Introduction

In a recent book, Haas and Drabek (1973) categorize organization research into eight perspectives or conceptual models—rational, classical, human relations, natural system, conflict, exchange, technological, and open system models—and add a ninth of their own, a stress-strain model. Their typology, like most typologies that have been developed to describe approaches to the study of organizations, is empirically derived from the work of practicing organization researchers. It is therefore potentially an excellent frame for representing the past and the present. But it severely limits invention of the future.

Inventing a future for organization theory is my intent in this paper. Doing so requires a framework of possible approaches that is more open-ended than that of Haas and Drabek. To maximize its creative contribution to the field, the framework should be drawn from some other field, yet still be relatable to organizational phenomena. And it should embody a relevant systematic basis for extending organizational research in fruitful directions.

Kenneth Boulding’s (1968) hierarchy of system complexity is a good candidate for such a framework and I shall use it to organize my ideas in this paper. He identifies nine levels of system 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.

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

Boulding’s hierarchy of system complexity

Level 1: Frameworks—Only static, structural properties are represented in framework models, as in descriptions of the human anatomy, the cataloguing system used in the Library of Congress, or an organization chart of the US Government. The latter may be complicated, but it is not ‘complex’ in Boulding’s sense.

Level 2: Clockworks—Non-contingent dynamic properties are represented in clockwork models, as in descriptions of a precessing gyroscope, the diffusion of innovations, or economic cycles in a laissez-faire economy. The crucial difference from level 1 is that the state of the system changes over time. At any given 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 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: 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 organization 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, 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...
Emergence: Complexity and Organization

Level 5: Blueprinted-growth systems – Level 5 systems do not reproduce through a process of duplication, but by producing ‘seeds’ or ‘eggs’ containing pre-programmed instructions for development, as in the acorn-oak system or egg-chicken system. 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. 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. (I shall interchangeably refer to models and systems at each of the levels, hopefully without confusion. The intent is that at a given level there is a structural isomorphism between the model and the system. Level 6 systems do, however, have level 5 properties that can be described using level 5 models, so that a system and a model of that system need not be at the same level. Thus open system models of human organizations can be built even though organizations exhibit higher levels of system complexity than level 4.

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 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. 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. But it is less obvious that human groups are level 7 systems. I am not sure what it means for a group to have an image of its environment, unless the process of socially constructing a reality (Berger & Luckman, 1966) gets at it. And what does it mean for a group to be a language user as distinct from its members being so? 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 is a group a language user if its members do speak the same language?

Level 8: Multi-cephalous systems – Literally systems with several brains. Boulding’s term for this level is ‘social organization’. But the unit of analysis, the ‘individual’, is open to choice, and it might be confusing to refer to a social organization of persons versus a social organization of organizations. What is at issue is that the collection or assemblage of ‘individuals’, whether they be genes, or humans, 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 that entire cultures, and some organizations (but no individual human beings) seem to have.

Level 9: To avoid premature closure, Boulding adds on a ninth, open level to reflect the possibility that some new level of system complexity not yet imagined might emerge.

Having sketched out some features of Boulding’s hierarchy of complexity, let me make a rash statement that I will attempt to justify. Human organizations are level 8 phenomena, but our conceptual models (with minor exceptions) are fixated at level 4, and our formal models and data collection efforts are rooted at levels 1 and 2. My worst expectation is that the field of organization theory will take its task for the next decade the refinement of analysis at levels 1 through 4. My 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 and reproduction, the use of language, the creation of meaning, the development of organizational cultures, and other phenomena associated with system complexity in the upper half of Boulding’s hierarchy.

Since open system models have 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 suck 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.

Open-system modeling

Open-system models have for the last decade dominated thinking and research in the field of organization theory. Most people would agree, I think, that Thompson’s Organizations in Action (1967) comes as close as any treatment to being accepted as a paradigm statement of the open-system perspective of what some might call the ‘macro’ level of organization. Actually,
Thompson intended his book to be a reconciliation of the rational or closed-system model of organizations with the natural or open-system model, and his success in transforming a zero-sum game into a positive-sum game for the profession probably accounts for the enthusiasm that greeted publication of the book. Despite its reconciliatory intent, the book is dominated by an open-system perspective. (After all, closing the system is a meaningful act only within an open-system model!)

About the same time or slightly earlier, others besides Thompson (e.g., Lawrence & Lorsch, 1967; Perrow, 1967; Crozier, 1964; Burns & Stalker, 1961; Cyert & March, 1963) made important contributions to articulating the point of view that has subsequently permitted us to analyze and understand the prob-lematic 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, fully 40% of the articles in the 1975 ASQ cite Organizations in Action. By contrast, only 5% cite Weick’s The Social Psychology of Organizing, a fact of relevance to my later discussion.) 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,” the ‘organization theory’ branch of organizational science has researched the organization-environment interface under the guidance of open-system thinking. So what’s the problem?

Problem is, 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. But there are other problems that we should be addressing, and there are competing models that we should be considering. This is the motivation for my arguing that we need to go beyond open-system theory. Specifically, I offer five major reasons in support of this position:

1. By focusing on maintenance of the organization’s own internal structure, open-system theory 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 changing conceptions of man in other fields, especially those that increasingly picture man as having the capacities for self-awareness and the use of language.

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 OT’s adaptability as an inquiring system (albeit a loosely-coupled one), we need to discredit what we know, to change for the naked sake of change to prevent ossification of our ideas.

These motives for change are discussed more fully below. Following that some alternative models of organization are proposed. The paper concludes with a brief examination of the implications of my 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 between ‘natural’ and ‘open’ system models (Haas & Drabek, 1973).

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 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. But it was not at the level of open systems! There is therefore considerable doubt 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: (i) the ecological consequences of using a control system model (even though it is spuriously labeled as an open system model); and (ii) the ecological consequences of using a true open system model. By ‘ecology’ here I mean the structure of the organization’s social, economic, and political environment as well as of its physical environment.

**Control system thinking**

We must remember that the aim of a control system is to produce uniformity, if it can. To the extent that the system environment is highly varied in its texture and 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. In Thompson’s language, this means creating the conditions necessary for rational operation at the technical core. This is the essence of Ashby’s (1956) Law of Requisite Variety. (It is not usually recognized that Ashby’s Law is a statement about level 3 properties of systems, not level 4 properties.)

The ecological implications 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’. 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.

**Open system thinking**

The ecological consequences of open system thinking are quite different. 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 milks its environment of all its diversity and differentiation, then it will have only a uniform, grey 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.

But suppose an open system ‘realizes’ that it needs its environment and does not attempt to buffer out variability. If environments are plentiful, and the system is mobile, 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. 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 de-organizing 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.

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. Complex systems have an appetite for novelty. They need what Stafford Beer (1964) has called “completion from without.” Somehow, the process of enacting an environment must escape this redundancy trap. Weick (1976) 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. Since there is a premium on this kind of inventive behavior when environments are scarce, we should expect to see playful, random, and discrediting behavior positively correlated with environmental scarcity (assuming that only the successful systems survive). If breaking the law can be thought of as discrediting, then this conjecture is consistent with the recent finding that violation of anti-trust regulations by corporations tends to increase when profits decrease (Staw & Szwarzkowski, 1975).

Thinking of open systems as needing environmental variety also 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 a proper 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 nicely 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 & Pondy, 1964: 58)

So, I have argued that organizations as open systems foul their environmental nests either by:

1. following a control system strategy and deliberately killing variety in the environment;
2. following a short-sighted open system strategy and failing to renew the successive environments that they occupy.

Open system theory as it is currently interpreted and practiced in the field of ‘organization theory’ does not come to grips with either of these problems. Important exceptions exist (Weick, 1969; Hedberg, et al., 1976; Cohen & March, 1974), but they do not yet occupy center stage. (Recall my earlier comment that Thompson’s book was eight times as frequently cited as Weick’s in the 1975 Administrative Science Quarterly.)

### Dysfunctions in organization theory

One of the striking differences between organizational behavior and organization theory is that OB 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, employee theft, interpersonal conflict, resistance to change, and failures of communication. Even equity theory is really a theory of inequities, how they’re perceived and how they’re resolved. But OT is a theory of order. We have theories of the proper match between structure and technology (Perrow, 1967), between environment and structure (Lawrence & Lorsch, 1967), 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: 6-7).

The prevailing view in OT offers no systematic typology of dysfunctions at the macro level. But the popular literature documents some of the more spectacular dysfunctions. 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 Viet Nam. Janis (1972) has done the same for the decision making that led up to the Bay of Pigs invasion in 1961. Smith (1963) described a number of crises in corporate decision making. And the more recent crises 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 so preoccupied with explaining order that it has not yet discovered these most interesting phenomena. (Note that it is not necessary to argue for the study of dysfunctions on normative grounds. From a purely descriptive, non-normative, perspective, such dysfunctions are intriguing scientific happenings.)

Consider Lordstown. Much has been written analyzing how and why the workers reacted to the speedup of the assembly line. But virtually nothing has been written explaining why General Motors made the wrong decision in the first place.

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 I don’t know. But in the organizational sciences, it’s the macro branch that eschews the inquiry into disorder.

Like all attempts at generalization, this one suffers its exceptions. I have already mentioned Staw and Szwajkowski’s (1975) study of anti-trust violations. And Staw (1976) and Staw and Fox (1977) have studied the phenomenon of escalation (a la Viet Nam) experimentally. The use of power to influence the allocation of resources away from rational norms has been studied by, among other, Pfeffer and Salancik (1974) and Salancik and Pfeffer (1974). But the dominant thrust of the field has been explaining why organizations work well
and do good.

Part of the responsibility for this harmony orientation can be assigned, I believe, to Thompson’s use of organizational rationality as a central and integrating concept. By treating 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 rationality (Bateson, 1972). 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, and dysfunctions come to be recognized as real phenomena worth explaining.

In a recent review of Schöns’s (1971) Beyond the Stable State, Rose Goldsen (1975) summarizes Schöns’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: 464).

Goldsen refers to Schöns’s redefining hotel chains as “total recreational systems,” and then 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: 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 ten years 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.

If studying the conditions of order is incomplete, how shall we change what we do? I would argue that we need to develop a theory of error, pathology, and dis-equilibrium in organization. And open system models as currently interpreted are of little help for that purpose.

Alternative conceptions of man and method

A third reason for needing to go beyond the open system model of organization is that it excludes many fruitful models of human behavior. Organization theorists 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 & Boje, 1976) designed to register various abstract organizational properties (e.g., complexity, formalization, etc.), or the role of passive carriers 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: 105).

Following Thompson, the vast majority of 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. They are also characteristic of that property called mind; and what we need to do is to “bring mind back in” (Pondy & Boje, 1976) to organization theory. A small number of organization theorists have made language, awareness, and meaning central concepts in their theories (Weick, 1969; Silverman, 1971), but the dominant trend is still toward mind-less conceptions of organization.

Not so with some other social science disciplines. Consider cultural anthropology. Geertz (1973) in The Interpretation of Cultures starts out by assuming that assigning meaning to events is a central human process, 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. Similarly, environments are not uncertain. (We do not even deal here with the more serious problem of how an organization decides where it leaves off and the environment begins. That boundary, too, is problematic (Weick, 1976). 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.

Consider Harre and Secord’s (1973) 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 (Boulding’s level 7) and with a capacity for language. 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 OT paradigm, no such mechanisms are presumed. 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 & Simon, 1972) makes sense, and a “science of the singular” (Hamilton, 1976) based on a case study methodology becomes rigorous science. The performance programs proposed by March and Simon (1958) are precisely such ‘generative mechanisms’ that produce organizational behavior. It is curious that organization theory should have drawn so heavily from parts of March and Simon, but missed that central point of the book for the most part.

The existence of alternative models of human behavior is insufficient by itself to cause us to desert open system models. But these alternative conceptions expose phenomena that the prevailing view cannot begin to handle. In short, the higher mental capacities studied by other disciplines offers a new avenue for OT to explore to gain fresh insights into organizational phenomena. With isolated exceptions, those new opportunities have not been explored.

**New theoretical questions**

I 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. 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 I believe it is useful to ask whether organizations ‘reproduce’ themselves in any sense. Consider the following model.

<table>
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<tr>
<th>1. Organizational development is constrained by environmental forces, but it is directed by fundamental rules for organizing stored inside the organization itself. Those organizing rules, or generative mechanisms, produce the observed patterns of differential functioning that make up the organization.</th>
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<tr>
<td>2. The organizing rules are stored in the brains of some, perhaps all, individuals in the organization. But those rules result from a previous process of negotiating the organizational order.</td>
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<tr>
<td>3. When a person leaves the organization, he carries with him those organizing rules. Should he be the founder of a new organization, those rules would find expression through unfolding in a new environment.</td>
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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. 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 ‘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 don’t 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. I have already alluded to some of the work in the area. Just a few more comments here: 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’re doing only after we’ve 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 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, especially for American organization theory. But the view they advance is more accepted within anthropology, recent advance within the philosophy of mind, and European organization theory.

It is not immediately obvious why Thompson’s model precludes such theoretical questions. As I 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: 39-47).

**Change for change’s sake**

As my colleague Michael Moch has impressed on me, OT is not only a field of study, it is itself “an organization of organization studiers,” albeit a loosely-coupled one. Since we only meet infrequently, it seems appropriate to refer to ourselves as an occasional organization.

If face-to-face interaction is a criterion for a manifested organization, then OT (or any dispersed scholarly field) spends most of its time underground, surfacing only occasionally during conventions. Although occasional organizations are loosely-coupled through personal interaction, they can be tightly coupled through common adherence to a system of belief, as in the case of a dispersed scientific community’s adherence to a paradigm. Such a paradigm is used not only to guide research by full members of the community, but is also the basis for indoctrination of new members.

If the paradigm is too well-defined, or is believed in too strongly, then creative ideas inconsistent 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, discredit what it knows and act hypocritically (Weick, 1976).

We need to maintain a certain creative tension in what we take to be true. 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 it’s hard-won, breeds resistance to change. Scott (1976) 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, I think it is time to change for change’s sake. Not because I think I have the correct paradigm to replace open system models. But because I 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 my litany of motives for abandoning open system models of organizations. In the next section I begin to outline some possible alternatives.

**Some new directions**
I 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 three, the level of simple control systems; and most of the empirical research and analysis generated by the model has been at level one, the level of static frameworks. Therefore, one promising direction for empirical inquiry is actually to test Thompson’s propositions at the proper level that reflects their dynamic rather than static content. One of the most sophisticated level two studies is Nystrom’s (1975) analysis of the budgeting, workflow, and litigation processes within the Federal Trade Commission. Using 13 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-tagged 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 two analysis.

Even better is the research of Hummon, et al. (1975). They have constructed a ‘structural control’ model of organizational change that is one of the few rigorous level three models in the field of organizational research. (The mathematics will be opaque to nearly all organization theorists, but is easily accessible to any undergraduate engineering major.) 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 away 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. (1975), demonstrate 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 two modeling, the analysis of Hummon, et al., (1975) provides a paradigm case for level three modeling.

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 empirical level. Before we can begin to answer questions about the behavior of open systems, we must first frame fruitful questions to ask. I believe that we have seriously misunderstood the nature of ‘open systems’, and have confused them with ‘natural’ or control systems. By an ‘open system’, we seem to have meant 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 is 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 I have in mind. I am suggesting that there is a boundary between level three and level four 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. 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 & 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 daunting 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 pre-determined 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 three/level four metamorphosis. For example, we might wish to study organizations under conditions of natural disaster. In fact, Thompson (1967: 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, or formal authority to enforce its rules or decisions... [I]t has great freedom to acquire and deploy resources, since the normal institutions of authority, property, and contract are not operating” (Thompson, 1967: 52-53).

In short, a synthetic organization is a self-organizing open system. But my 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 three to level four, or to seek out environments rich enough in variety to maintain system means 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. Having established that caveat, we can move on to consideration of some of those higher level properties.

In previous sections I have already dealt, albeit briefly, with possible research questions about organizational birth and reproduction, and with phenomenological and socially constructed realities. But I have dealt only in passing with language and its relevance to organizational research. It is therefore to language that I should like to direct my attention here. 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 experiences. Language allows us to define the typical features of the social world and the typical acts of typical actors” (Silverman, 1971: 132).

In a sense, language is a technology for processing both information and meanings just as production technologies process 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 non-verbal cues is less able to detect complex events than is the verbal plus non-verbal language of face-to-face communication. Thus we would 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. Furthermore, in highly unstructured situations, even face-to-face communication may be inadequate for conveying the full meaning. We would therefore expect physical inspection 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: 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

<table>
<thead>
<tr>
<th>Type of information source</th>
<th>Field of Specialization</th>
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<tbody>
<tr>
<td></td>
<td>General Mgt.</td>
</tr>
<tr>
<td>Documentary</td>
<td>18%</td>
</tr>
<tr>
<td>Human (face-to-face)</td>
<td>71%</td>
</tr>
<tr>
<td>Physical Inspection</td>
<td>11%</td>
</tr>
<tr>
<td></td>
<td>100%</td>
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Emergence: Complexity and Organization

To discuss the implications for teaching and research of any theoretical position on management and organization theory is a tricky business. There is every likelihood that what we teach now to practitioners will create – assuming that they will practice

Table 2

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<thead>
<tr>
<th>Form of communication</th>
<th>Experimentalists</th>
<th>Theorists</th>
</tr>
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<tbody>
<tr>
<td>Verbal</td>
<td>66%</td>
<td>31%</td>
</tr>
<tr>
<td>Publications</td>
<td>34%</td>
<td>69%</td>
</tr>
</tbody>
</table>

With regard to the fourth function of language, the social influence function, Pondy (1974, 1976) 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 sub-languages 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 organization, 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. 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, et al., 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 for exchanging and preserving rich sets of meaning (Milburn, 1975; Mitroff & Kilmann, 1976). This attention to the less conscious, less rational aspects or 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 eight.

Let me try to place this brief discussion of the functions of language in organization theory in context. Imagine an organization of sub-humans incapable of the use of language. Modes of communication would be ‘hard-wired’ into the organisms. 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. If that premise is accepted, then the fundamental structures of language must be reflected in social organization. By ‘fundamental structures’ I mean such characteristics as the absence of the verb ‘to be’ in Turkish, Hopi, Hungarian and other languages, or the use of idiosyncratic 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 a short paper, there is no easy way to conclude such an open-ended topic as attempting to define organization theory. I have barely scratched the surface of many intriguing ideas. I leave them knowingly but regretfully jagged, and turn in the last section to a discussion of a few implications of my position for the teaching and doing of organization theory.

Some implications

To discuss the implications for teaching and research of any theoretical position on management and organization theory is a tricky business. There is every likelihood that what we teach now to practitioners will create – assuming that they will practice

“...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 & Simon, 1958: 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 non-standardized than theories. Therefore, we would expect experimentalists to rely less on publications for obtaining research-relevant information from professional colleagues than theorists. Gaston (1972) in a study of particle physicists in the UK collected data to support that conjecture, as shown in the following table (2):
what we preach – the very phenomena that we will have available to study in the future. Today’s theories 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, I have been unable to resolve this paradox, and it circles buzzard-like over what I have to say in this concluding section.

The implications for research on organizations follow fairly directly from much of what I’ve already said, but the implications for teaching of practitioners will need to be made more actively explicit.

To summarize what I believe should now be obvious, if not agreeable, implications of my position for research:

1. Conceptually, the status of an organization shifts from that of an objective reality to one which in the extreme is phenomenologically represented in the subjective experience of individual participants, or more moderately is a socially constructed reality. Given such a conception, to endow such concepts as technology with measurable and perceivable attributes is questionable. 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 role that institutional leaders play in the creation of myths and metaphors 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 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 I have in mind is analogous to discovering the relationship between a given acorn and oak, and subsequently establishing it for all acorn-oak pairs. (But that process will be intimately tied up with the definitions of ‘acorn’ and ‘oak!’) By implication, we must drop our reliance on comparative analysis as the only source of scientific generalizations about organizations;

3. These two conceptual hooks imply some radical methodological departures as well. I suspect that questionnaire design, large sample surveys, and multivariate analysis will need to recede in importance in favor of more abstract model-building (as in linguistics) and ethnographic techniques more suitable for studying meaning and belief systems. (The heavy investment in questionnaire design, large sample survey techniques, and multivariate analysis will, of course, occasion no resistance to these suggestions on the part of current practitioners of comparative analysis!) This is in no sense a suggestion that we return to the purely descriptive case study. Our aim is to find out how things work, and that can best be done 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 comparative analysis. 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 you ascend Boulding’s hierarchy 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. And “how things work” is tied up with the nature of causation.

My attempt to summarize the implications for research (and the teaching of research) has turned out to be more abstract than I had hoped. Let me try to be a bit more specific in tracing some implications for teaching of practitioners.

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. These prescriptive out-takes from Thompson’s descriptive analysis have, I believe, formed the primary basis for management training in organization theory for the past decade. The position I have 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 shown the boomerang quality of organizational ‘rationality’ as the environment becomes more tightly coupled. Somehow, we need to generalize the concept of ecology and build it into the conscious calculus of administrative decision makers. My best guess is that the most effective – because experiential – way to do that is through large-scale, time-compressed simulations. We may not be able to eliminate the motivation of self-interest, but we may be able to enlarge the manager’s concept of self 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 my knowledge, nowhere do we now teach a diagnosis-and-treatment-of-macro-pathologies to managers or would-be managers.

3. I believe that the most radical implication of my 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 my view, the administrator’s role shifts from technologist to linguist, from structural engineer to mythmaker. That is, a key function of management in a level 7 or 8 system, is that of helping the organization to make sense of its experiences so that it has a confident basis for future action. The administrator must have a skill in creating and using metaphors. This suggests the delightful conclusion that we should be teaching our institutional leaders not only capital budgeting and inventory control, but also poetry.

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