergent, and its associated sum finite (despite the time-stream's infinite length). Together with the development described next, it comprises a plausible (and logically clearly-defined) explanation of how <u>interest rates</u> came about.

> Consider the decisions of a task force commander in a naval engagement. He must try to get the most from the particular items that are at his disposal—destroyers that are now in force, man hours available for maintenance and operations, ammunition on hand, and so on. It is beside the point that, by allocating money in a budget differently, he could have equipped a task force with another aircraft carrier at a sacrifice of so many destroyers. The option of shifting basic resources from the production of one item to the production of another is hardly open at this stage of the game. Hence, in this situation, it is not a budget that constrains the actions of the commander. Limited stocks of specific items <u>are</u> the constraints, and they should be expressed in that fashion.[34]

Like Hitch's task force commander, when economic man's planning horizon is very short (i.e., the Robinson Crusoe stage of economic organization), the resource constraints upon his economic activity are seen in terms of the real resources lying at hand. Robinson Crusoe's at-hand resource mix cannot be changed simply by wishing it were different: he is stuck with it, and with the job of planning in terms of specific, concrete things. And these things are easy to see. But, as Crusoe lengthens his planning horizon, it becomes increasingly difficult for him to assess even the order of magnitude of his future resources, let alone their exact mix. Then he needs some kind of value- or accounting-unit that will overcome this difficulty, and "money" is a natural kind of answer. The invention of what is now called money is thus a readily understandable--in an *ad hoc* sense it is an inevitable--development in man's effort to lengthen his planning horizon. It enables him to order his uncertainty as to the future, in a perhaps even more fundamental sense than Keynes had in mind.

It is interesting to note that the ploy of discounting future payoffs in order to render an indefinitely long series of such payoffs convergent, while an attractive explanation of how (in conjunction with other factors) interest rates came about, is not the only possible technique of bringing stable co-operation to a Prisoner's Dilemma Game. It can be shown mathematically that co-operative stability in the kind of economic Game considered here can be guaranteed simply by assigning (within a suitable range) an inescapable probability that the Game will terminate at each iteration.[35] In the real world of socio-economic phenomena, there is a dispassionate Law of Nature which does this now.

[1]Kenneth E. Boulding, "Social Sciences," The Great Ideas Today (Chicago: Encyclopaedia Britannica, Inc., 1965), p. 281.

[2]Quoted by Carlos Fallon, "Principles of Value Analysis," in Wm. D. Falcon, (ed.) Value Analysis Value Engineering [sic] (New York: American Management Association, 1964), p. 84.

[3]Quoted by James R. Newman (ed.), The World of Mathematics (New York: Simon & Schuster, 1956), Vol. 3, p. 1832.

[4]This terminology is Ashby's. See W. Ross Ashby, An Introduction to Cybernetics (New York: John Wiley & Sons, 1963).

[5]Immanuel Kant, Critique of Pure Reason (transl. Norman Kemp Smith; London: Macmillan & Co. Ltd., 1963). Kant also warned that concepts without percepts are empty, which is pretty much the case when homomorphism becomes an end in itself, shutting itself off from the substantial, informing (in the Aristotilean sense of "informing") influence of real-world percepts.

[6]To be sure, further homomorphisms are logically possible; but these entail one-many transformations, which will only be treated later on in the present paper, viz. Chapter IV (Markov processes).

[7]As a layman in matters of community health, the present author cannot, of course, tell whether this particular partitioning is indeed exclusive and exhaustive. It is adapted from Frankel, Marvin, "Federal Health Expenditures in a Program Budget," in David Novick, (ed.) Program Budgeting (Cambridge: Harvard University Press, 1965), pp. 208-247. The main idea is that the separate categories be (more or less) distinctive competitors for each marginal dollar spent on community health services.

[8]The discussion in this section relies much on the following two sources: (1) Duncan R. Luce and Howard Raiffa, Games and Decisions (New York: John Wiley & Sons, Inc., 1957), esp. pp. 278-286; and John L. Dillon and Earl O. Heady, Theories of Choice in Relation to Farmer Decisions (Ames: Iowa State University, Agricultural and Home Economics Experiment Station, Research Bulletin 485, October, 1960). But neither source is here accepted uncritically. E.g., the present notation does not entirely follow Luce and Howard, while Dillon and Heady's work is elliptical and confusing in some places. Also, see fn. no. 10, below.

[9]Luce and Raiffa, op. cit., p. 276. They attribute this illustration to Savage.

[10]This is according to Dillon and Heady, who in op. cit., p. 907, say: "The Laplace principle of insufficient reason is the oldest of all

decision theories. It specifies that since information about the likelihood of occurence of the various possible states of Nature is zero, the decision maker should act as though each of Nature's states has an equal chance of being the true state, the state to be realized." But cf Luce and Raiffa, op. cit., p. 284: "The principle of insufficient reason, first formulated by Jacob Bernoulli (1654-1705), states in boldest terms that, if there is no evidence leading one to believe that one event from an exhaustive set of mutually exclusive events is more likely to occur than another, then the events should be judged equally probable."

[11]Loc. cit.

[12]The figures and the computational results which follow are from Dillon and Heady, op. cit., pp. 907-8.

[13]This prescription holds just if a pure-strategy solution alone be permitted, a mixed-strategy solution being barred. Permission of a mixed-strategy solution allows the expected regret shown in the text (1,500) to be reduced to 1,365, on randomly, sequentially choosing acts A_1, A_3, and A_4 in the proportion 9:23:68.

[14]This assumes (a) that the elements (0, 1, 2, 3), and they alone, are used in adding to the economist's optional actions, and (b) that, though the elements (0, 1, 2, 3) may be permuted any way, no element may be repeated in any new vector A_i. One reason for these assumptions is to rule out absurd solutions like ($-6, -\infty$ $-500, -1$), or trivial ones like (1, 1, 1, 1). A further reason is that no information-theoretic measure of E's variety is yet at hand up to the present point in the text (this is presented in Ch. III, sec. 1.), so that exhaustive treatment of this illustration would be cumbersome, and would add labour in a way that is not germane to the reader's understanding of the Law of Requisite Variety--the main point at issue here.

[15]John von Neumann and Oskar Morgenstern, Theory of Games and Economic Behavior (New York: John Wiley & Sons, Inc., Science Editions, 1964).

[16]Martin Shubik, "Game Theory and the Study of Social Behavior: An Introductory Exposition," in Martin Shubik (ed.), Game Theory and Related Approaches to Social Behavior (New York: John Wiley & Sons, Inc., 1964), p. 8.

[17]The two-step cycle described earlier was intended just to facilitate exposition.

[18]A "saddle-point" denotes a solution prescribing a pure strategy (p_i, q_j) such that, by application of Wald's maximin criterion (cf. sec. 4 of the present Chapter),

$$\max_i \min_j u_{ij} = \min_j \max_i u_{ij} = v$$

The graphic representation of a saddle-point and its neighbourhood looks indeed like a conventional saddle: hence its name.

[19]Ignoring player I's problem, as the present illustration does, does not compromise the generality of the point at issue, since (a) player I's problem is simply the dual (in the technical, linear programming sense) of player II's, and (b) the present illustration is hardly a general case anyway, and is intended just as an introduction to the general statement which follows later in the text.

[20]Of course the linear programming formulation of the resource-allocation paradigm assumes a linear objective function and a linear set of constraints. But this does not compromise the point at issue, because (a) the assumptions are, on the whole, more realistic than the assumptions underlying traditional marginal analysis (hence the substantial recent impact of linear programming in reformulating the theory of the firm), and (b) they do not entail a linear production function, and (c) this paradigm can be generalized to the non-linear case by the techniques of "non-linear programming." Vide George B. Dantzig, Linear Programming and Extensions (Princeton: Princeton University Press, 1963), pp. 471 ff.

[21]Ibid., pp. 286-291.

[22]Quoted in Richard Bellman, Adaptive Control Processes (Princeton: Princeton University Press, 1960), pp. 148-9.

[23]A zero-sum Game is one where the algebraic total of opponents' payoffs is zero, i.e., each util lost by one player is gained by his opponent(s). In a non-zero-sum Game, the algebraic total of opponents' payoffs is not zero, e.g., due to external economies, increasing returns to scale, or simply because each player assigns a different utility to the same outcome.

[24]Due to A. W. Tucker, per Luce and Raiffa, op. cit., pp. 94 ff.

As to the question whether the use of two-person Games is not perhaps a too-simplistic way to approach important issues like the control of socio-economic phenomena, the classic opinion of von Neumann and Morgenstern may be cited:

> We believe that it is necessary to know as much as possible about the behavior of the individual and about the simplest forms of exchange. This standpoint was actually adopted with remarkable success by the founders of the marginal utility school, but nevertheless it is not generally accepted. Economists frequently point to much larger, more "burning" questions, and brush everything aside which prevents them from making statements about these. The experience of more advanced sciences, for example physics, indicates that this impatience merely delays progress, including that of the treatment of the "burning"

questions. There is no reason to assume the existence of shortcuts.

(von Neumann and Morgenstern, op. cit., p. 45.)

[25] See, for example, Anatol Rapoport and Albert M. Chammah, Prisoner's Dilemma (Ann Arbor: The University of Michigan Press, 1965).

[26] The numbers shown there are utils of some kind or other, as indeed were the numbers of the previous illustration.

[27] Given some dependable system of sanctions for enforcing a preplay agreement, the agreement and its concomitant sanctions (e.g., rewards, penalties) could be reflected in the payoff matrix. However this would unnecessarily complicate the present illustration; hence the assumption of no preplay communication.

[28] It is here assumed that the players' utils are additive.

[29] James G. Holland and B. F. Skinner, The Analysis of Behavior (New York: McGraw-Hill, 1961).

[30] Edward A. Feigenbaum, "The Simulation of Verbal Learning Behavior", in Edward A. Feibenbaum and Julian Feldman, (eds.), Computers and Thought (New York: McGraw-Hill, 1963), pp. 297-309, although this entire volume is an excellent source of work along these lines. The interested reader should not overlook the stochastic learning models of Estes and Bush. Cf. W. K. Estes and C. J. Burke, "Application of a Statistical Model to Simple Discrimination Learning in Human Subjects," Journal of Experimental Psychology, Vol. 50, 1955, pp. 81-88; and R. R. Bush and F. Mosteller, Stochastic Models for Learning (New York: John Wiley & Sons Inc., 1955).

[31] Luce and Raiffa, op. cit., p. 101.

[32] These are reported in Anatol Rapoport, Strategy and Conscience (New York: Harper & Row, 1964) pp. 157-8, from which the quoted excerpt comes; and, far more extensively, in Rapoport and Chammah, op. cit.

[33] "Does the amount of cooperation depend on whether the players see the matrix?"

"Yes. Cooperation is about twice as frequent when they see it as when they do not."

(Rapoport, Strategy and Conscience, p. 154).

[34] Charles J. Hitch and Roland N. McKean, The Economics of Defense in the Nuclear Age (Cambridge: Harvard University Press, 1963), p. 24.

[35] Luce and Raiffa, op. cit., Appendix 8, pp. 457 ff.

CHAPTER III

INFORMATION AND DECISION PROCESSES

> It is a very inconvenient habit of kittens (Alice had once made the remark) that, whatever you say to them, they always purr. "If they would only purr for 'yes' and mew for 'no' or any rule of that sort," she had said, "so that one could keep up a conversation! But how can you talk with a person if they always say the same thing?"
>
> —Lewis Carroll[1]

1. Information, Uncertainty, and Surprise.

A necessary condition of control is the communication of information. In the naive zero-sum model, the controller needs to know what is the state S_j of the environment, or at least what its states have been in the past. In the more sophisticated Prisoner's Dilemma model, communication of information proceeds in a subtler way.

The process of communication is the process whereby a particular selection of objects from among a set of candidates for selection, is transmitted from sender (or selector) to receiver. Note that the selection is what gets transmitted, not necessarily the objects. This selection is embodied in a message, which contains more or less—i.e., a greater or smaller quantum of—information. If the sender's selection was completely determined in advance by the fact that he had no choice in the matter, and the receiver knew this beforehand, then the message will come as no surprise to the receiver; and it is said to contain no information. By contrast, if the sender had so much choice in his selection that the receiver could hardly have guessed in advance what the selection would be, then the message will indeed come as a surprise to the receiver (more or less as the sender's actual selection is improbable), and the message is said to contain more or less information accordingly.

Suppose Jones tosses a fair coin repeatedly, sending a sequence of messages to Smith about the result of each toss. The set of possible messages from which Jones must choose a single message after each toss possesses two elements: (1) heads (or "H"), and (2) tails (or "T"). Each time Jones makes a selection, the probability p_i that he will select either of these two elements is equal to one-half, i.e., $p_1 = Pr(H) = p_2 = Pr(T) = 0.5$. This in turn is a measure of the uncertainty in Smith's mind that each message from Jones settles. Two is the number of possible messages from which Jones' actual message to Smith is chosen. Another way of writing this number (i.e., "two"), which in the general case can be called "N", is 2^1, i.e., $N = 2^1$, so that $\log_2 N = 1$. Now it happens that the expression of choice among possible messages, _in binary terms_ (where each possible message either "is" or "is-not" selected, and hence falls always into one of just two distinct states), is particularly convenient mathematically;[2] and from this consideration is quite naturally derived the fundamental unit for measuring information quanta: the "binary digit," which is conventionally shortened to the term _bit_.

Thus one "bit" of information is the amount arising from selection between two equally probable alternatives, two bits is the amount of information arising from selection among 4 equally probable alternatives (i.e., $\log_2 4 = 2$), three bits is the amount of information embodied in a distinct selection from among 8 equiprobable alternatives (i.e., $\log_2 8 = 3$), and so forth. In general, where p_i is the probability that the sender will select the i^{th} member of a set of n possible messages (where a "message" may be a number, a letter, a stock phrase, etc.) then the amount of information contained in each selection is

$$- p_i \log_2 p_i$$

and the amount of information contained in a sequence of such messages is

$$H = -\sum_i p_i \log_2 p_i \qquad (3.1)$$

When Jones tosses his coin just once and then tells Smith the result, he is communicating one bit of information to Smith, provided the coin is a "fair" coin. Suppose for a moment it were not: suppose it were a trick penny that always landed "heads." Then by (3.1) Jones should communicate no information to Smith each time he informed Smith the outcome of a toss: for Smith can anticipate Jones' message with absolute certainty, and hence is "surprised" by it (in the sense that it settles prior uncertainty in his mind) not at all. Again, suppose Jones' penny were biased in such a way that it landed heads one-third of the time and tails two-thirds: once again, on (3.1), each of Jones' messages to Smith would contain (approximately) 0.91 bits of information, on average.

Some further examples of information content may be helpful at this point. The outcome of a fair die yields 2.58 bits of information per throw. When one selects a card at random from an ordinary deck of playing cards, one gets 5.70 bits of information on learning the outcome of one's selection. If Smith wishes to know what day of the year is Jones' birthday, and Jones tells him, Jones has given Smith 8.53 bits of information.

2. Redundancy and Constraint.

The best way to get a clear idea of "redundancy" is to start with an important, concrete example: the English language. If, in composing socially meaningful messages in English, the selection of every particular linguistic symbol were equally likely, one could easily measure the amount of information in any particular message by applying formula (3.1). For example, if the entire inventory of linguistic symbols for English consisted of the 26 letters of the alphabet, plus a space, and each of these 27 symbols were equally likely of selection, then each letter (or space) in a printed English message would contain about 4.75 bits of information. This is the maximum amount of information that an inventory of 27 symbols could yield, because it assumes equal probability for the selection of each separate symbol. If each available symbol were not in all circumstances equally likely to get selected, then the average information content per symbol of English message would be something less than 4.75 bits. Now it is a matter of common fact that in construction of messages in English, each possible symbol is not equally likely to be selected. (For example, the letter J will almost never follow the letter Q, whereas the letter U almost certainly will). Thus in composing any socially meaningful message in English, one's freedom to select symbols is considerably constrained (by the rules of grammar, spelling, etc.). The amount of information per symbol that an author conveys is less than the amount he could convey if he enjoyed complete freedom of choice, and were not bound by established conventions. Suppose these conventions constrain him to 2.375 bits per symbol.

Then the amount of redundancy R in his message would be

$$R = 1.000 - \frac{2.375}{4.750} = 0.500$$

This would mean that one-half of anything he writes is dictated by his free choice, while the remaining one-half is determined by the (socially-contrived and socially-enforced) rule of his language. In general

$$R = 1.0 - H/H_{max} \qquad (3.2)$$

And (3.2) is a definition of <u>redundancy</u>.

3. <u>Information, Redundancy, and Control</u>.

The notion of information presented here is but a rigorous way of describing variety (Ch. II, sec. 6). Practically everything said about variety and control can be said also about information and control. The essence of the control problem consists in coping with environmental information. The controller, by appropriate selections of acts A_i, attempts to limit the outputs a_{ij} of some system E to some designated subset of the larger set from which they would come in the absence of his, and the presence only of the environment's, influence; which is to say, he tries to lessen the amount of information present in the system's output. If, using an information theoretic measure, the amount of information present in the system's output be designated H_a, while that embodied in the controller's variety be designated H_A, and that emanating from the environment H_S, then by the Law of Requisite Variety

$$H_a \leq H_S + H_A \qquad (3.3)$$

Thus the total information present in a system's output cannot exceed the sum of what is put into the system by the controller and the various environmental influences. Moreover, for purposes of control

$$H_a - H_A \leq H_S \tag{3.4}$$

which is to say that the controller, in seeking to reduce the information content of the subject system's output, cannot do better than subtract from it (at most) the amount of information at his disposal. Again, for any controller, the only way of coping with environmental information is by possessing, or acquiring, sufficient information of his own.

This last statement (as did its earlier form, in terms of variety alone) assumes a fixed, given quantity of H_S. If, as is often the case in socio-economic phenomena, the environment is vulnerable to constraint, then the controller has a second course open to him: deliberate application of redundancy. Enforceable legislation, social customs, and binding contracts are some examples of redundancy applied to the socio-economic environment. Were it not for the presence of such redundancy in the workday world, we should all suffer from near-overwhelming environmental information overload, and find it impossible to control more than a small fraction of what we now control. In the real world, our socio-economic institutions give us much of the redundancy we possess. They constrain our freedom of action in rather artificial ways which, like the grammar of our language, are socially-contrived and socially-enforced. They thus dictate a certain percentage of our economic activity. Our traffic lights and our income tax laws, our subsidies and deficiency payments, our exchange rates and marketing boards and sales taxes and interest rates, the volume of money kept in circulation (indeed the very institution, "money," together with the conventions governing its use), the commercial machinery for borrowing and lending--all are techniques of deliberate (i.e., in the sense of logically non-necessary) redundancy which have evolved more or less the way Topsy "growed," and which are ripe targets for the economist's optimization paradigms.

4. Bayesian Inference and Information.

In Chapter I it was said that an important notion for control theory was "significance," playing as it does a key role in the controller's decision (or choice) processes. Significance depended upon (1) how the controller chose to partition his subject phenomena, (2) his stock of *a priori* judgements and information, and (3) his willingness to risk error. Now it is possible to be somewhat more precise in discussing this view of decision processes.

Partitioning of the subject phenomena represents an attempt to impose constraints upon the environment. To be sure, it is generally not possible to constrain the set of S_j's so thoroughly as to remove the environment as an information source to a system's outputs. But *some* improvement along these lines is possible in most cases. A trite example might be the busy university professor who partitions each working day into three parts: (1) all matters connected with his private, professional research get his full attention from 8 a.m. until noon; (2) all matters connected with his personal business get his full attention from noon until 2 p.m.; and (3) all matters connected with administration, lectures, students, and third parties generally, get his full attention during the interval 2 p.m. to 6 p.m. This is not to say that the professor's environment behaves unfailingly according to his partitioning; but it is to say that his partitioning will reduce the environmental information load his outputs might otherwise have to bear, and that he will accordingly be able to spend more of his information "stock" on (say) producing a subtly controlled output, and less of it on fighting environmental disturbance. Similarly, the putative controller of a business enterprise tries to constrain the "noise" emanating from its environment by devising an appropriate accounts structure which will partition his "noise" into Sales, Direct Expenses, Overhead Expenses, etc. And the same principle holds, of course, at the levels of the national and international economic system.

The controller's stock of _a priori_ information (and how it changes over time), together with his willingness to risk error, are bound up with the question of statistical inference. In Chapter I there appeared the illustration of a person who is given an urn containing ten balls, some white and some red. He draws at random a single ball from this urn, and notes its colour. His _a priori_ belief as to the true proportion of white _versus_ red balls in the urn will determine his degree of surprise at whatever the result of his random selection; which is to say that his _a priori_ hypothesis will determine the amount of information conveyed to him by the outcome of his random selection. Too, this information will modify his _a priori_ hypothesis: the random selection's outcome changes the selector's information stock (which is embodied in his hypothesis). This _is_ learning; and the learning process here underway is aptly characterized as Bayesian, according to the expression

$$p(H_i \mid E) = \frac{p(E \mid H_i)\ p(H_i)}{\sum_i p(E \mid H_i)\ p(H_i)} \tag{3.5}$$

where

"H_i" is the i^{th} member of a set of mutually exclusive hypotheses;

"E" represents incoming evidence;

"$p(H_i)$" is the probability of H_i being true prior to the receipt of evidence E;

"$p(E|H_i)$" is the probability of obtaining the data E, if H_i is accepted

"$p(H_i|E)$" is the probability of H_i being true <u>after</u> the receipt of evidence E.

To illustrate: let the number of balls in the selector's urn be three.[3] Then the number of exclusive and exhaustive possible hypotheses H_1 is four:[4]

H_1: all three balls are white

H_2: exactly one ball is red

H_3: exactly one ball is white

H_4: all three balls are red

Suppose the selector is fully indifferent as between these four hypotheses, believing each to be equiprobable. Then

$$p(H_1) = p(H_2) = p(H_3) = p(H_4) = 0.25$$

and the amount of information $\bar{H}_0$ that awaits him on learning the true colour-mixture of balls in the urn is

$$\bar{H}_0 = -\sum_{1}^{4} p_i \log_2 p_i$$

$$= 2.000 \text{ bits.}$$

Now he selects one ball from the urn, and notes that it is red. This is his evidence E_1, and may be evaluated by (3.5) as follows:

$$p(E_1|H_1) = 0.000$$
$$p(E_1|H_2) = 0.333$$
$$p(E_1|H_3) - 0.667$$
$$p(E_1|H_4) = 1.000$$

Therefore, although a priori the selector would be indifferent as between the four hypotheses about the true state S_j of the environment, a posteriori, having regard for E_1, he can no longer believe them equally probable, and his probability distribution over all possible states of nature becomes, by (3.5):

$$p(H_1|E_1) = 0.000$$
$$p(H_2|E_1) = 0.166$$
$$p(H_3|E_1) = 0.334$$
$$p(H_4|E_1) = 0.500$$

Now the amount of information $\bar{H}_1$ present in the urn is

$$\bar{H}_1 = -(0.166 \log_2 0.166 + 0.334 \log_2 0.334 + 0.500 \log_2 0.500)$$
$$= 1.458 \text{ bits.}$$

Therefore, by taking account of his experience the selector has gained ($\bar{H}_0 - \bar{H}_1 = 2.000 - 1.458 =$) 0.542 bits of information; and as, due to his experience, his probability distribution over all possible states of the world has altered, he is said to have learned.

I repeat, feedback is a method of controlling a system by reinserting into it the results of its past performance. If these results are merely used as numerical data for the criticism of the system and its

> regulation, we have the simple feedback of the control engineers. If, however, the information which proceeds backward from the performance is able to change the general method and pattern of performance, we have a process which may well be called learning.[5]

Note, too, that his learning results in constraints upon the environment. After E_1, the maximum amount of information that could logically remain in the urn is $\log_2 3 = 1.584$ bits. Therefore, the amount of redundancy now present is

$$R_1 = 1.000 - 1.458/1.584$$

$$= 1.000 - 0.920$$

$$= 0.08$$

or 8 *per cent*.

Suppose the selector now draws a second ball from the urn (without replacing the first), which is also *red*. Prior to this second datum E_2 his information stock was as calculated *a posteriori* E_1; now

$$p(E_2|H_1) = 0.000$$

$$p(E_2|H_2) = 0.000$$

$$p(E_2|H_3) = 0.500$$

$$p(E_2|H_4) = 1.000$$

and a further application of (3.5) gives his new *a posteriori* probability distribution of states, *viz*.

$$p(H_1|E_2) = 0.000$$

$$p(H_2|E_2) = 0.000$$

$$p(H_3|E_2) = 0.250$$

$$p(H_4|E_2) = 0.750$$

The amount of information $\bar{H}_2$ now left in the urn is

$$\bar{H}_2 = - 0.250 \log_2 0.250 - 0.750 \log_2 0.750$$
$$= 0.811 \text{ bits}$$

so that the selector gained a further ($\bar{H}_1 - \bar{H}_2 = 1.458 - 0.811 =$) 0.647 bits from E_2. And as the maximum amount of information that could now remain in the urn is $\log_2 2 = 1$ bit, the redundancy present is

$$R_2 = 1.000 - 0.811/1.000$$
$$= 0.189$$

or about 19 per cent.

The learning process just illustrated is characteristic of how learning proceeds both in the individual and in the aggregate, social senses. Learning informs choice, thereby facilitating control. Through a process of experience, the controller learns that some S_j's in his environment are more likely of occurrence than others; through the special, systematic learning procedure called "scientific method" he discovers constraints upon his environment which are so dependable as to merit the designation, Scientific Laws. This process enables him at intervals to re-partition his environment, to restructure his models, to amend his beliefs--to improve his control.

In a narrower, technical sense, the process of Bayesian inference offers a powerful alternative to the techniques of traditional statistical inference. These latter techniques are inadequate to cope with the non-stationarity (i.e., instability) of socio-economic phenomena; and they require intolerably long runs of data to give reliable estimates. As regards the first difficulty, Cherry writes:[6]

> Bayes put forward an axiom, in addition: If there are no prior data, then all hypotheses are to be assumed equally likely. That is $p(H_i) = 1/n$. The point about this axiom which really matters, as regards the practical use of this inverse probability method . . . is that the results of applying the theorem to successive events, and the resulting hypothesis probabilities, are not very "sensitive" to the original probabilities $p(H_i)$ and Bayes axiom is as useful an assumption as any. The practical unimportance of the original probabilities has been stressed by I. J. Good.[7]

As regards the second difficulty, Wald, whose Sequential Analysis is distinctly Bayesian, says:[8]

> Sequential analysis is a method of statistical inference whose characteristic feature is that the number of observations required by the procedure is not determined in advance of the experiment. The decision to terminate the experiment depends, at each stage, on the results of the observations previously made. A merit of the sequential method, as applied to testing statistical hypotheses, is that test procedures can be constructed which require, on the average, a substantially smaller number of observations than equally reliable test procedures based on a predetermined number of observations.
>
> . . . The sequential probability ratio test frequently results in a saving of about 50 per cent in the number of observations over the most efficient procedure based on a fixed number of observations.

Traditional statistical inference is dominated by the procedure of confidence interval estimates. The statistician, told in advance how reliable an estimate of some particular parameter must be (e.g., 80 *per cent*, 90 *per cent*, etc.), and given certain assumptions about the variability of the phenomenon he is investigating, then specifies a sample size which, only after the entire sample has been examined, will permit an inference to be drawn ("your parameter lies in the interval 16.32 to 28.01, with a probability of 90 *per cent*"). The entire procedure is static. It permits no use of knowledge gained *during* the act of sampling--no learning. To preserve its logical consistency it must fall back on rather fussy, sometimes ludicrous rules.

> Someone comes to a statistician and says, "I have inspected 831 of these vacuum tubes, and 17 of them are defective. What can I conclude?" The typical frequentist inquires, "Did you plan to inspect 831 of these? Or did you plan to inspect until you saw 17 defective tubes?" The engineer asks, "What difference does it make?" "Oh," says the frequentist, "It makes all the difference in the world. The probability of drawing 17 defectives in a sample of 831 when the true frequency of defectives is p is
>
> $$\binom{831}{17} p^{17}(1 - p)^{814}$$
>
> But, if you had decided to look until you accumulated a collection of 17 defectives, the probability is
>
> $$\binom{830}{16} p^{17}(1 - p)^{814}$$
>
> These two probabilities are not even approximately equal; the first exceeds the second by a factor of 831/17, or almost 50." When the engineer confesses, "To be honest with you, I quit when I got tired," or "I quit when the boss came along and said, 'Enough statistics--we've got to get the product out'," the frequentist statistician wonders what to do. If only he had been consulted before the sample was drawn! Suppose, however, the engineer says, "It was like this. My boss told me he didn't think there were even three defectives per thousand and I sampled until I had enough to make him listen to me." The statistician is shocked. Perhaps he gives way to indignation: "You cheater, you perverter of data, you stopped at your own pleasure, optionally, when you thought you were ahead. That's just like erasing figures from a laboratory notebook, or worse."[9]

The modern, sequential methods, by contrast, are dynamic. They permit of learning. They enable a controller to establish hypotheses about his environment and amend them as necessary, without the long runs of data that the instability of socio-economic phenomena--socio-economic "learning" if you will--renders meaningless. They assimilate such instability. They prescribe acts of choice in the very process of describing the environment to the controller in a continuous stream. They facilitate control.

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[1]From _Through the Looking Glass_; quoted by Colin Cherry in _On Human Communication_ (New York: M.I.T. Press and John Wiley & Sons, Inc.; Science Editions, 1961), p. 167.

[2]See Claude E. Shannon and Warren Weaver, _The Mathematical Theory of Communication_ (Urbana: The University of Illinois Press, 1959).

[3]This reduction from the original number, ten, is made just to facilitate computation.

[4]This assumes that the balls have no individual identity, so that if one ball is red and two white, it does not matter which is the red one. If the balls _were_ individually identified, then _eight_ distinct hypotheses would be necessary to partition the possibility-space, _viz_.

H_1: ball one white
ball two white
ball three white

H_2: ball one white
ball two white
ball three red

H_3: ball one white
ball two red
ball three white

H_4: ball one white
ball two red
ball three red

H_5: ball one red
ball two white
ball three white

H_6: ball one red
ball two white
ball three red

H_7: ball one red
ball two red
ball three white

H_8: ball one red
ball two red
ball three red

This is a good illustration of two alternative partitions for the same situation. The "correct" partition would depend on the partitioner's aims (i.e., what he wished to achieve with his model), on which partition effected the greatest reduction of environmental "noise," on how much information the partitioner himself commanded, and on the cost of each alternative.

[5]Norbert Wiener, The Human Use of Human Beings: Cybernetics and Society (2nd ed., rev.; New York: Doubleday & Company, Inc., 1954) p. 61.

[6]Cherry, op. cit., p. 63

[7]At this point, Cherry cites the reference I.J. Good, Probability and the Weighing of Evidence (London: Charles Griffin & Co. Ltd., 1950). Cf. I. J. Good, The Estimation of Probabilities, An Essay on Modern Bayesian Methods (Research Monograph No. 30; Cambridge: The M.I.T. Press, 1965). Cf. also L. J. Savage et al, The Foundations of Statistical Inference (London: Methuen & Co. Ltd., 1962), where the Laplacian axiom of Bayes is replaced by the decision-maker's subjective judgement.

[8]Abraham Wald, Sequential Analysis (New York: John Wiley & Sons, Inc., 1947), p. 1. See also ibid., pp. 196 ff.

[9]Leonard J. Savage, "Bayesian Statistics," in Robert E. Machol and Paul Gray (eds.) Recent Developments in Information and Decision Processes (New York: The Macmillan Company, 1962), p. 179.

CHAPTER IV

THE ECONOMY AS A BLACK BOX

1. Statistical Determinism.

In Chapter I a black box was defined, following Dantzig, as "any system whose detailed internal nature one willfully ignores." Recall that Dantzig goes on:

> An activity is thought of as a kind of "black box" into which flow tangible inputs, such as men, material, and equipment, and out of which may flow the products of manufacturers, or the trained crews of the military. What happens to the inputs inside the "box" is the concern of the engineer or the educator; to the programmer, only the rates of flow into and out of the activity are of interest.[1]

The operation of a black box may be strictly determined, or it may be stochastic. Black boxes which operate deterministically are common in the physics laboratory. Wiener probably had such a model in mind when he wrote:

> I shall understand by a black box a piece of apparatus, such as four-terminal networks with two input and two output terminals, which performs a definite operation on the present and past of the input potential, but for which we do not necessarily have any information of the structure by which this operation is performed.[2]

By contrast, a socio-economic system, viewed as a black box, operates stochastically. The relationship between any input state S_i and an output state S_j of the system is not strictly determined, but probabilistic. The probability that state S_j will succeed state S_i is designated p_{ij}, and is called a transition probability.

Now at first glance a stochastic system would appear the very antithesis of a determined system. Such, however, is not the case. A strictly deterministic system is merely a special case of the stochastic system, where $p_{ij} = 1.$ or $p_{ij} = 0.$ for all i, j. Moreover, the probabilism of a stochastic system is hardly at odds with determinism generally, and represents indeed a paradigm of <u>statistical determinism</u>.

Examples of statistical determinism abound in the real world. A modern life insurance company does not know which individuals among its policyholders will die during the coming year; but it can forecast accurately the total number of deaths that will occur, on the knowledge that for each particular policyholder the transition probability of proceeding from the state S_i of being alive this year to the state S_j of death next year is some definite p_{ij}. The toss of a fair coin represents another illustration of statistical determinism: no one can say for sure how the coin will land on any single toss, yet accurate predictions of aggregate coin tossing behaviour are commonplace.

An economic system as a black box is a stochastic system, and its behaviour over time is an instance of statistical determinism.

2. Input-Output Analysis.

"The input-output method," writes its chief author, Wassily Leontief,[3]

> is an adaptation of the neo-classical theory of general equilibrium to the empirical study of the quantitative interdependence between interrelated economic activities. It was originally developed to analyze and to measure the connections between the various producing and consuming sectors within a national economy, but has also been applied, on the one hand, in the study of smaller economic systems such as a metropolitan area or even of a large integrated individual enterprise and, on the other hand, to the analysis of international economic relationships.

Anyone familiar with the notion of Walrasian equilibrium will readily grasp the idea of Leontief's system from a hypothetical interindustry transactions matrix like the following:

An Interindustry Transactions Matrix
(All data are in dollars per period)

Industry Producing \ Industry Consuming	Corn	Cloth	Shoes	Bill of Goods	Activity Level = Total Output
Corn	20	60	40	80	200
Cloth	80	60	80	80	300
Shoes	40	60	120	180	400
Households	60	120	160	340	
Total Inputs	200	300	400		900

For example, the Corn Industry, in producing $200. worth of corn per period (of which $80. worth enters into national income Y, and the remaining $120. worth is for intermediate use), absorbs $20. worth of its own output, $80. worth of the Cloth Industry's output, and $40. worth of the Shoe Industry's output; and $60. worth of factors' services besides. For each dollar's worth of total output, the Corn Industry requires (20/200 =) 0.1 dollar's worth of input from itself, (80/200 =) 0.4 dollar's worth of input from the Cloth Industry, and (40/200 =) 0.2 dollar's worth from the Shoe Industry. This latter way of expressing interindustry flows results from the transformation of a transactions matrix into a <u>coefficient matrix</u>, the coefficients just reckoned being 0.1, 0.4, and 0.2. The complete coefficient matrix corresponding to the preceding transactions matrix is:

Coefficient Matrix

	Corn	Cloth	Shoes
Corn	0.1	0.2	0.1
Cloth	0.4	0.2	0.2
Shoes	0.2	0.2	0.3

In general terms, where x_1, x_2, and x_3 are the activity levels respectively of producing corn, cloth, and shoes, and the coefficients of production appearing in the coefficient matrix are designated a_{ij}, where the subscript i identifies the producing sector and the subscript j identifies the consuming sector, y_i being the bill of goods absorbed from the i^{th} sector, the input-output model may be represented by the set of linear equations

$$a_{11}x_1 + a_{12}x_2 + a_{13}x_3 + y_1 = x_1$$

$$a_{21}x_1 + a_{22}x_2 + a_{23}x_3 + y_2 = x_2$$

$$a_{31}x_1 + a_{32}x_2 + a_{33}x_3 + y_3 = x_3$$

Thus a_{32} represents the rate at which the production of shoes (x_3) is absorbed by the cloth industry in outputting cloth (x_2); $a_{11} = 0.1$, $a_{12} = 0.2$, $a_{21} = 0.4$, and so forth.

Input-output analysis furnishes a method for reckoning what activity levels would be required of each sector in an economy to produce a specified bill of goods; and much else. But these further details are passed over here, as ingermane to the present discussion.

An input-output model of some economic system may be regarded as a black box.

3. Markov Chain Transforms.

One species of statistical determinism is the Markov chain process. Kemeny *et al* say[4]

> that we may think of a Markov chain as a process that moves successively through a set of states $s_1, s_2, \ldots, s_r \ldots$. Given that it is in state s_i, it moves on the next step to state s_j with probability p_{ij}. These probabilities can be exhibited in the form of a *transition matrix* P:
>
> $$\begin{array}{c} \\ s_1 \\ s_2 \\ \cdot \\ s_r \end{array} \begin{array}{c} s_1 \quad s_2 \ \cdots \ s_r \\ \begin{pmatrix} p_{11} & p_{12} & \cdots & p_{1r} \\ p_{21} & p_{22} & \cdots & p_{2r} \\ \cdot & \cdot & \cdots & \cdot \\ p_{r1} & p_{r2} & \cdots & p_{rr} \end{pmatrix} \end{array} = P$$
>
> The entries of P are non-negative and the sum of the components in any given row is 1. A vector with non-negative components having sum 1 is called a *probability vector*. Therefore, each row of P is a probability vector.

Howard gives the following illustration:[5]

> A graphic example of a Markov process is represented by a frog in a lily pond. As time goes by, the frog jumps from one lily pad to another according to his whim of the moment. The state of the system is the number of the pad currently occupied by the frog; the state transition is of course his leap. If the number of lily pads is finite, then we have a finite-state process.

Let A be the (n x n) coefficient matrix for a given economic system with input-output coefficients a_{ij}. Then A may be transformed into the (n x n) transition matrix P of a Markov chain process with transition probabilities p_{ij}, by the following rule:

$$p_{ij} = \frac{a_{ij}}{\sum_j a_{ij}} \qquad (i = 1, 2, \ldots, n) \tag{4.1}$$

Recall the economic system presented in Chapter II, sec. 1, possessing n distinct states which correspond to the n distinct industries (or outputs) of the system. At a specified time t, each unit of money in the system will reside in a particular state. For example, at midnight each Sunday, each dollar in the system will be in the possession of some firm or individual belonging to one and only one system-state. Each representative dollar thus traces out a path about the system; and the set of all possible such paths may be described as a Markov process by a transition matrix P.

One might expect that the pattern produced by such interindustry money flows somehow reflects the pattern of interindustry economic activity imposed upon the subject economy by its technology, institutions, and so on. This is indeed the case: for these interindustry influences are described by the economy's coefficient matrix A associated by the expression (4.1) to the transition matrix P.

Thus does every input-output model entail a Markov process.

4. A Proposed Application: Measurement of Technological Change.

To anyone familiar with the contemporary literature of growth theory, a reminder about the technological "progress" controversy is like carrying potash to Saskatchewan. Briefly, the controversy arises because empirical production function analyses have shown that by far the largest influence in generating per capita economic growth is neither labour nor capital, but a "residual" factor which, in an unfortunately accurate sense, is but a measure of economists' ignorance.[6] It is thought that "technological change" is the main ingredient of this residual factor. But no satisfactory measure of technology's influence upon the productive process has yet emerged, so that empirical analysis of the important residual cause in the production of economic wealth is quite blocked.

Let A_t be the ($n \times n$) coefficient matrix of an economic system E at time t (or during some interval t), where a_{ij} is (as usual) the amount of the i^{th} industry's output used by the j^{th} industry to produce each unit of j^{th} industry's output, $(i, j = 1, 2, \ldots, n)$.

Suppose that a unit of money—one dollar, say—enters E through industry i. It will, in the normal course of E's interindustry activity, get passed on to industry j with some particular probability p_{ij}. For any particular i (=1*), clearly

$$\sum_j p_{i*j} = 1.$$

and the particular value of each p_{ij} over the j columns in the i^{th} row of A_t will depend on—will indeed be uniquely associated with—the corresponding values of a_{ij}. This unique association is simply the association of the matrix P of transition probabilities for a Markov process, to its corresponding coefficient matrix A derived from a standard input-output model.

Now the particular interindustry activity-pattern that characterizes E's technology—E's technological "fingerprint," so to speak—may be thought of as a pattern of interindustry communication, with money (or money's worth) employed as the medium of communication. The system at any time $\underline{t}$ possesses a definite, measurable quantity of information H_t, which is a function of its technological fingerprint, derived from A_t.

The essence of an economy's technology is a particular pattern of constraints imposed upon economic activity-as-communication. Organization--in this case organization dictated by the requirements of a particular technology--ipso facto entails constraint, of which a quantitative measure is redundancy R.

If E utterly lacked organization--were completely chaotic--then one representative dollar entering E at industry $\underline{i}$ would proceed with equal probability to any other industry in E, and for any particular $\underline{i}$ it would be the case that

$$p_{i1} = p_{i2} = \cdot \cdot \cdot = p_{ij} = \cdot \cdot \cdot = p_{in}$$

It may be shown by the differential Calculus that, in these circumstances, the total amount of information embodied in E would be the maximum amount possible (i.e., $H = H_{max}$);[7] and, therefore, there would be zero redundancy--absolutely no organized pattern--in E. Such an utterly chaotic economy, of course, suggests a very low level of technology. By contrast a technologically sophisticated economy is apt to go hand in hand with a high degree of organization (i.e., constraint), and would, therefore, score something more than zero--something close to unity, perhaps, if it were organized to the point of rigidity--on a measurement of its redundancy.

In any case, the information content for E would be

$$H = -\sum_{ij}\sum p_{ij} \log_2 p_{ij}$$

and its redundancy

$$R = 1.0 - H/H_{max}$$

At any particular time _t_, E would possess a particular measure R_t of technology, which could be fed into the standard production function along with the usual data about capital- and employment-levels, thereby accounting for some of the variance that now passes for "residual." Exactly how much of the present residual this proposed measure of technology would explain remains an empirical question. But given the availability of today's high-speed electronic computers, plus the strategic importance of technology-measurement for economic theory and economic policy, this empirical question need not long beg reply.

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[1]Dantzig, _op. cit._, p. 32.

[2]Wiener, _Cybernetics_ (2nd ed.), p. xi, _fn._ 5, Chap. III.

[3]Wassily Leontief, _Input-Output Economics_ (New York: Oxford University Press, 1966), p. 134. _Cf._ Hollis B. Chenery and Paul G. Clark, _Interindustry Economics_ (New York: John Wiley & Sons, Inc., 1965). A lucid, detailed, and insightful exposition appears in A. Charnes and W. W. Cooper, _Management Models and Industrial Applications of Linear Programming_ (New York: John Wiley & Sons, Inc., 1964), Vol. I, pp. 72 ff., from which the numerical illustration used in the present paper is drawn.

[4]John G. Kemeny _et al_, _Finite Mathematical Structures_ (Englewood Cliffs: Prentice-Hall Inc., 1960). pp. 384-5.

[5]Ronald A. Howard, _Dynamic Programming and Markov Processes_ (Cambridge: The M.I.T. Press, 1964), p. 3.

[6]Evsey D. Domar, "The Capital-Output Ratio in the United States: Its Variation and Stability," D. C. Hague, (ed.), _The Theory of Capital_ (London: Macmillan & Co. Ltd., 1961), p. 117:

If we join the company of several recent investigators (Abramovitz, Kendrick and Solow) who have found, each in his own way, that by far the largest fraction of the per capita rate of growth of income in the United States should be attributed to technological progress rather than to capital accumulation . . .

See also Lester B. Lave, Technological Change: Its Conception and Measurement (Englewood Cliffs: Prentice-Hall, Inc., 1966), pp. 5-6:

Assuming a Cobb-Douglas production function and no complications, one might say that output will rise 1 per cent for every 3 per cent capital increases, holding labor constant. This ratio is simultaneously an estimate of the elasticity of output with respect to capital and the share of capital in income. Using this ratio, a preliminary explanation of the observed increase in per capita output is possible.

Between 1909 and 1949, employed capital per man-hour in the private, nonfarm sector of the United States economy rose by 31.5 per cent. By the preceding argument, this increase in capital should have given rise to an increase in per capita output of about 10 per cent. The data showed that output per man-hour in this same sector and over this same period rose not by 10 per cent, but rather by 104.6 per cent. What caused the 90 per cent increase in productivity that is unexplained by the increase in capital per worker?

With a magnificent wave of his hand, Solow named this 90 per cent technological change.

. . . Domar (1961) thought that a more appropriate name was "the residual," that is, that part of increased output per man which is left over after increases in capital per man are accounted for.

The "residual" is clearly defined: the increase in productivity not explained by increases in capital per man.

. . . The importance of this force is easily seen in the fact that it represents approximately 90 per cent of the increase in productivity.

[7]Cf. Shannon and Weaver, op. cit., p. 21.

CHAPTER V

CYBERNETICS AND ECONOMICS: AN AGENDA

1. Redundancy and Optimum Social Organization.

The information-theoretic measure of redundancy proposed in Chapter IV may furnish useful insight into some normative questions of social organization. For example, the competitive structure of industry is often a controversial topic, whose controversy spills far beyond the economist's purview. Yet specific answers to practical questions arising from this controversy--Should the merger of ABC Co. with XYZ Co. be allowed or prohibited? Should anti-combines legislation be strengthened, or weakened, or left alone? Should the railways be nationalized?--inevitably go ignored for lack of a "handle" to the general problem they represent, or else receive their answers from rather unclear dogmas about freedom or slavery, or only partly relevant pronouncements from the frozen voices of Adam Smith or Karl Marx. By contrast, a measure of technology such as the index of redundancy R would yield a production function like

$$Y = aK^{\alpha} L^{\beta} R^{\gamma}$$

which could importantly inform judgement about industrial structure. For example, if

$$\gamma > 0$$

then the constraints added to (or subtracted from) industrial structure by (say) the recent merger movement could *prima facie* be judged as beneficial for economic growth; while if

$\gamma < 0$

there would be a prima facie case for believing these constraints to inhibit economic growth. Better still, proposals for legislative change or economic action to alter industrial structure could be evaluated by examining their probable impact upon the relevant input-output table, translating this impact into information-theoretic terms, deriving a measure of associated changes in R, and simulating such changes in a production function model. Admittedly, such a procedure could hardly claim infallibility. But it would compel articulation of operationally meaningful criteria upon which policy judgements were made, thereby (a) enabling erroneous judgements to be recognized as such at an early date, and appropriately amended, and (b) building up a fund of successively more sophisticated and reliable criteria for future normative judgements about social organization. It would facilitate social learning and concomitant social control.

The mention of simulation procedures brings the topic of electronic computers quite naturally into the present discussion. The advent of computers, so closely (and often mistakenly) associated with cybernetics *per se*, opens rich possibilities for synthetic experimentation with socio-economic systems. Such possiblities go very far towards remedying the social scientists' legitimate complaint about lack of opportunity for (ethically acceptable) controlled experimentation.[1] But they are not without a certain potential for mischief:

> The very power of the computer to simulate complex systems by very high-speed arithmetic may prevent search for those simplifed formulations which are the essence of progress in theory. I have an uneasy feeling, for instance, that if the computer had been around at the time of Copernicus, nobody would have ever bothered with him, because the

computers could have handled the Ptolemaic epicycles with perfect ease.[2]

2. Double-Entry Theory and Economic Analysis.

The theory of double-entry accounting, which forms the basis for practically all commercial accountancy, offers at once a method of obtaining an information-theoretic measure of technology's contribution to output at the level of the firm, and a technique of integrating the collection of economic data at the level of the national economy.

Double-entry accounting theory is based on the simple identity

$$A - L \equiv W \tag{5.1}$$

where W is the net worth of an enterprise,

A is the value of all assets owned by the enterprise, and

L is the value of all liabilities owed by the enterprise (to outsiders).

The identity (5.1) corresponds quite fully with common sense, and requires little comment. By adding L to each side a further identity results, *viz*.

$$A \equiv L + W \tag{5.2}$$

which furnishes the operational basis for conventional accounting procedure. On (5.2) all the resources A of an enterprise may be viewed (and recorded) from two distinct aspects: (1) their value as property (e.g., their cost), and (2) the claims against this value laid respectively by the enterprise's creditors (L) and owners (W). (There is no non-negativity constraint upon any of A, L, or W). Every single transaction of the enterprise affecting any of A, L, or W, must be recorded exactly twice: (1) as it affects the value of property owned by the enterprise, which is to say as it affects the left hand side of the identity (5.2); and (2) as it affects the claims against the enterprise's resources by either its creditors or its owners, i.e., as it affects the right hand side of (5.2). An act of recording is called an <u>entry</u> (presumably into some book of account), hence the name "double-entry" of the general procedure. Any entry which increases the value of the left hand side of (5.2) is called a <u>debit</u>, as is any entry which diminishes the value of the right hand side of (5.2). Similarly, any entry which increases the value of the right hand side of (5.2) is called a <u>credit</u>; and so is any entry which diminishes the value of the left hand side of (5.2). The act of entering either a debit or a credit may be designated by the verbs "to debit" and "to credit," respectively.

Now at the level of the firm, it is possible to construct a matrix M whose rows and columns consist equally in a complete listing of all the firm's accounts. Let the debit aspect of each account be designated by its row address, and the credit aspect by its column address. In this way, every economic transaction capable of being recorded by conventional double-entry technique would occasion entry into an unique element m_{ij} of M, where "entry" means the addition to or subtraction from the prior value of m_{ij}, of a sum representing the value of whatever transaction is being recorded.

In this way the act of record keeping would simultaneously create an input-output model of the firm's operations. This model could (a) form the basis for management planning, after the usual techniques of input-output analysis, which are based on a linear algebra procedure known as "matrix inversion"; and (b) be used to obtain an information theoretic measure of the firm's technology, and more particularly the impact of that technology on the firm's production function.

At the macroeconomic level, the same results could be expedited by bringing the theory of double-entry accounting explicitly to bear on the recording of economic data. Most of a national input-output table could then be assembled from records which already exist and already are submitted routinely to national authorities, e.g., the financial statements which accompany the annual income tax returns of all proprietorships, partnerships, and private and public corporations. Moreover, the double-entry procedure described here would obviate the need of constructing production functions on an industry-by-industry basis, piecemeal by the often arduous and always obverse (vide Ch. I, sec. 3) technique of multiple regression analysis, this latter procedure being an important, practical bottleneck in present input-output table construction. Finally, an arrangement of the kind recommended here would remove the need of "error" entries in the national accounts. Piecemeal assembly would give way to the production of the national accounts as a natural outcome of an integrated system of record keeping, based upon the accounting identity (5.2).

3. Money, Communication, and Control.

Control of an economic system requires intra-system communication, which in turn requires the existence of some medium or channel through which communication ("message") may proceed. In a small, two-person zero-sum Game model, messages are sent and received simply by each player watching what the other player does. In the more sophisticated two-person non-zero-sum Game which is iterated over time, communication between the two players still can proceed by the simple acts of choice which are performed by each player in the presence of the other. But in a large, highly complex system, such as a real national economy, the acts of individual choice whose aggregate effect jerks the system along its time-path perforce cannot be communicated in any simple physical way. Some other medium of communication is necessary. In a modern economy, money generally functions as such a channel.

A further complication arises in moving from a simple two-player economy to a complex many player economy: in a Robinson Crusoe type system, acts of choice which entail resource deployment may almost universally be executed, and hence given material effect, by the decision maker himself. To choose and to do are the same. By contrast, in a large system, while undoubtedly there is for each player a domain of choice where choice and execution are one (e.g., taking out the garbage, repairing the storm windows, digging a backyard garden, cataloging the books in one's personal library), on the whole the act of choosing is separate from its material, asset-deployment consequences. Hence the power to choose is circumscribed by the power to deploy real assets. And this latter power, by social convention, resides in money. Choice backed up by money is effective choice; it is choice which is reinforced by the chooser's command over a sufficient portion of socially-acknowledged channel capacity.

Given the information content of an economy, there is some particular size of channel capacity which is optimum for the communication needs of that economy. Cherry writes:[3]

> Depending upon the type of noise, and the type of channel, redundancy is best added in different ways; but the whole subject is very difficult. Shannon has indicated a general technique of <u>coding</u> messages in advantageous ways, for combatting noise, that is more subtle than mere repetition of every transmitted sign.

Too much channel capacity encourages the transmission of noise and other sources of confusion; too little channel capacity inhibits the legitimate processes of economic activity. Consider, for example, a period of vigorous growth in technology. During such a period, the number of strategies open to important players (or groups of players) in the economic Game--the set of acts over which they may make their selections--is being increased. Now if there could simultaneously with the appearance of these new strategy-options occur the "evaporation" of an exactly equal number (or in any event a number of strategies possessing the same amount of information), as through sudden obsolescence, or instantaneous, complete switching from old techniques to new, then technological development would impose no additional information load on the economy. But this is evidently not possible, so that the new options, reflecting the new techniques, require enlargement of the economy's channel capacity to deal with the additional information load. This enlargement can come by the injection of new money into the system, though it is further important to ensure that such new money be made available to the very decision makers whose strategy sets are growing on account of (the postulated) technological development. Political authorities who are concerned to promote economic growth must take care that innovators get adequate channel capacity for transmitting their messages (and thereby translating their acts of choice into facts of resource-deployment). For the individual innovator, the choice is between receiving such enlarged capacity, dropping his innovation, or deliberately closing off "old" strategy-options (i.e., "diverting resources") so that he may attend to the new.

Monetary inflation is an example of too much channel capacity. *In extremis* it may produce such a high noise-to-message ratio as to provoke general abandonment of the money-channel, producing either a shift to some other kind of channel (e.g., cigarettes), or, if no alternative channel proves sufficiently large to enable adequate communication in the system, collapse of the system.

Where extra channel capacity is required, the provision of fresh money is, of course, just one way of tackling the problem. More efficient use of existing channel capacity may be possible through the application of the coding theorems of information theory,[4] and this provides an alternative to simple channel enlargement. Another alternative is to *force* (i.e., politically, legislatively, administratively) a contraction of the strategy-set's size, thus reducing the total amount of information required to be transmitted. Forceful reductions like this are common in wartime, when fear of inflationary developments in an already fully-employed economy inhibits the creation of new money. This suppression of controllers' variety--through shunting of channel capacity to war-effort messages--is a meaningful measure of the economic harm that war, or even war-like preparations that fall short of actual combat, inflict upon the belligerents' economies.

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[1]See, for example, O. Helmer, *Social Technology* (Santa Monica: RAND

Corporation, Publication No. P-3063, February, 1965): "A reappraisal of methodology in the social sciences with specific proposals for modifications of traditional procedures." (Abstract.); N.L. Gilbreath et al., Construction of a Simulation Process for Initial Psychiatric Interviewing (Santa Monica: RAND Corporation, Publication No. P-2933, June, 1964): "A description of the construction of a simulation of an initial psychiatric interview, regarded as an example of an adaptive, multi-stage decision process." (Abstract.); John T. Gullshorn and Jeanne E Gullshorn, "A Computer Model of Elementary Social Behavior," Feigenbaum and Feldman, op. cit.; Harold Guetskow, (ed.), Simulation in Social Science (Englewood Cliffs: Prentice-Hall, Inc., 1962); K. D. Tocher, The Art of Simulation (London: The English Universities Press Ltd., 1963).

[2]Boulding, "The Economics of Knowledge . . . ," p. 10.

[3]Cherry, op. cit., pp. 185-6. Shannon's work on coding probably has important economic interpretations in this regard. But it is, unfortunately, not directly relevant to the object of the present paper.

[4]Please see note 3, immediately above. Efficient encoding would take the form of constraints imposed upon the use of money.

SUMMARY

The need to control the environment--originating perhaps from the survival-drive of the species homo sapiens--is the ultimate rationale of science, and therefore of the social sciences, including economics. "Control" in this context is not intended in any oppressive sense, but rather in the sense commonly associated with "predictive power" in scientific models: a prediction from such a model being merely a deductive procedure for projecting forward in time some aspect of the modelled phenomenon which has been described by the model, thereby enabling changes in the modelled phenomenon to be detected (by the modeller)conveniently. (In particular, these changes are detected when actual events diverge significantly from prior, deduced expectation). Far from possessing insidious overtones, "control" is thus simply a procedure for getting a target process to "tell on itself" over time, so that hypotheses about the process may be continuously amended as necessary, left alone where warranted, and stand therefore as sound bases for sequential acts of (rational) choice. These latter acts are the stuff of economic decision-making.

Control-oriented model construction in the social sciences has until recently been inhibited by the apparent instability of socio-economic phenomena. Society does not behave like a determinate machine, yet the construction of models of socio-economic behaviour, much influenced by the practical success of Newtonian (determinate) physics, has until recently

proceeded precisely from assumptions of machine-like determinism. This approach is wrong in principle, even where it is occasionally empirically successful (and rare indeed are the practical achievements of traditional economics, especially by contrast with those of classical physics). Even probabilistic treatment of socio-economic phenomena is compromised by the demand of traditional statistical techniques for intolerably long runs of data. This leads to a dilemma: because of the inherently stochastic character of socio-economic phenomena, deterministic models cannot generally serve the ends of control, and must yield to models which are stochastic; but stochastic models require data-runs which, precisely because of the (statistical) instability of socio-economic phenomena, are too long to be meaningful; therefore the social scientist must choose between modelling deterministically phenomena which he knows are not deterministic but stochastic, or employing stochastic models which _ipso facto_ bar him from the empirical testing without which model-building is merely a sterile exercise.

The dilemma is solved by construing the apparent instability of socio-economic phenomena as a social (i.e., aggregate) learning process. Statistical methods based on Bayes' Theorem neatly assimilate this construction. Moreover, they end the need of fixed sample sizes in statistical estimating procedures, replacing the very notion of (static) statistical estimation with the notion of (dynamic) _sequential_ testing of hypotheses, which hypotheses can legitimately change as often as desired, e.g., following each fresh observation (datum). The viewpoint underlying this procedure is normatively neutral, being silent as to the desirability or otherwise of observed social learning processes; yet it permits the economist *qua* economist to judge whether such learning, to the extent it influences economic output, either facilitates or inhibits achievement of economic objectives.

The foregoing process of sequentially testing hypotheses is important to the question of controlling an economic system because such efforts at control proceed from models which are essentially sets of hypotheses. Effective control demands close correspondence between these models and their client phenomena.

The basic model of a control process takes the form of a species of von Neumann-Morgenstern Game (iterated over time), which is logically identical to the problem of optimally deploying constrained resources. In the most general sense, this Game is between the controller (e.g., a philosopher-king planning his kingdom's economy, the president of an industrial corporation contriving the growth and prosperity of his company, etc.) and his environment, the controller's object being to confine the variety present in the target system's time path of output to a desired subset of its potential variety. The controller's efforts are constrained by the Law of Requisite Variety, which compels the controller to secure adequate variety over his domain of choice, or to constrain environmental variety, or both, in fashioning effective control policies. These techniques of control, as indeed the question of control generally, may conveniently be discussed with the concepts (and concomitant jargon) of Information Theory, the core of theoretical cybernetics.

Some examples of important topics in economics which cybernetics may thus address are: the measurement of technology and of technological change; the problem of optimal institutions, and of industrial structure; the theory of the firm. But such examples are tentative and prefatory, and the systematic application of cybernetics to economics remains an unfinished—in fact a scarcely begun—task.

BIBLIOGRAPHY

Arrow, Kenneth J. "Control in Large Organizations," Management Science, Vol. 10, No. 3 (April, 1964).

_______. Social Choice and Individual Values. Second edition. New York: John Wiley & Sons, Inc., 1951.

Ashby, W. Ross. An Introduction to Cybernetics. New York: John Wiley & Sons, 1963.

Attneave, Fred. Applications of Information Theory to Psychology. New York: Henry Holt and Company, 1959.

Ayer, Alfred Jules. Language, Truth and Logic. New York: Dover Publications, Inc.; no date, but c. 1936.

Baumol, William J. Economic Dynamics. Second edition. New York: The Macmillan Company, 1964.

Beer, Stafford. Cybernetics and Management. New York: John Wiley & Sons, Inc., Science Editions, 1964.

Bellman, Richard. Adaptive Control Processes. Princeton: Princeton University Press, 1960.

Blau, Peter M. Exchange and Power in Social Life. New York: John Wiley & Sons, Inc., 1964.

Boulding, Kenneth E. Conflict and Defense. New York: Harper & Row, 1963.

_______. "The Economics of Knowledge and the Knowledge of Economics," American Economic Review, Vol. LVI, No. 2, (May, 1966).

_______. "Social Sciences," The Great Ideas Today. Chicago: Encyclopaedia Britannica, Inc., 1965.

_______. A Reconstruction of Economics. New York: John Wiley & Sons, Inc., Science Editions, Inc., 1965.

_______. "General Systema Theory - The Skeleton of Science," Management Science, Vol. 2, No. 3, (April, 1953).

Braybrooke, David and Charles E. Lindblom. A Strategy of Decision. Glencoe: The Free Press of Glencoe, 1963.

Buchanan, James M. and Gordon Tullock. The Calculus of Consent. Ann Arbor: The University of Michigan Press, 1962.

Charnes, A. and W. W. Cooper. Management Models and Industrial Applications of Linear Programming. New York: John Wiley & Sons, Inc., 1964 (2 vols.)

Chenery, Hollis B. and Paul G. Clark. Interindustry Economics. New York: John Wiley & Sons, Inc., 1965.

Chernoff, Herman. and Lincoln E. Moses. Elementary Decision Theory. New York: John Wiley & Sons, Inc., 1959.

Cherry, Colin. On Human Communication. New York: M.I.T. Press and John Wiley & Sons, Inc., Science Editions, Inc., 1961.

Clarkson, Geoffrey P. The Theory of Consumer Demand: A Critical Appraisal. Englewood Cliffs: Prentice-Hall, Inc., 1963.

_______. "A Model of Trust Investment Behavior," in Cyert, Richard M. and James G. March. A Behavioral Theory of the Firm. Englewood Cliffs: Prentice-Hall, Inc., 1963.

Cohen, Morris R. and Ernest Nagel. An Introduction to Logic and Scientific Method. New York: Harcourt, Brace and World, Inc., 1934.

Copleston, Frederick. Contemporary Philosophy. Westminster: The Newman Press, 1963.

Dantzig, George B. Linear Programming and Extensions. Princeton: Princeton University Press, 1963.

Deutsch, Karl W. The Nerves of Government: Models of Political Communication and Control. New York: The Free Press of Glencoe, 1963.

_______. "Communication Codes for Organizing Information," Behavioral Science, Vol. 11, No. 1, (January, 1966).

Dillon, John L. and Earl O. Heady. Theories of Choice in Relation to Farmer Decisions. Ames: Iowa State University, Agricultural and Home Economics Experiment Station, Research Bulletin 485, October, 1960.

Domar, Evsey D. "The Capital-Output Ratio in the United States: Its Variation and Stability," Hague, D. C. (ed.) The Theory of Capital. London: Macmillan and Co. Ltd., 1961.

Dorfman, Robert. "Operations Research," American Economic Review, Vol. 50, 1960. pp. 575-623; but especially pp. 584-586, "Information Theory."

Downs, Anthony. "The Returns From Information and Their Diminution," Ulmer, S. Sidney (ed.), Introductory Readings in Political Behavior. Chicago: Rand, McNally & Company, 1962. pp. 316-320.

Durant, Will. The Story of Philosophy. New York: Simon and Schuster, 1953.

Fallon, Carlos. "Principles of Value Analysis," in Falcon, Wm. D. (ed.), Value Analysis Value Engineering. New York: American Management Association, 1964.

Feigenbaum, Edward A. and Julien Feldman (eds.). Computers and Thought. New York: McGraw-Hill Book Company, Inc., 1963.

Feigl, Herbert and Grover Maxwell (eds.). Current Issues in the Philosophy of Science. New York: Holt, Rinehart and Winston, 1961.

Fellner, William. Probability and Profit: A Study of Economic Behavior Along Bayesian Lines. Homewood, Illinois: Richard D. Irwin, Inc., 1965.

Fishburn, Peter C. Decision and Value Theory. New York: John Wiley & Sons, Inc., 1964.

Forrester, Jay W. Industrial Dynamics. New York: The M.I.T. Press, Massachusetts Institute of Technology, and John Wiley & Sons, Inc., 1961.

Frankel, Marvin. "Federal Health Expenditures in a Program Budget," Novick, David (ed.). Program Budgeting. Cambridge: Harvard University Press, 1965.

Gilbreath, N. L., et al. Construction of a Simulation Process for Initial Psychiatric Interviewing. Santa Monica: RAND Corporation. Publication No. P-2933, June 1964.

Good, I. J. The Estimation of Probabilities, An Essay on Modern Bayesian Methods. Cambridge: The M.I.T. Press. Research Monograph No. 30. 1965.

______. Probability and the Weighing of Evidence. London: Charles Griffin & Co. Ltd., 1950.

Guetzkow, Harold (ed.). Simulation in Social Sciences. Englewood Cliffs: Prentice-Hall, Inc., 1962.

Gullahorn, John T. and Gullahorn, Jeanne E. "A Computer Model of Elementary Social Behavior," Feigenbaum and Feldman, op. cit.

Helmer, O. Social Technology. Santa Monica: RAND Corporation, Publication No. P-3063. February, 1965.

Hitch, Charles J. Decision-Making for Defense. Berkeley and Los Angeles: University of California Press, 1965.

______, and Roland N. McKean. The Economics of Defense in the Nuclear Age. Cambridge: Harvard University Press. 1963.

Hofling, Charles K. Textbook of Psychiatry for Medical Practice. Philadelphia and Montreal: J. B. Lippincott Company, 1963.

Hook, Sidney (ed.). Determinism and Freedom in the Age of Modern Science. New York: Collier Books, 1961.

Hospers, John. An Introduction to Philosophical Analysis. Englewood Cliffs: Prentice-Hall, Inc., 1960.

Howard, Ronald A. Dynamic Programming and Markov Processes. Cambridge: The M.I.T. Press, 1964.

Jenny, Hans H. "Operations Research: Its Nature and Scope, With Some Comments Concerning Its Impact on the Smoothing of Cyclical Fluctuations," in Boulding, Kenneth E. and W. Allen Spivey. Linear Programming and the Theory of the Firm. New York: The Macmillan Company, 1961.

Kaldor, Nicholas. "Capital Accumulation and Economic Growth," Hague, D. C. (ed.). The Theory of Capital. London: Macmillan and Co. Ltd., 1961.

Kaplan, Morton A. System and Process in International Politics. New York: John Wiley & Sons, Inc., Science Editions, 1964.

Kemeny, John G., et al. Finite Mathematical Structures. Englewood Cliffs: Prentice-Hall, Inc., 1960.

Kendall, M. G. and A. Stuart. The Advanced Theory of Statistics. London: Charles Griffin & Company Limited, 1961. Vol. 2.

Krupp. Sherman Roy. "Equilibrium Theory in Economics and in Functional Analysis as Types of Explanation," Martindale, Don (ed.). Functionalism in the Social Sciences. The strength and limits of functionalism in Anthropology, Economics, Political Science, and Sociology. Philadelphia: The American Academy of Political and Social Science, 1965.

Kuhn, Alfred. The Study of Society: A Unified Approach. Homewood, Illinois: Richard D. Irwin, Inc., 1963.

Lave, Lester B. Technological Change: Its Conception and Measurement. Englewood Cliffs: Prentice-Hall, Inc., 1966.

Leontief, Wassily. Input-Output Economics. New York: Oxford University Press, 1966.

Lewis, W. Arthur. The Theory of Economic Growth. London: Unwin University Books, 1963.

Lotka, Alfred. Elements of Mathematical Biology. New York: Dover Publications, Inc., 1956. originally published as Elements of Physical Biology, c. 1924.

Luce, R. Duncan and Howard Raiffa. Games and Decisions: Introduction and Critical Survey. New York: John Wiley & Sons, Inc., 1957.

Lusted, Lee B. "Cybernetics and Medicine," Science, Vol. 151, February, 1966. p. 559.

Machol, Robert E. and Paul Gray (eds.). Recent Developments in Information and Decision Processes. New York: The Macmillan Company, 1962.

Marshall, Alfred. Principles of Economics. Eighth edition. London: Macmillan & Co., Ltd., 1961.

Nagel, Ernest. The Structure of Science: Problems in the Logic of Scientific Explanation. New York: Harcourt, Brace & World, Inc., 1961.

_______. "Some Notes in Determinism," in Hook, Sidney (ed.). Determinism and Freedom. New York: New York University Press, 1958.

_______, and James R. Newman. Gödel's Proof. New York: New York University Press, 1960.

Newman, James R. (ed.). The World of Mathematics. New York: Simon and Schuster, 1956. Four volumes.

Oxford Universal Dictionary, The. Third edition, revised. Oxford: The Clarendon Press, 1955.

Quine, Willard Van Orman. Methods of Logic. Revised edition. New York: Henry Holt and Company, Inc., 195.

Raiffa, Howard and Robert Schlaifer. Applied Statistical Decision Theory. Boston: Division of Research, Graduate School of Business Administration, Harvard University, 1961.

Rapoport, Anatol. "What is Information?" Ulmer, S. Sidney, (ed.). Introductory Readings in Political Behavior. Chicago: Rand, McNally & Company, 1962.

_______. Strategy and Conscience. New York: Harper & Row, 1964.

_______, and Albert M. Chammah. Prisoner's Dilemma. Ann Arbor: The University of Michigan Press, 1965.

Reza, Fazlollah, M. An Introduction to Information Theory. New York: McGraw-Hill Book Company, 1961.

Rudner, Richard S. Philosophy of Social Science. Englewood Cliffs: Prentice-Hall, Inc., 1966.

Samuelson, Paul A. Foundations of Economic Analysis. Cambridge: Harvard University Press, 1961.

_______. Economics: An Introductory Analysis. Sixth edition. New York: McGraw-Hill Book Company, 1964.

Savage, Leonard J.,"Bayesian Statistics," in Machol, Robert E. and Gray, Paul, op. cit.

Savage, L. J., et al. The Foundations of Statistical Inference. London: Methuen & Co. Ltd., 1962.

Schumpeter, Joseph A. History of Economic Analysis. New York: Oxford University Press, 1963.

Shannon, Claude E. and Warren Weaver. The Mathematical Theory of Communication. Urbana: The University of Illinois Press, 1959.

Shubik, Martin(ed.). Game Theory and Related Approaches to Social Behavior. New York: John Wiley & Sons, Inc., 1964.

Tocher, K. D. The Art of Simulation. London: The English Universities Press Ltd., 1963.

von Neumann, John and Oskar Morgenstern. Theory of Games & Economic Behavior. New York: John Wiley & Sons, Inc., Science Editions, 1964.

Wald, Abraham. Sequential Analysis. New York: John Wiley & Sons, Inc., 1947.

Ways, Max. "Tomorrow's Management: A More Adventurous Life in a Free-Form Corporation. Fortune. July, 1966.

Whitehead, Alfred North and Bertrand Russell. Principia Mathematica. Cambridge: Cambridge University Press, 1962.

Wiener, Norbert. Cybernetics. New York: The Technology Press and John Wiley & Sons, Inc., 1948.

_______. Cybernetics. Second edition. New York: The M.I.T. Press and John Wiley & Sons, Inc., 1961.

_______. The Human Use of Human Beings: Cybernetics and Society. Second edition. Revised. New York: Doubleday & Company, Inc., 1954.

_______. God & Golem, Inc. Cambridge: The M.I.T. Press, 1966.