# BEYOND OPEN SYSTEM MODELS OF ORGANIZATION

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## ABSTRACT[1]

Since the middle 1960s, the "macro" branch of organizational studies has operated with a set of assumptions commonly referred to as the open system model. No more eloquent, systematic, and widely used statement of this model exists than James D. Thompson's *Organizations in Action*. In this paper, it is argued that the open system model, as illustrated by Thompson's book, does not really satisfy the conditions of an open system. It is further argued that Thompson's model has directed our attention away from organizational dysfunctions at the macro level, and from higher mental functions of human behavior that are relevant to understanding organizations. An

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overarching framework of models is used to begin the development of a new set of assumptions, one that might be referred to as a cultural model of organization, the key elements of which include an emphasis on the use of language and the creation of shared meanings. In this sense, the paper attempts to invent a future for organization theory.

## INTRODUCTION

Inventing a future for organization theory is the intention of this paper. Our aim is not to provide new answers to old questions, but to raise wholly new questions, ones which, more so than the old questions, address problems likely to face organizations over the next decade, and to capitalize on more advanced concepts of human behavior. Invention is always difficult, because it requires both an act of forcible escape from old assumptions grown commonplace and the creation of new and fresh ways of thinking about one's world. Escaping old assumptions is especially hard when they have served us well, when they have provided reliable guidance to our inquiry, and when they have helped us to frame questions that were answerable. Creating fresh viewpoints can be equally hard, because it seems to force us to work outside any structure whatsoever. How can one secure a new vision, a new representation of reality except by some dimly understood and unreliable leap of intuition? In fact, we believe that this fear is ungrounded and that creative insight can be achieved within a structure, provided that the structure is sufficiently open-ended. If that structure or framework also serves to help us become aware of the shortcomings of the present model, then so much the better. In this paper, we propose such a structure for organizing our thought, and use it both to critique the dominant model of organization theory and to frame new questions that will constitute a future for the field.

A few preliminaries are in order before we proceed further. The field of organizational studies is commonly segmented into "micro" and "macro" branches, the former typically, but not universally, being labeled "organizational behavior," and the latter "organization theory" (although it is by no means limited to theory). By whatever labels, the micro branch is ordinarily thought to comprise the study of individual, interpersonal, and intergroup behavior, as in the study of leadership, motivation, and job design, whereas the macro branch concerns organization-wide aspects such as structure, relations with the environment, effects of technology, and so forth. This distinction is by no means clear-cut, as will be obvious from the uncertain placement of studies of organization climate. This paper will concentrate its analysis on the macro branch, however ill-defined, and will refer to it as "organization theory,"



however inappropriate that terminology might seem. The domain of the paper will hopefully become clearer as we proceed. Primarily we wish to defuse any illusion that we address the state of the entire field of organization studies.

Within the subfield of organization theory there has been a remarkable unity of viewpoint and method since the mid-1960s, with a degree of coherence nearly deserving of Thomas Kuhn's label of "paradigm" (Kuhn, 1970). We shall not attempt a systematic summary of the model at this point in the paper, but it would be suggestive to say that the viewpoint has been characterized by asking how an organization could solve the problem of achieving a functional alignment of its goals, structure, technology, and environment in the presence of persistent uncertainty. And its primary method of studying this question has been to conduct systematic comparisons across organizations in hopes of discovering empirical regularities. Many scholars contributed both to the formulation of the model and empirical inquiry under its aegis. But one book stands out as the most systematic statement of the model: James D. Thompson's *Organizations in Action* (1967). Our critique of the field takes this eminent and influential treatise as its prime focus. We admit at the outset how important and useful it has been. But we also take as a working assumption that even (perhaps especially!) successful works need to have the root metaphors on which they are founded reexamined.

Within what sort of structure should the model represented by Thompson's book be reexamined? And can that same structure serve to suggest alternative approaches as well? The structure ideally should be able to incorporate the dominant perspectives within the field of organizational studies. But if it is derived from those perspectives, it is unlikely to lead us in new directions. The structure (or framework) should therefore be independent of the field, although still intimately related to the nature of its subject matter. From one point of view, the subject matter is organization (no "s"), not a collection of people and tasks, but a property of that collection having to do with orderliness and patterns. This suggests that our framework of analysis should be able to categorize pattern or organization along some dimension. But what dimension? Inasmuch as we would like to be able to extend the field in the direction of handling more complex issues, the structure should exhibit varying degrees of complexity. Suppose we put these two dimensions together—organization and complexity? Can we find or invent a framework that describes varying types of organization along a scale of complexity? If so, this might give us the handle we need both to critically examine the current state of organization theory and to extend it in new directions. In fact, such a framework does exist, and we have used it to organize our analysis and discussion. It was developed by Kenneth Boulding (1968) as part of an attempt to create a

general theory of systems, and it orders systems along a hierarchy of complexity.[2] Inasmuch as theories, models, and viewpoints can be thought of as systems of ideas, it would seem to suit our purposes admirably. In the next section we describe Boulding's hierarchy, and then we shall go on to use it in inventing a future of organization theory.

## BOULDING'S HIERARCHY OF COMPLEXITY: A FRAMEWORK FOR ANALYSIS

Boulding identifies nine levels of complexity. The systems in question can be either "real" systems (e.g., a cell, a chemical reaction, a tree, a bird, a man, a family). Or they can be *models* of those systems. But models are just idea-systems, so Boulding's hierarchy can be taken as a description of the complexity of *either* phenomena or models for analyzing those phenomena. This dual use of the hierarchy to describe both organizations and models of organizations will be helpful in clarifying the state and possible directions of organization theory.[3]

It should be emphasized that adjacent levels in the hierarchy differ in complexity not merely in their degree of diversity or variability, but in the appearance of wholly new system properties. For example, the difference between open-system models of level 4 and blueprinted growth models of level 5 is the presence of the capacity for genotypic growth and reproduction.

Boulding's levels of complexity are as follows:

*Level 1: Frameworks* Only static, structural properties are represented in frameworks, as in descriptions of the human anatomy, the cataloguing system used in the Library of Congress, or an organization chart of the U.S. government. The latter may be complicated, but it is not "complex" in Boulding's sense.

*Level 2: Clockworks* Noncontingent dynamic properties are represented in clockwork systems, as in models of a precessing gyroscope, the diffusion of innovations, or economic cycles in a laissez-faire economy.[4] The crucial difference from level 1 is that the state of the system changes over time. At any given point in time, level 2 phenomena can be described using a level 1 model.

*Level 3: Control systems* Control system models describe regulation of system behavior according to an externally prescribed target or criterion, as in heat-seeking missiles, thermostats, economic cycles in centrally



*Figure 1.* Boulding's hierarchy of system complexity.

*Level 9:* SYSTEMS OF UNSPECIFIED COMPLEXITY

*Level 8:* MULTI-CEPHALOUS SYSTEMS

*Level 7:* SYMBOL PROCESSING SYSTEMS

*Level 6:* INTERNAL IMAGE SYSTEMS

*Level 5:* BLUEPRINTED GROWTH SYSTEMS

*Level 4:* OPEN SYSTEMS

*Level 3:* CONTROL SYSTEMS

*Level 2:* CLOCKWORKS

*Level 1:* FRAMEWORKS

controlled economies, and the physiological process of homeostasis. The crucial difference from level 2 is the flow of information within the system between its "regulator" and its "operator," and in fact the functional differentiation between operation and regulation. For a *given* control criterion, level 3 systems behave like level 2 systems.

*Level 4: Open systems* Whereas a control system tends toward the equilibrium target provided to it and therefore produces uniformity, an open system maintains its internal differentiation (resists uniformity) by "sucking orderliness from its environment" (Schrödinger, 1968, p. 146). Some people have mistakenly characterized an open system as having the capacity for self-maintenance *despite* the presence of throughput from the environment, and therefore have recommended buffering the system against environmental complexity. Quite to the contrary, it is precisely the throughput of nonuniformity that preserves the differential structure of an open system. In an open system, what we might call the Law of Limited Variety operates: A system will exhibit no more variety than the variety to which it has been exposed in its environment. Examples of phenomena describable by open system models are *flames* (simple physical systems in which the transformation of oxygen and, say, methane into water, carbon dioxide, and heat maintain the system's shape, size, and color), and *cells* (biological systems involving complex chemical trans-

formations and differentiated structures, and also the phenomenon of mitosis—duplication through cell division).

*Level 5: Blueprinted growth systems* Level 5 systems do not reproduce through a process of duplication, but by producing "seeds" or "eggs" containing preprogrammed instructions for development, as in the acorn-oak system, or egg-chicken system, or other "dual level" systems. While the phenomenon of reproduction is not involved in language usage, the Chomskian distinction between the "deep-structure" and "surface-structure" of grammar seems to tap the same relationship as in acorn-and-oak (Chomsky, 1972). Both involve a rule-based generative mechanism that characterizes level 5 models. Explaining level 5 systems means discovering the generating mechanisms that produce the observed behavior. And *models* of level 5 systems will exhibit this dual level structure as well. (The intention is that at a given level there is a structural isomorphism between the model and the system. Level 5 systems do, however, have level 4, 3, 2, and 1 properties that can be described using those less complex models, so that a system and a model of that system need not be at the same level. This principle will be seen to be relevant to our later argument.)

*Level 6: Internal image systems* Level 3, 4, and 5 models incorporate only primitive mechanisms for absorbing and processing information. To quote Boulding, "It is doubtful whether a tree [level 5] can distinguish much more than light from dark, long days from short, cold from hot." The essential characteristic of level 6 systems (and models of them) is a *detailed* awareness of the environment acquired through differentiated information receptors and organized into a knowledge structure or image. (Boulding argues that his hierarchy is cumulative—each level incorporates all the properties of all lower levels. However, one might argue that some sophisticated computer software systems are at level 6, yet do not exhibit the blueprinted growth of level 5, unless one wanted to describe the relationship of programming languages to machine language as "blueprinting.") Level 6 systems do not exhibit the property of *self*-consciousness. They do not know that they know. That enters at level 7. Thus, a pigeon in a Skinner box and an organization that forgot why it instituted a certain rule might be examples of level 6 systems.

*Level 7: Symbol processing systems* At level 6, the system is able to process information in the form of differences in the environment. But it is unable to generalize or abstract that information into ideas, and symbols that stand for them. To do that, the system has to be conscious of itself, and this is the defining characteristic of a level 7 system. It has to be able to form the concept "*my* image of the environment," and work on it. And to work on that image, it needs a coding scheme or language. So level 7 systems are *self-conscious* language users, like individual human beings.[5] What is not so obvious is that human groups can be level 7 systems (Ackoff and Emery, 1972). The best example of what it means for a group to have an image of its environment is the process of the social construction of shared models of reality (Berger and Luckmann, 1966). That is, a group can be said to be a symbol-processing entity if its members share a common definition of reality. This is not to say that this approach is not without deep problems. For example, what does it mean for a group to be a language user as distinct from its members? Suppose the members all speak different languages. Then the group is not a language user, even though its members are, and it cannot construct a reality socially. But a group is not necessarily a language user even if its members *do* speak the same language. For a group to use language, not only must verbal interchange take place, but shared definitions of the group's situation must also be constructed.



*Level 8: Multi-cephalous systems*[6] These are literally systems with several brains. Boulding's term for this level is "social organization," a collection of "individuals" (any acting unit) acting in concert. What is at issue is that the collection or assemblage of "individuals," whether they be genes, humans, human groups, or computers, creates a sense of social order, a shared culture, a history and a future, a value system—human civilization in all its richness and complexity, as an example. What distinguishes level 8 from level 7 is the elaborate shared systems of meaning (e.g., a system of law) that entire cultures, and some organizations, but no individual human beings, seem to have.[7]

*Level 9:* To avoid premature closure, Boulding adds a ninth, open level to reflect the possibility that some new level of system complexity not yet imagined might emerge (see also Churchman, 1971).

Having sketched out some features of Boulding's hierarchy of complexity, let us make a bold statement that we will attempt to justify in the remainder of the paper. All human organizations are level 8 phenomena, but our conceptual models of them (with minor exceptions) are fixated at level 4, and our formal models and data collection efforts are rooted at levels 1 and 2.

Generalizing from the above conclusion, our worst fears are that the field of organization theory will take its task for the next decade to be the refinement of analysis at levels 1 through 4. Our greatest hope is that we

will make an effort at moving up one or two levels in our modeling (both conceptual and formal) and begin to look at, for example, phenomena of organizational birth, death, and reproduction, the use of language, the creation of meaning, the development of organizational cultures, and other phenomena associated with the types of complexity in the upper half of Boulding's hierarchy.

## EXAMINING CURRENT "OPEN SYSTEM" MODELING

Since open system models have allegedly played such a central role in organization theory in the recent past, it would be useful to sketch the present view and some of the motives for change. Empty categories in a conceptual framework of approaches tend to attract a field in their direction, but they are insufficient to divert a field entirely from a useful paradigm. We must also show why the open system model, as it has been interpreted, is too limiting.

As we have previously noted, for the last decade, thinking and research in the field of organization theory have been dominated by a point of view labeled as open system models. We have contended that most would agree that Thompson's *Organizations in Action* (1967) comes as close as any to being accepted as a paradigm statement of the "open system" perspective[8] of organization theory. Actually, Thompson intended his book to be a reconciliation of the rational or closed-system model of organizations with the natural system model, and his success in resolving this conflict for the profession probably accounts for the enthusiasm that greeted publication of the book. Despite its reconciliatory intent, the book is dominated by a natural system perspective. But within this perspective, heavy emphasis is placed on closing the system to outside influence so that rational choice can take place.

About the same time or slightly earlier, others besides Thompson (e.g., Lawrence and Lorsch, 1969; Perrow, 1967; Crozier, 1964; Burns and Stalker, 1961; Cyert and March, 1963) made important contributions to articulating the point of view that has subsequently permitted us to analyze and understand the problematic nature of uncertainty for the organization, and how uncertainty ties together technology, structure, and environment in a contingent relationship. The resulting paradigm statement generated a large amount of research and continues to do so. (For example, in the four most recent issues of *Administrative Science Quarterly* prior to completion of this paper, 35 percent of the articles reference *Organizations in Action*, even 10 years after its publication.) We have made substantial progress from where we were in 1967 in the



direction pointed by Thompson. And despite Pfeffer's (1976) recent complaint that organizational behavior has been "... dominated by a concern for the management of people *within* organizations," organization theory *has* researched the organization-environment interface under the guidance of open system thinking. So what is the problem?

The problem is that models not only direct attention *to* some phenomena and variables but also *away* from others. And if a model is highly successful in helping a researcher to cope with problems the model says are important, habituation will take place: The researcher will simply not "see" other problems, and he will have no basis for being receptive to competing models (Hanson, 1958). But there *are* other problems that we should be addressing, and there are competing models that we should be considering.[9] This is the motivation for our arguing that we need to go beyond open-system theory. Specifically, we offer five major reasons in support of this position:

1. By focusing on maintenance of the organization's own internal structure, open-system thoery has directed us away from ecological effects—broadly defined—of the organization's actions, to the ultimate detriment of the organization itself.
2. We should be directing our efforts to understanding massive dysfunctions at the macro level, not just explaining order and congruence. How do organizations go wrong?
3. We need to reflect in our own models conceptions of people in other fields, especially those that picture persons as having the capacities for self-awareness, for the use of language, for creative growth, and for learning from their experience.
4. Troublesome theoretical questions ignored by open-system theory are suggested by other models. For example, do organizations reproduce themselves? If so, how?
5. For the purpose of maintaining organization theory's adaptability as an inquiring system (Churchman, 1971; Mitroff, 1974), we need to discredit what we know, to change for the naked sense of change to prevent ossification of our ideas.

These five reasons for going beyond open system models of organization are closely interrelated and are, we believe, merely five different aspects of the same underlying problem with the field. Each of these motives for change are discussed in detail below. Following this, some alternative models of organization are proposed. The paper concludes with a brief examination of the implications of our position for the doing and teaching of organizational research, and the teaching of present and future managers.

## MOTIVES FOR CHANGE: THE LIMITS OF OPEN SYSTEM MODELS

*The Ecology of Organizational Action*

In order to understand how open system models can blind us to the nest-fouling impact of organizations' actions on their environment, we need to examine how open system theory has been interpreted and used by organization theorists. Frequently, those who claim to be using an open-system strategy are in reality using level 3 control system models. They have failed to make the distinction, as have Haas and Drabek (1973), between "natural" and "open" system models.

Consider Thompson(1967):

> Central to the natural-system approach is the concept of homeostasis, or self-stabilization, which spontaneously, or naturally, governs the necessary relationships among parts and activities and thereby keeps the system viable in the face of disturbances stemming from the environment (p. 7).

In other words, the environment is a source of disturbance to be adapted to, instead of the source of "information" that makes internal organization possible. Self-*stabilization* referred to by Thompson is a level 3 process. The equivalent level 4 process is self-*organization*. Haas and Drabek (1973) recognize the difference between natural and open system models, but classify Thompson (incorrectly, we believe) as an open system theorist. What Thompson calls a closed system is equivalent to Boulding's clockwork (level 2). Thompson made a major contribution by formalizing organization theory at a higher level than it had been at. But we argue that it was not at the level of open systems as understood by Boulding and other systems theorists. There is therefore some question of whether organization theory (as represented by Thompson's book) is even at the open system level, to say nothing of whether it is ready to go beyond it. So this section will have to be split into two parts: (1) the ecological consequences of using a control system model (even though it might be spuriously labeled as an open system model); and (2) the ecological consequences of using a true open system model. By "ecology" we mean the structure of the organization's social, economic, and political environment as well as of its physical and biological environment.

*The ecological consequences of control system thinking* We must remember that the aim of a control system is to produce uniformity, i.e., to decrease variety, if it can. To the extent that the system environment is highly varied in its texture over time, the regulator part of the system must match the variety of the environment so that it can control that variety and produce a uniform environment for its operator part. This is the essence of Ashby's Law of Requisite Variety (Ashby, 1956). In Thompson's language, this means creating the conditions necessary for rational operation at the technical core by controlling environmental uncertainty.



The ecological implication of control system thinking, both theoretical and practical, is that environments as well as organizations will become more uniform. Environments are made up of other organizations each of whom, according to this view, is following a control system strategy. Each attempts to impose uniformity on the others so that uniformity can be created "inside."[10] The result is that the entire system will grind toward a social-system-wide equilibrium. Within the context of a control system model this is a desirable state of affairs. Not so for open systems.

*The ecological consequences of open system thinking* The ecological consequences of open system thinking are quite different from those of control system thinking. An open system is at such a level of complexity that it can maintain that complexity *only* in the presence of throughput from a differentiated environment. If an open system insulates itself from environmental diversity and differentiation, or if it attempts actually to kill environmental diversity, then it will have only a uniform, gray soup to feed on, and eventually its own internal structure will deteriorate to the point that open system properties can no longer be maintained. If control system models are used to manage open systems, the system will be led to take precisely the *wrong* actions! The organization will attempt to insulate itself from the very diversity that it needs to survive as an open system.[11]

Suppose an open system does not attempt to buffer out variability, but exposes itself to the uncertainties of the environment. If environments are plentiful, and the system is agile, it may still extract the needed organizing information from the immediate and present environment, leave it depleted (i.e., undifferentiated) and move on to another.[12] But suppose environments are scarce. A system must then in some sense replenish its environment. It must, paradoxically, put variety back into the environment so that it can subsequently use it. But how to return variety to the environment without deorganizing the system itself?

The key to resolving this dilemma is realizing that only part of an organization's environment is given to it. Another part is enacted (Weick, 1969) by the organization itself. Some people have misunderstood Weick's concept of enactment to be identical with imagination or mental invention. But Weick means that the organization literally *does* something, and once done, that something becomes part of the environment that the system can draw on to maintain its own internal order. To put it somewhat differently, one of an organization's most crucial design decisions concerns how it attempts to design its own environment.

There is a trap here to be avoided. If the enactments are merely an expression of the system's current organization, then nothing new will be

created for the system to feed on. The system can only enact what it already knows. Complex systems have an appetite for novelty (Ackoff and Emery, 1972). They need what Stafford Beer (1964) has called "completion from without." Somehow, the process of enacting an environment must escape this redundancy trap. Weick (1977) has suggested a number of strategies that apply here: *(a)* be playful, *(b)* act randomly, *(c)* doubt what you believe and believe what you doubt (i.e., discredit the existing organization). All these strategies have promise of escaping the trap.

Thinking of open systems as *needing* environmental variety sheds fresh light on the widely replicated finding that organizational complexity is positively correlated with environmental diversity. The usual explanation from contingency theory is that the organization needs to be complex in order to cope with environmental variety. Implicit in this explanation is that "surplus" complexity is possible but not necessary. The alternative explanation flowing from our analysis of open systems is that an organization is unable to maintain internal complexity *except* in the presence of environmental diversity. Surplus complexity is simply not possible from this view, but a shortage is. This might provide a basis for choosing between contingency theory and open system theory.

Hans Hoffman's view of the nature of man captures this property of open systems, especially as it relates to the necessary character of the enactment process: "The unique function of man is to live in close, creative touch with chaos, and thereby experience the birth of order" (quoted in Leavitt and Pondy, 1964, p. 58).[13]

Thus far, we have argued that organizations as open systems foul their environmental nests either by: *(a)* following a control system strategy and deliberately killing off variety in the environment; *(b)* following a shortsighted open system strategy and failing to renew the successive environments that they occupy.

Open system theory as it is currently interpreted and practiced in organization theory does not come to grips with either of these problems. Important exceptions exist (Weick, 1969; Hedberg, Nystrom, and Starbuck, 1976; Cohen and March, 1974), but they do not yet occupy center stage.

### *Dysfunctions in Organization Theory*

One of the striking differences between organizational behavior and organization theory is that organizational behavior (the micro branch of organization studies) defines much of its research effort in terms of dysfunctions of the system. For example, there are theories of absenteeism, turnover, low productivity, industrial sabotage, work dissatisfaction, interpersonal conflict, resistance to change, and failures of communication.



Even equity theory is really a theory of *in*equities, how they are perceived and how they are resolved. But organization theory has been a theory of order.[14] We have theories of the proper match between structure and technology (Perrow, 1967), between environment and structure (Lawrence and Lorsch, 1969), between forms of involvement and forms of control (Etzioni, 1961). Thompson (1967) most eloquently speaks of administration as the "co-alignment" of goals, technology, structure, and environment, and he treats dysfunctions as neither serious nor permanent. Corrective mechanisms, true to the control system model, take care of any problems: "Dysfunctions are conceivable, but it is assumed that an offending part will adjust to produce a net positive contribution or be disengaged, or else the system will degenerate" (Thompson, 1967, pp. 6–7).

The prevailing view in organization theory offers no systematic typology of dysfunctions at the macro or systemic level. But some of the more spectacular dysfunctions have been documented in case analyses. For example, Halberstam (1972) has described the pressures for consensus decision making that operated within the Johnson White House to systematically exclude opposing points of view on our involvement in Vietnam. Janis (1972) has done the same for the decision making that led up to the Bay of Pigs invasion of 1961. Smith (1963) described a number of crises in corporate decision making. And the more recent crisis of bribery within Lockheed and the misuse of power within Watergate are familiar to the point of contempt. But organization theory as a field is currently so preoccupied with explaining order that it has not yet discovered these most interesting phenomena. (Note that it is not even necessary to argue for the study of dysfunctions on normative grounds. From a purely descriptive, nonnormative perspective, such dysfunctions are intriguing scientific happenings.)

Consider Lordstown. Much has been written analyzing how and why the workers reacted to the speed-up of the assembly line. But virtually nothing has been written explaining why General Motors made the wrong decision in the first place. Such decisions about research strategy have been termed "errors of the third kind," or $E_{III}$" (Mitroff and Featheringham, 1974), where $E_{III}$ is defined as the "probability of solving the 'wrong' problem when one should have solved the 'right' one."

It's curious that in economics the situation is reversed. *Macro*economics is focused heavily on the system dysfunctions of inflation, unemployment, and recession. But *micro*economics is concerned with explaining the rationality of choice. Whether that reversal is significant we cannot tell. But in the organizational sciences, it is the macro branch that eschews the inquiry into disorder.

Like all attempts at generalization, this one suffers its exceptions. Staw

and Szwajkowski's (1975) study of antitrust violations is a case in point. And Staw (1976) and Staw and Fox (1977) have studied the phenomenon of escalation experimentally. The use of power to influence the allocation of resources away from rational norms has been studied by, among others, Pfeffer and Salancik (1974) and Salancik and Pfeffer (1974). But the dominant thrust of the field has been explaining why organizations do work well, or at least how they are presently structured to perform the kinds of work they currently do.

Part of the responsibility for this functional orientation can be assigned, we believe, to Thompson's use of *organizational* rationality as a central and integrating concept. By redefining the unit of analysis as the organization *plus* the environment, we would instead be forced to define the bounds of rationality to be broader, to invoke a concept of *ecological* or *systemic* rationality (Bateson, 1972; Churchman, 1971). It is not merely the organization that adapts to the environment. The organization and its environment adapt together. Within such a model of ecological rationality, the environment's problems become also the organization's.

In a recent review of Schon's (1971) *Beyond the Stable State*, Rose Goldsen (1975) summarizes Schon's argument that institutional dysfunctions arise from a belief in the possibility of a stable state buffered against change and uncertainty:

> Change and paradox are not anomalies to be corrected, but the very nature of open systems. Learning systems accept these principles as axioms, rejecting the "myth of the stable state." Our current institutions still base themselves on that myth and it is their compulsive insistence on trying to achieve it that leads to many dysfunctions and ultimate breakdown (Goldsen, 1975, p. 464).

Goldsen, after referring to Schon's redefining of hotel chains as "total recreational systems," asks:

> Is it dysfunctional when informational breakdown in the Coca Cola Company (say) interferes with efforts to maintain sugar economies in developing nations? Is it functional when recreational systems "convert large proportions of the indigenous labor force into waiters, bellboys and cab drivers, chambermaids and prostitutes?" One man's "dysfunction" is another man's "function." (Goldsen, 1975, p. 468).

Within the paradigm represented by Thompson's seminal book, the effects described in the above quote would not be recognized. But Thompson's book was written more than a decade ago. The image of the world that it projects is a history of growth and prosperity, of munificent environments. But times have changed. It is no longer an accurate description of the world we live in. Nor is it a sensible guide to solving the problems our institutions face and have created, because it does not recognize that some of the environmental problems organizations face are of their own making.



If studying the conditions of order is incomplete, how shall we change what we do? We would argue that we need to develop a theory of error, pathology, and disequilibrium in organization (Mitroff and Turoff, 1974). And open system models as currently interpreted are of little help for that purpose.[15]

*Alternative Conceptions of Man and Method*

So far, we have argued that the prevailing model in organization theory *(a)* creates an artificial split between an organization and its environment, to the detriment of both, and *(b)* directs our attention away from the dysfunctional consequences of organizational action. A third reason for needing to go beyond the prevailing model of organization is that it excludes many fruitful models of human behavior. Organization theories seem to have forgotten that they are dealing with *human* organizations, not merely disembodied structures in which individuals play either the role of "in-place metering devices" (Pondy and Boje, 1976) designed to register various abstract organizational properties (e.g., complexity, formalization, etc.), or the role of passive carriers[16] of cultural values and skills. Thompson's conception of the individual is that society provides a variety of standardized models of individuals that organizations can use as inputs:

> . . . if the modern society is to be viable it must sort individuals into occupational categories; equip them with relevant aspirations, beliefs, and standards; and channel them to relevant sectors of "the" labor market (Thompson, 1967, p. 105).

Following Thompson, many macro organization theorists have downplayed man's higher capacities, including his ability to use language, his awareness of his own awareness, and his capacity to attribute meaning to events, to make sense of things. These capacities are characteristic of Boulding's level 5 through level 8. Some macro organization theorists have made language, awareness, and meaning central concepts in their theories (March and Simon, 1958; Weick, 1969; Silverman, 1971), but the dominant trend is still toward mindless conceptions of organization.[17]

There are major exceptions within the social sciences, and we can benefit from examining how they conceptualize the subject matter. Consider cultural anthropology. Geertz (1973), in *The Interpretation of Cultures*, starts out by assuming that assigning meaning to events is a central human process (see also Leach, 1972), and that the task of the anthropologist is to ferret out those meanings and the meanings that lie

beneath them in multiple layers. To describe only the events is "thin" description, but to describe the layers of meaning underlying those events is "thick description." One important class of meanings is the set of beliefs about causality. To Geertz these would be problematic, requiring explanation. But to Thompson, they are given in society and organization members are simply "equipped" with them. But how do those beliefs originate and change with experience? Perrow (1967) has been influential in getting us to think of technology as well or poorly understood, as though technology could be understood without someone to do the understanding. But since technical knowledge varies from individual to individual, the degree of understanding is clearly a property of the object-observer pair, *not* of the object alone (Mitroff, 1974). Similarly, environmental uncertainty does not reside exclusively in the environment itself.[18] (We do not even deal here with the more serious problem of how an organization decides where it, i.e., the organization, leaves off and the environment begins. That boundary, too, is problematic [Weick, 1977].) We have been describing environment and technology "thinly." A thick description would probe into environment and technology as ways of classifying experience and thereby giving meaning to it.

Or, consider Harre and Secord's (1972) recent reconstruction of social psychology. They propose an "anthropomorphic model of man," in which man is treated, for scientific purposes, as if he were a human being! That is, man is endowed not only with an awareness of external events, but an awareness of his own awareness and with a capacity for language (Boulding's level 7). Most importantly, man is presumed to have generative mechanisms that produce observable behavior. The task of inquiry is to discover those mechanisms for each individual. In the prevailing organization theory paradigm, no such mechanisms are presumed. Structure is presumed to exist only at the level of empirical reality.[19] The form of explanation is therefore necessarily comparative across organizations at the same level of abstraction. But with a presumption of a "deep structure" (Chomsky, 1972) that generates the "surface structure" of observable behavior, a "theory of the individual" (Newell and Simon, 1972) makes sense, and a "science of the singular" (Hamilton, 1976) based on a case study methodology becomes rigorous science.[20] The performance programs proposed by March and Simon (1958) are such "generative mechanisms" that produce organizational behavior, and the task of inquiry is to infer the nature of those programs. It is curious that organization theory should have drawn so heavily from parts of March and Simon, but largely missed this very central point of the book.

The existence of alternative models of human behavior is insufficient by itself to cause us to desert open system models. But these alternative conceptions make us aware of phenomena that the prevailing view cannot



begin to handle. In short, the higher mental capacities studied by other disciplines offer a new avenue for organization theory to explore to gain fresh insights into organizational phenomena. With isolated exceptions, those new opportunities have not been explored.

*New Theoretical Questions*

We have previously argued that Thompson's open system model is inadequate for dealing with important practical problems. But it is also inadequate for conceptualizing some important theoretical questions as well. No theory should be expected to cope with the full range of phenomena, of course, but neither should allegiance to a theoretical position become so strong that it prevents us from considering phenomena outside its purview.

One important class of theoretical questions addresses the phenomena of organizational birth and reproduction. Extant open system models, for the most part, are about *mature* organizations.[21] Although Thompson discusses some aspects of growth, his analysis is about continued growth of adult organizations. And it is growth whose patterns are shaped by external forces, not the blueprinted growth of Boulding's level 5. The same is true of the best known treatment of organizational growth and development (Starbuck, 1971).

Biological analogies can sometimes be carried too far, but in this case we believe it is useful to ask whether organizations "reproduce" themselves in any sense. Consider the following model.

1. The development of organizations is constrained by environmental forces, but it is directed by fundamental rules for organizing which are stored inside the organization itself. Those governing rules, or generative mechanisms, produce the observed patterns of differential functioning that make up the organization.
2. The organizing rules are stored in the brains of some, perhaps all, individuals in the organization. Those rules result from a previous process of negotiating the organizational order. Some organizing rules are also stored on *paper* (e.g., job descriptions, standard operating procedures), so that the content of the rules may transcend the tenure of any organization member.
3. When a person leaves the organization, he carries with him those organizing rules.[22] Should he be the founder of a new organization, those rules would find expression through unfolding in a new environment.

This is essentially the underlying model in a recent analysis by Kimberly (1976) of the birth of a new medical school at the University of Illinois.[23] At first glance, Pettigrew's (1976) analysis of entrepreneurship seems to tap the same phenomenon, but I believe something distinct is at

work in Pettigrew's model. Whereas Kimberly is conceptualizing organizational birth as a reproductive process through the mechanism of "fathering offspring" Pettigrew seems to have a model of autonomous birth in mind. The entrepreneurs whom he has studied have formed organizations on the foundation of creative, novel myths or cultures. Those entrepreneurs do not seem to have come from any previous organizational experience.

A second important class of theoretical questions outside of Thompson's model is that dealing with higher mental capacities. We have already alluded to some of the work in the area. How people make sense of their experiences is a crucial issue for organization theory because the answer potentially overturns models of rational behavior. A phenomenological approach is that sense-making is retrospective; we can understand what we are doing only after we have done it. An action-theoretic approach argues that meanings are socially constructed, that therefore there are multiple realities. These positions have been most systematically developed within organization theory by Weick (1969) and Silverman (1971), but their influence on empirical research on organizations has been minimal. Weick's and Silverman's models are extreme points within organization theory.

It is not immediately obvious why Thompson's model precludes such theoretical questions. As we have suggested, part of the reason is that Thompson seems to have mature, already organized systems in mind. What is problematic for him is simply maintaining that organization, not creating it in the first place. A second source of blockage is the causal priority Thompson assigns to norms of rationality. How the organization comes to articulate these norms of rationality is not problematic in Thompson's model, except to say that the organization goals are negotiated within the dominant coalition. But each member of the dominant coalition is presumed to have specific interests already in mind. An alternative model reduces rationality norms to retrospective outcomes. We have not yet discussed the role that language plays in this rationalizing process, but work from other fields suggests that terms in our language affect what we see (Whorf, 1956) and even the logic we use to structure our thought (Tung-Sun, 1970; Alexander, 1967, pp. 37–39).

*Change for the Sake of Change*

All of the four previous reasons for going beyond open systems models have dealt with the substance and content of the theory, its shortcomings, and its neglect of new dimensions it might explore. But all that we have said about maintaining the vitality of organizations applies with equal force to the field itself. Scholars making up organization theory themselves constitute an organization. What can our analysis tell us about how



the field should conduct its affairs, and whether from time to time it should change its paradigm, that is, its world view, its basic models and methods? And can a field come to believe in its models so strongly that it forgets that they are only metaphors of the phenomena being studied, not the phenomena themselves?

If a field's paradigm is too well-defined, or is believed in too strongly, then creative ideas consistent with the paradigm will gradually be selected out. If the field is to continue to be effective in working on worthwhile problems, then it must, to a certain degree, act "hypocritically," that is, in a way that it both believes and disbelieves what it knows (Weick, 1977).

We need to maintain a certain creative tension in what we take to be true (Churchman, 1971; Mitroff, 1974). Our system of scientific beliefs should be a *nearly* organized system—organized enough to provide the confidence for researching uncertain topics, but not so organized that doubt is no longer possible. The illusion of success, especially when hard-won, breeds resistance to change. Scott (1977) has voiced a similar concern:

> After searching so long for "the one best way to organize," this insight [contingency theory] was hard to come by, but having now won it, the contingency approach seems so obviously correct that we are not likely to easily give it up.

In short, we think that it is time to change for change's sake, not because we think we have the correct paradigm to replace open system models, but because we fear that some people have begun to treat contingency theory and other derivatives of open system modeling as the truth rather than as the most recent set of working assumptions. If we have begun to confuse the map with the territory, then it is time to change maps.

This concludes our litany of motives for going beyond the prevailing model of organizations. In the next section we suggest some possible alternatives.

## A SKETCH OF POSSIBLE FUTURES

*Bringing Empirical Work Up to Thompson's Model*

We have previously argued that although the *language* of Thompson's model is at the level of open systems, the actual *content* is wedged at Boulding's level 3, the level of simple control systems; and most of the empirical research and analysis generated by the model has been at level 1, the level of static frameworks, i.e., cross-sectional comparative analysis. Therefore, one promising direction for empirical inquiry is actu-

ally to test Thompson's propositions at the proper level that reflects their dynamic rather than static content. One of the most sophisticated level 2 studies is Nystrom's (1975) analysis of the budgeting, workflow, and litigation processes within the Federal Trade Commission. Using 18 years of data from 1954 to 1971, Nystrom estimated a simultaneous, six-equation model describing funds requested and appropriated, investigations completed, formal complaints, cease and desist orders, and litigations. Two of the equations included time-lagged variables, thus making the model (as well as the data) longitudinal or dynamic. Since the endogenous time lag was only one year in length, the model could not exhibit any natural cyclical behavior, but at least some dynamic characteristics were built into the model. Nystrom's strategy of analysis is important, and serves as a prototype of level 2 analysis.

Even better is the research of Hummon, Doreian, and Teuter (1975). They have constructed a "structural control" model of organizational change that is one of the few rigorous level 3 models in the field of organizational research. A structural control model presumes the existence of equilibrium points (not necessarily stable) within a system of variables, and a set of processes that describe how the system behaves when displaced from those equilibrium points. If the system is stable, it will tend to converge on its equilibrium when displaced. Using Blau's (1970) model of structural differentiation as the content of the control model, and Meyer's (1972) data on governmental finance departments to test it, Hummon et al. demonstrated the feasibility of estimating the equilibrium points, the control processes, and therefore the stability of the system. Just as Nystrom's (1975) research serves as a paradigm for level 2 modeling, the analysis of Hummon et al. (1975) provides a paradigm case for level 3 modeling.[24]

*Reformulating the Open System Model*

If we are to go "beyond open system models," we must first get there in content as well as in language. This suggests a second promising direction for inquiry, now primarily at the theoretical rather than the empirical level. Before we can begin to answer questions about the behavior of open systems, we must first frame fruitful questions to ask. We believe that we have seriously misunderstood the nature of "open systems," and have confused them with "natural" or control systems, as we have argued throughout this paper. By an "open system," we seem to have meant only that the organization is influenced by the environment, or must take the environment into account, or can interact with the environment. But the interpretation advanced here has been that a high-variety environment is a *necessity* to an open system, not a problem, nor even a mere opportunity. The cognitive cycling produced by sensory deprivation provides



an analog at the individual level of the phenomenon we have in mind. We are suggesting that there is a boundary between level 3 and level 4 systems across which the function of the environment undergoes a reversal. The human mind seems to be a system of sufficient complexity that it cannot continue to be a "mind" in an environment of sensory deprivation.[25] Those investigating the area of work motivation and job design have for some time realized the importance of task variety to continued satisfaction and productivity, especially for those with high growth needs (read "high system complexity"?) (Hackman and Oldham, 1975). Is it unreasonable to conjecture that organizations of sufficient complexity also need high task variety in their environments? If so, what are the implications of Thompson's strategies of buffering, smoothing, standardizing, etc.? Do they constitute a self-imposed sensory deprivation for the organization?

If an organization is to advance across the boundary between a control system and an open system, it may need to be *flooded* with variety. Otherwise the control system will have time to develop buffers against a gradually developing complexity in the environment. A dunking in a sudden lack of structure is alleged to be what brings about change in sensitivity training groups. That insight suggests that the rate at which uncertainty overwhelms an organization will be more related to the complexity of its internal structure than just the amount of environmental uncertainty that happens to exist at the time of a cross-sectional study, or the predetermined data collection periods of a longitudinal study. Since "variety floods" cannot, by definition, be anticipated, an opportunistic research strategy is forced upon us if we wish to study the level 3/level 4 metamorphosis. For example, we might wish to study organizations under conditions of natural disaster, or extreme opportunity (e.g., a small organization getting a very large influx of capital or clients). In fact, Thompson, 1967, pp. 52–54) labels organizations that arise in response to disasters "synthetic organizations," and he attributes to them many open system characteristics quite different from the buffered systems operating under norms of rationality:

> . . . headquarters of the synthetic organization . . . only occasionally emerge around previously designated officers . . . [A]uthority to coordinate the use of resources is attributed to—forced upon—the individual or group which by happenstance is at the crossroads of the two kinds of necessary information, resource availability and need . . . [W]hen normal organizations are immobilized or overtaxed by sudden disaster, the synthetic organization rapidly develops structure . . . [T]he synthetic organization emerges without the benefit of planning or blueprints, prior designations of authority, great freedom to acquire and deploy resources, since the normal institutions of authority, property, and contract are not operating (Thompson, 1967, pp. 52–53).

In short, a synthetic organization is a self-organizing open system. But our only quibble with Thompson—a major one—is that such synthetic organizing processes are not limited to natural disasters and are far more common than he suggests.

To keep our models straight, we must be careful not to endow an open system with too many properties that characterize Boulding's higher levels of system complexity. For example, we should not attribute any desire or motivation or even tendency to the system to move from level 3 to level 4, or to seek out environments rich enough in variety to maintain system complexity, or to reproduce itself by means other than mitosis-like duplication, or to have a sense of self-awareness. Those are higher-level properties. The sole property at issue in this immediate discussion has been an open system's capacity for self-organization and the important role of environmental variety in maintaining that capacity. As important as it might be to reformulate the open system model along these lines, that task does not begin to move us nearly far enough along the route toward models of a higher order of complexity. To that we turn next.

*Beyond Open Systems: The Role of Language*

In previous sections we have already dealt, albeit briefly, with possible research questions about organizational birth and reproduction, the concept of generative mechanisms, and with phenomenological and socially constructed realities.[26] But we have dealt only in passing with language and its relevance to organizational research in the future. It is therefore to language that we should like to direct our attention here.[27] Language plays at least four important and distinct roles in social behavior, including organizational behavior:

1. It controls our perceptions; it tends to filter out of conscious experience those events for which terms do not exist in the language.
2. It helps to define the meaning of our experiences by categorizing streams of events.
3. It influences the ease of communication; one cannot exchange ideas, information, or meanings except as the language permits.
4. It provides a channel of social influence.

Silverman has addressed the first two of these functions in his action theory of organizations:

> Social reality is "pre-defined" in the very language in which we are socialized. Language provides us with categories which define as well as distinguish our experience. Language allows us to define the typical features of the social world and the typical acts of typical actors (Silverman, 1971, p. 132).



*Table 1.* Information Sources used by Headquarters Executives of Multinational Corporations

| | | General Management | Field of Specialization: Marketing | Finance |
|---|---|---|---|---|
| Type of Information Source | Documentary | 18% | 30% | 56% |
| | Human (face-to-face) | 71% | 65% | 44% |
| | Physical inspection | 11% | 5% | 0% |
| | | 100% | 100% | 100% |

(From Keegan, 1974)

Language is a technology for processing both information and meanings just as production technologies process material inputs into outputs. Both types of technology constrain what inputs will be accepted and what transformations will be permitted. Languages vary in their capacity to process high-variety information. For example, the language of written communication unaided by nonverbal cues is less able to represent complex events than is the verbal plus nonverbal language of face-to-face communication. Thus we might expect face-to-face communication to be used more heavily in ill-structured fields such as "general management" than in well-structured fields such as "finance," with "marketing" falling between them.[28] Furthermore, in highly unstructured situations, even face-to-face communication may be inadequate for conveying the full meaning. We might therefore expect direct, on-site physical inspection of the phenomenon being talked about to be most common in the poorly structured areas. This is precisely what Keegan (1974) found in a study of information sources used by headquarters executives of multinational corporations, as Table 1 taken from Keegan's article shows.

Although Thompson ignores language as a variable of interest, an earlier classic in organization theory does not; in fact, March and Simon (1958, pp. 161–169) make language a central feature of their analysis of communication in organizations. Like Silverman, they recognize the importance of language in perceiving and defining reality. But they offer a thorough (and largely ignored) treatment of the effects of language on the efficiency and accuracy of communication. They define language broadly to include engineering blueprints and accounting systems as well as "natural" languages such as English. Standardized languages permit the communication of large amounts of information with minimal exchanges of symbols. On the other hand,

> . . . it is extremely difficult to communicate about intangible objects and nonstandardized objects. Hence, the heaviest burdens are placed on the communications system

by the less structured aspects of the organization's tasks, particularly by activity directed toward the explanation of problems that are not yet well defined (March and Simon, 1958, p. 164).

(But we should recognize the earlier point that objects become standardized by having terms in the language for referring to them. Objects are not standardized in and of themselves.)

For example, among physicists, experimental techniques and procedures probably are more ad hoc and nonstandardized than theories. Therefore, we would expect experimentalists to rely less heavily on written publications for obtaining research-relevant information from professional colleagues than theorists, and to rely more heavily on verbal, face-to-face communication than the theorists. Gaston (1972) in a study of particle physicists in the United Kingdom collected data that support our conjecture, as shown in Table 2.

With regard to the fourth function of language, the social influence function, Pondy (1977a, b) has argued that possession of a common language facilitates the exercise of social control, and that organizations can be thought of as collections of "jargon groups," within each of which specialized sublanguages grow up that set it apart from the other jargon groups in the organization. And the size and number of these jargon groups can be related to the age and size of the organizations, its technology, and the rate of turnover of personnel (Pondy, 1975). Within a scientific community, the scientific paradigm provides a language for talking about professional matters (Mitroff, 1974). When this paradigm is poorly developed, as in academic departments of sociology, political science, and English, it has been shown that the turnover of department heads is more frequent than in departments with well-developed paradigms such as mathematics and engineering, the argument being that department heads in low paradigm fields are less able to exercise social control in the resolution of professional conflicts (Salancik, Staw, and Pondy, 1976).

Not all communication operates at the level of conscious, expressed language. Some recent papers have suggested that myths, stories, and metaphors provide powerful vehicles in organizations for exchanging and

*Table 2.* Forms of Communication Used By Particle Physicists in the United Kingdom

| | | Experimentalists | Theorists |
|---|---|---|---|
| Form of Communication | Verbal | 66% | 31% |
| | Publications | 34% | 69% |
| | | 100% | 100% |

(From Gaston, 1972)



preserving rich sets of meaning (Boje and Rowland, 1977; Clark, 1972; Huff, 1977; Meyer and Rowan, 1977; Sproull and Weiner, 1976; Mitroff and Kilmann, 1976. This attention to the less conscious, less rational aspects of organizational language and communication provides one of the most exciting avenues for exploration open to us. It begins to approach the models characteristic of Boulding's level 8.

Let us try to place this discussion of the functions of language in organization theory in context by imagining what it must be like in an organization without the capacity for verbal language. Consider an organization of subhumans incapable of the use of language. While they could exchange signals within a finite set of messages, as is thought to characterize animal communication, they would not have the capacity for producing an infinite number of distinct sentences, as can humans. They would be incapable of reconceptualizing their relationships to each other, their technologies, or their environments. But language permits codification of those conceptualizations, and therefore sharing and social modification of them. Not only is language functional for the operation of the organization, but it is central to the evolution of organizational forms within the lifetimes of individual members. Mind need not wait for genetics to bring about change.[29] If that premise is accepted, then the fundamental structures of languages must be reflected in social organization. By "fundamental structures" we mean such characteristics as the absence of the verb "to be" in Turkish, Hopi, Hungarian, and other languages, or the use of idiographic characters in Chinese. For example, it may be easier to communicate metaphorically in Chinese than in alphabet-based languages. And the fundamental structure of language may dwarf such surface characteristics as "standardization" in their impact on organizational structure and behavior.

In summary, integrating the concept of language into formal organization theory can begin to give us a deeper understanding of perception, meaning creation, communication, and social influence. These are the four functions of language that we listed at the beginning of this section. But less obviously, it can also help us to understand the very processes by which human organizations are created and evolve. There is no better example of this organizationally creative process of language than Burton Clark's (1972) study of organizational sagas. By a "saga" Clark means a reconstruction of an organization's history that stresses its origins, its triumphs over adversity, and its tangible symbols. Clark's study of the sagas undergirding three unique colleges (Reed, Antioch, and Swarthmore) provides us with a method and a theoretical perspective that should be emulated. Through the use of language in creating and propagating a saga, an organization can become much more than just an instrumental social device; it can become a culture with a meaningful past and a

meaningful future. But this image of an organization would not be possible without considering the symbolic and expressive functions of language. In short, language is a key element in moving toward a cultural metaphor of organization.

*Some Implications for Doing Research*

To discuss the implications for teaching and research of any theoretical position on organization theory is a tricky business. There is every likelihood that what we teach now to practitioners will create the very phenomena that we will have available to study in the future. Today's theories can enact tomorrow's facts.

To deny this likelihood is to accept the ineffectiveness of our teaching; to admit it is to reject the role of scientist in favor of one closer to that of playwright. To be quite honest, we have been unable to resolve this paradox, and it circles buzzard-like over what we have to say in this section. Our statements are either prescriptions or predictions, but we cannot tell which.

To summarize what we believe should now be obvious implications of our position for research:

1. Conceptually, the status of an organization shifts from that of an objective reality to one which is a socially constructed reality. Given such a concept of organization, to endow such concepts as technology with measurable and perceivable attributes is senseless. Instead, we need to study how participants themselves come to invoke categories such as "organization" and "technology" as a means of making sense of their experience. The resulting meanings will frequently be stored in organizational myths and metaphors to provide rationales for both membership and activity in organizations. The creation and use of myths and metaphors in organizations is a worthwhile focus for study.

2. More generally, organizations are represented as collections of "organizing rules" that generate observable behavior. While comparative analysis can document empirical regularities at the observable level, the true task of theory is to infer the generative mechanisms, or underlying models, that produce the surface behavior *in each case*. That is, to develop a theory of the individual case is a meaningful scientific activity. Determining whether collections of individuals have the same theories is a proper task for comparative analysis. What we have in mind is analogous to discovering the relationship between a given acorn and oak, and subsequently establishing it for all acorn-oak pairs. By implication, we must drop our reliance on comparative empirical analysis as the only source of scientific generalizations about organizations.[30]

3. These two conceptual hooks imply some radical methodological departures as well. We suspect that questionnaire design, large sample surveys, and multivariate analysis will need to recede in importance in favor of more abstract model-building and ethnographic techniques more suitable for documenting individual cases of meaning and belief systems. This is in no sense a suggestion that we return to the purely descriptive case study. Our aim is to find out how individual organizations work, and that can best be done only one at a time. Whether a collection of individual cases work the same should be the end result of empirical inquiry, not the initial presumption as in the comparative analysis (Leach, 1961). What is at issue is what we mean by the phrase "how things work." Perhaps it would help to point out that the nature of causation changes as one ascends Boulding's hierarcy of complexity. Correlational models of causation implicit in comparative analysis are appropriate only at the levels of frameworks and clockworks, not at the level of blueprinted growth. But at level 5 and above discovering how things work means inferring the underlying model in each case.



The upshot of these three implications for research is that the concepts and methods are all being defined at a more abstract level than the level of empirical reality. Organizations are not just groups of people; they are sets of organizing rules. And "explaining" organizations is not merely establishing empirical regularities across a set of organizations; it is discovering those deeper organizing rules in each case, and only then comparing across organizations.[31] What we need is less brute-force empiricism and more of what Leach (1961, p. 5) has called "inspired guesswork."

*Implications for Management Education*

Thompson's view of organizations suggests that administrators should be trained in the skills of "co-aligning" environment, goals, technology, and structure in harmonious combination. And the conditions of harmony should derive from a rationality based on *organizational* well-being. (Also see Pfeffer [1976] for an excellent description of this position.) These prescriptive out-takes from Thompson's descriptive analysis have, we believe, formed the primary basis for management training in organization theory for the past decade. The position advocated in this paper has a number of contrary implications for management education:

1. By highlighting the true open system characteristics of organizations, managers can perhaps be made aware of the environmental consequences of actions taken in the narrow interests of the organization and be shown the boomerang quality of organizational rationality as the environment becomes more tightly coupled.[32] Somehow, the concept of ecology needs to be generalized and built into the conscious calculus of

administrative decision makers. The most effective—because experiential—way to do that is through large-scale, time-compressed simulations.[33] We may not be able to eliminate the motivation of self-interest, but we may be able to enlarge the manager's time perspective of rationality through such simulations.

2. By developing a typology of system dysfunctions and early warning signals, we may be able to train administrators to react adaptively when Thompson's harmony and co-alignment do not materialize according to plan. To our knowledge, nowhere do we now teach a diagnosis-and-treatment-of-macro-pathologies to managers or would-be managers.

3. We believe that the most radical implication of our position for management education derives from the view of organizations as language-using, sense-making cultures. In Thompson's view, the organization is an input-output machine, and the administrator is a technologist. In our view, the administrator's role shifts from technologist to linguist, from structural engineer to mythmaker. A key function of management according to level 8 thinking is that of helping the organization to make sense of its experiences so that it has a confident basis for future action. That is, the administrator must have a skill in creating and using metaphors. A manager's need to use metaphors skillfully suggests the conclusion that we should be teaching our institutional leaders (and organization theorists!) not only statistical analysis, but also poetry.

## A FINAL NOTE

Having said what we have to say about the future of organization theory, we reflect on it as being already desperately inadequate. It is almost as though the saying of it immediately raises new problems that we must rethink at once. Given this thought and the note on which the preceding section ended, we can think of no more appropriate way to end the paper than the following, from Frederick Morgan's (1977) *Poems of the Two Worlds*.[34]

*Saying*

There always is another way to say it.
As when you come to a dusty hill and say,
"This is not the hill I meant to climb.
That one I've perhaps climbed already—see,
there it looms, behind me, green with trees."
And then climb as you can see the present hill.



Or when you walk through a great childhood forest
latticed with sun, carpeted in brown pine,
knowing the one you were and the one you are,
and think, "I shall not speak this forest's name
but let it densely live in what I am . . ."

The saying changes what you have to say
so that it all must be begun again
in newer reconcilings of the heart.

## FOOTNOTES

1. The authors would like to acknowledge the many helpful suggestions made by David Whetten during the preparation of this paper. A number of others made helpful comments on an early draft of the paper, and we would especially like to thank George Huber, Jos Ullian, Keith Murnighan, Ray Zammuto, Karl Weick, and Gerald Salancik. Various versions of the manuscript were typed by Marsha Kopp and Norma Phegley at Illinois and Lavonne Buttyan at Pittsburgh.

2. An equally fruitful and related framework is provided by Ackoff and Emery (1972). Indeed, the similarities between Boulding's (1968) and Ackoff and Emery's framework is all the more striking since they were developed independently. Of the two, we are tempted to call the one by Ackoff and Emery the more basic, since it is the more grounded in fundamental distinctions, and in this sense, it is the more systematic. However, in this paper we mainly make use of Boulding's framework for reasons of its historical priority.

3. Ackoff and Emery (1972) employ a similar set of distinctions between "abstract" and "concrete" systems. An abstract or conceptual system is a system all of whose elements are concepts, whereas a concrete system is a system at least two of whose elements are (real) objects.

4. In Ackoff and Emery (1972), a common example of such systems is given by the class of entities called "meters," e.g., thermometers, accelerometers, etc.

5. Ackoff and Emery (1972) call examples of systems at this level "purposeful individuals." The prime example here is people, individuals who are capable of displaying "will," the autonomous creation of self-imposed goals (ends), and the ability to invent new patterns (means) of obtaining them.

6. This is our term, not Boulding's.

7. It is important to point out that Ackoff and Emery (1972) do *not* distinguish this level from that of the individual. For them, all of the concepts *necessary* to describe a purposeful individual are *sufficient* to describe a socially organized set of individuals. Note that this is *not* to say that there are no basic differences between groups and individuals. There are. Rather, this is to deny the sharp differences between the level of the individual (psychology) and that of the group (sociology) *without* thereby subsuming either science (or level) within the other (Churchman, 1968, 1971).

8. Because Thompson's point of view has been labeled as being an "open system model," we feel constrained to refer to it that way in this section, although we shall argue that relative to Boulding's definition of open system, Thompson's is not an open system approach. This labeling problem will create some unavoidable, but temporary, confusion.

9. For the necessity of perpetually considering competing models, see Feyerabend (1975).

who argues that science never outruns its need for the strongest competing models if it is to continue to advance. That is, such models are not a luxury but a dire necessity for the continued progress, i.e., development of science.

10. George Huber has pointed out to us that imposing uniformity seems to imply a proactive stance, but that control systems only react. Intentionality should properly be excised from the notion of creating uniformity. Nevertheless, the effect of control systems thinking, whether intended or not, is to create uniformity everywhere. Others have argued that we have seriously misinterpreted Thompson's emphasis on buffering the technical core. What Thompson intended, according to this line of argument, was not to homogenize the environment, but to "order its variety." It was claimed that stockpiling materials and supplies in an irregular market is a way of not only dealing with the environment, but *using* it. How stockpiling *uses* environmental variety is utterly obscure to us, and we stick by our initial interpretation that the force of all of Thompson's strategies for coping with environmental uncertainty is in the direction of producing uniformity, not only for the technical core, but outside the organization as well.

11. Ackoff and Emery (1972) put this point in a rather striking way by showing that every system must be *either* variety increasing *or* decreasing. That is, if an organization insulates itself from diversity, then this becomes equivalent to a strategy of attempting to actually reduce diversity.

12. What it means to deplete an organization of its variety is an extremely abstract concept, and perhaps an example will help to clarify what we mean. Students in MBA programs typically come from a wide variety of backgrounds ranging from engineering to the humanities. Inasmuch as students can learn from one another, this can be a major strength of the program. But the experience of going through the program together tends to make them more alike, thus reducing their original diversity. Originally, faculty members in new discipline-oriented graduate management programs also came from a wide variety of backgrounds, primarily because few programs existed to train faculty for such programs. But as programs have proliferated, they have also produced faculty members for one another according to the standards developed within the programs, thus reducing the diversity of faculty input. Some people have argued that such environments are likely never to grow uniform because they will be continually renewed by new entrants to the field and purely by chance events. We think that such assessment underestimates how closed the system can become, how much it can feed back on itself to produce the optimal inputs it thinks it needs. But in producing optimal inputs, for example, by seeking the type of students who will do best in MBA programs according to the performance criteria of grades, graduate management programs are likely to encourage uniform preparation by program aspirants. Perhaps this effect is clearer in the field of medical education where competition to gain entry to medical school creates both standardized premedical programs and selection on a very narrow base of unambiguous admissions criteria.

13. By "order" we do not mean uniformity. We mean organized complexity, departures from both uniformity and pure chaotic randomness, structured differences that have significance and meaning. And the essence of Hoffman's quote is that order, in this sense, cannot be created out of a uniform environment.

14. We must be careful here to recognize that dysfunction is defined relative to some particular value system. So interpersonal conflict might be functional for a high level of motivation or learning, and turnover might be functional for organization performance if the "right" people leave. Because macroanalysts have tended not to pay much attention to questions of value, they have consequently not paid much attention to organizational dysfunctions at the macro level. But even if one defines dysfunction more narrowly as a departure from equilibrium and from smooth operation, we think that our statement about the focus of organization theory on the creation of order (qua stability, predictability, and uniformity) is still correct. Perhaps this was not true of the organizational sociologists of the 1940s and 1950s (e.g., Selznick, Gouldner, etc.), nor even perhaps of Crozier's (1964) analysis of power and conflict in French bureaucracies, but most macro organizational analysis is now done in schools of management rather than in departments of sociology, and this may account for the stronger functionalist orientation of the last decade. Another way to look at the micro-macro split is that the micro branch's problem-orientation is probably due to its descent from industrial psychology. Industrial psychologists tend to be practitioner-oriented and work on solving managers' applied problems of selection, turnover, training, and so forth. In contrast, organization theory (i.e., the macro branch) has a parentage in the functionalist school of sociology. Given that tradition of descriptive research and aspirations to be "scientific" in the positivist sense, it is not surprising that organization theory tends to eschew the consideration of values in inquiry and gives dysfunctions a minor role in organizational analysis.



15. Focusing on behavior away from equilibrium would also make our models more appropriate for studying (and bringing about) structural change. On the other hand, if our theories posit the existence of a correct way of organizing, changing away from that ideal coalignment makes no sense whatsoever.

16. The passive model of individual behavior implicit in the prevailing model has also been criticized by Argyris (1972). Because of his passivity, the individual is not conceived as a source of organizational change. Argyris argues, and we agree, that change, when it occurs, is conceptualized as a reaction or adjustment to change the environment or technology.

17. Some readers of an early draft have suggested that we seriously underestimated the role that language, awareness, and meaning have played in the organizational theories of Dalton, Gouldner, Goffman, and others, and that we have focused too narrowly on Thompson's book as a statement of the prevailing model. This criticism fails to recognize the fact that for the last decade Thompson's work, not that of others mentioned, has been the prime reference for research on organization, environment, technology, and structure.

18. By this we mean simply that uncertainty is a property of the object-observer pair. For a knowledgeable observer, the environment may seem quite certain, but an uninformed observer of the *same* environment may be quite baffled by what he sees or thinks may happen. Thus, it seems misguided to try to measure *the* uncertainty of the environment without specifying who the observers are and what their state of knowledge is.

19. What is at issue here is that there are different levels of reality: empirical reality or the world of actual behavior; explicit and formal assertions about the rules that govern behavior (e.g., standard operating procedures); myths, rituals, metaphors, and other implicit statements about organizational values and processes; and finally, generative mechanisms of a fundamental sort that produce behaviors of all kinds, not just organizational, and which are typically not easily accessible, for example, the general problem-solving routines of Newell and Simon (1972) or the transformational grammar of Chomsky (1972). Our argument here is that "organizational" structure (i.e., enduring patterns or models of behavior) should be studied at several levels of reality, not merely as an observed regularity at the empirical level.

20. Single case studies, especially qualitative analyses, have been out of fashion for the last 15 years. There seems to have been a presumption that one could understand phenomena at a class level, without needing to understand the individual organizations that make up the class. In fact, we argue quite the opposite, that one needs first to understand the individual case in its own terms, to build a model or theory of it. When one does compare across organizations, what is compared is not the empirical descriptions of the organiza-

tions, but the models of them. Building, and only then comparing, models of organizations is what we mean by developing theories of individuals or by creating a science of the singular case. (See Leach, 1961, pp. 1–27; Pondy and Olson, 1977.)

21. A similar criticism can be raised about most treatises on scientific method and philosophy of science; i.e., they are descriptions (prescriptions) about mature, already well-formulated theories, not about the messy process governing the birth of theories (Mitroff, 1974).

22. He also, of course, carries with him general cultural rules of organizing that are common across organizations. Peoples' personal knowledge of such organizing rules is why some administrative personnel are so valuable to pirate away from competing organizations. If all such organizing rules could be codified in a publicly accessible form, one would not need to secure the services of particular people.

23. It has been argued to us that in Kimberly's case, the founder lost control within a year and structural processes took over pushing the organization back to the norm. In fact, we have first-hand knowledge of the continuing situation that Kimberly studied, and the medical school is proceeding to engage in quite nonstandard programs such as making an institutional commitment to join M.D.-Ph.D. programs with a wide variety of academic departments on the campus. Furthermore, we make no claim that the founder will always be successful in reproducing his image of the organization. So even if the founder of the medical school had lost control, that single empirical fact would not invalidate the organization-reproduction model as a fruitful model to use in our investigations. Theories are not assertions of fact; they are guides to inquiry.

24. Richard Daft has suggested to us that the Hummon et al. model is not a level 3 model after all, because the equilibrium point of the control process is not changed. Nevertheless, it *could* be changed in principle and the system would adapt to the new equilibrium point. That is, a control target is provided for in the model, whereas by comparison in the Nystrom (1975) analysis, there is no state toward which the system is presumed to be tending; Nystrom simply has his system behaving dynamically through time. Another recent analysis that does seem more clearly to embody control system ideas within its structure is Hall's (1976) simulation model of the decline of the *Saturday Evening Post*. Hall's model is a particularly revealing one about the nature of level 3 models, because it demonstrates that control systems are not necessarily stable: The *Saturday Evening Post* after all *failed*.

25. Some care needs to be exercised here, inasmuch as some studies have shown that sensory deprivation can have short-term beneficial effects for individual behavior. For example, Suedfeld reported on an experiment in which "[P]erformance on simple tasks was seldom impaired and often improved by sensory deprivation. Complex task performance, on the other hand, was usually worse after deprivation" (Suedfeld, 1975, pp. 64–65). To carry the analogy to the organizational level, buffering against environmental uncertainty may be dysfunctional only when the technology demands innovative and nonroutine behavior. "[W]e know that sensory deprivation leads to increased daydreaming and fantasizing and to more openness and to new experiences" (Suedfeld, 1975, p. 65). However, extreme deprivation over an extended period of time can lead to a breakdown in complex cognitive processes. We see these qualifications as completely consistent with our argument that open systems need environmental variety in order to maintain their level of complexity.

26. Taken together, these research questions suggest that our underlying root metaphor of organization is shifting. If the first three levels of Boulding's hierarchy can be said to rest on a machine metaphor, and if levels 4 through 6 derive from a biological or organic metaphor, then levels 7 and 8 suggest a cultural metaphor of organizations. Some critics of our position have argued that Thompson really does operate with such a cultural metaphor of organizations. After all, he does explicate the organization's dependence on the embedding social system for belief systems, for occupational categories, and so forth, but it is a particularly



rigid conception of that relationship, and for the most part, organizations are not endowed in Thompson's model with the same kind of cultural properties and functions as the embedding social system. Thompson seems to have made the same kind of assumption as that made by Blau and Scott (1972, pp. 2–5) that the principles of "formal organization" are distinct from those of "social organization" that operate in the social system at large. But a cultural metaphor of organizations suggests that principles of social organization operate within organizations as well as outside them.

27. We define "language" here broadly to include nonverbal language (e.g., paralinguistics, kinesics, proxemics), formal languages (e.g., mathematics, computer programming), as well as natural language. Language is especially important to study not only because it is a phenomenon in its own right, but also because it provides a point of view, a linguistic metaphor, in which to study the relationship of things to the signs that stand for them. But part of our effort is simply to draw attention to language as something worth studying in organizational settings. There are some interesting language vacuums in certain parts of the social science literature. For example, Hare (1972) has recently reviewed a decade of small group research. In all of the studies of communication networks that Hare cites, not one single study so much as mentions language as a variable that was considered or investigated. And if any phenomenon would be naturally expected to include language as a variable, surely it would be communication. In any case, we attempt here to rectify that neglect of language.

28. This argument presumes that the form of a language will be functionally adapted to the setting in which it is used.

29. The argument here is that social organization among subhumans can change only by virtue of species change through genetic mechanisms. The conjecture is that in order to change social structure among a given set of living organisms, that structure must be *talked* about, verbally negotiated. Because humans can talk about their social organization, they can renegotiate it and create new forms of social organization. They need not wait for evolutionary mechanisms to change the social organization through changing the organizing rules that are genetically programmed into the organism.

30. At first glance, this proposal may seem to suggest that all macro phenomena can be reduced to micro phenomena, and that macro phenomena cannot be analyzed at the micro level of analysis—in short, that there are no emergent properties. We are suggesting nothing of the kind. We do not think it is fruitful to try to reduce all of sociology to psychology. What is being suggested instead is that generative mechanisms of a *macro* character produce observable macro level properties, and that these generative mechanisms need not be rooted in individual psyches. For example, written rules that reside in, say, filing cabinets could be such generative mechanisms that produce certain structures. Or Clark's (1972) notion of a saga as a *socially shared* story about the organization's history is a macro level generative mechanism that does not require a reductionist's retreat to individual psychology. All we are suggesting is that it would be profitable to study the relationship between organizing rules stored in the social structure (or, possibly in the minds of individuals as well) and the empirical observables that they produce. That relationship between rules and behavior need not be a perfect one, but that in no sense invalidates the study of the rules or of the rule-behavior relationship.

31. As we elaborated in note 19, there is an empirical level of reality, but there are also deeper levels of reality at which patterns reside: the levels of expressed rules, of metaphors and myths, and of inexpressible rules. All we are saying here is that organizations can be "explained," i.e., patterns can be discovered at any one of those levels of reality. Most of the current work tries to find patterns at the level of empirical reality. We are merely suggesting that we should be looking for patterns at these deeper levels of reality, too. In no sense can this proposal be construed as calling for a hiatus in macro level analysis until

cognitive psychology has told us how individuals conceptualize their worlds. But it does suggest a different strategy of macro analysis.

32. The boomerang effects referred to above include as one important subclass the so-called "tragedy of the commons," in which it is individually rational for each sheepherder to overgraze common pastures even though the collective result for all sheepherders is to destroy the grazing land.

33. The reason that time-compressed experiential learning may be necessary to expand the concept of one's self is that the long-run, indirect personal consequences of one's own actions need to be presented as contiguously and vividly as possible in order to overcome the self-environment split that is so intimate a part of our epistemology. Simply talking about it, as we are doing here, is unlikely to effect the shift.

34. Frederick Morgan, *Poems of the Two Worlds*. Urbana, Ill.: University of Illinois Press, 1977, p. 105. © 1977 by Frederick Morgan. Reprinted by permission of the author and the University of Illinois Press.

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