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**Utilization of Organizational Behavior
Knowledge: The Improbable Task**

**CEO Publication
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Joseph DeBell Chair
Professor of Management and Organization

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ABSTRACT

This paper aims to outline and evaluate the major interventions used to apply behavioral science to improving organizational effectiveness. Such approaches as OD, organizational change, socio-technical systems and QWL are reviewed. Illustrations are provided. Essentially, this is a handbook of applied behavioral science used in and for organizations.

Revised

UTILIZATION OF ORGANIZATIONAL BEHAVIOR KNOWLEDGE:
THE IMPROBABLE TASK

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Not one rummy has been taken off of Baltimore streets by this research. Not one drunken husband has been dissuaded from beating his wife or one drunken mother from beating her child. These research projects are like exotic, expensively mounted butterfly collections, hidden away in vaults and only exhumed from time to time to display to other collectors of the rare and unusual in mutual reaffirmation of their elite status.

-Rep. Barbara Mikulski (D-Md), 1982

The basic question, then, is what must we as researchers do in order to respond more usefully to the pragmatic questions which now face the field...We cannot remain in the aloof stance caricatured in the familiar picture of the basic scientist who prefers to seek after truth untrammelled by the noisy yammerings of the secular world. The fact is that in the practical world in which we must find support for our research we can only hope to settle for half-aloof.

-Morris Parloff, 1980

It almost looks as if analysis were the third of those "impossible" professions in which one can be sure beforehand of achieving unsatisfying results. The other two, which have been known much longer, are education and government.

-Sigmund Freud, 1937

Few people would argue with the statement that we are living in the most satisfying and unsettling period of any that the human species has spent on earth. It is satisfying because we enjoy and profit from discoveries which enable us to live healthier, longer, more fulfilling lives. It is unsettling because we can no longer be certain that what we know today will be valid tomorrow. Should we move against this current? Or do we navigate it, utilize it, and allow it to lead us into a new era, with an air of enthusiasm and audacity? It is not, as Brecht

said, "a night of disaster when a man sees the truth." On the contrary: it is a night of hope; it is disastrous only when the "truth" is neglected or misused.

Although they are not new phenomena, the nonutilization of knowledge and the lack of communication among different groups of our society become less tolerable as society finds itself unable to rely on tradition and more dependent on adaptability and knowledge which facilitates adaptation. We have produced many individuals with highly specialized, greatly useful knowledge for society as a whole; we cannot afford to neglect them, or their contributions. Equally important, they must be aware of the ways in which they can make their contributions most effective and appreciated.

Like C. P. Snow, I sense that there is growing separatism of two isolated cultures. But not the chasm between scientist and humanist but of that between men and women of knowledge who lack power and men and women of power who lack knowledge.

Nowhere is this "implementation gap" more glaring than in the social sciences. Our literature demonstrates reliable and significant applications for social policy. Yet while theory and science proliferate, translation into practice lags behind. This was true over twenty years ago when The Planning of Change was first published. (Bennis, Benne, and Chin, 1961). And it remains true today.

Whenever and wherever we read or hear about the topic of a useable knowledge--whether it arrogates too much or claims too little--whether it's the naive technocrats selling the latest "instrument" or the naive existentialist who feel you just have to get yourself in a good Zen mood and emote, whether it's the parlous situation described by Mikulski or

the despair voiced by Freud or the "half-aloofness" advocated by Parloff, whether it's the discouraged OD practitioner, the hopeful theorist challenged by a theory of practice or the skeptical CEO, I sense an obligation of hope for a usable body of knowledge which will enhance the quality and character of our workaday lives.

This was, after all, at the center of Kurt Lewin's contribution, his preoccupation with the relationship between theory and practice, between the abstract and the concrete. He once compared this task to the building of a bridge across the gorge separating theory from what he called "the full reality." He wrote: "The research worker can achieve this only if, as a result of a constant intense tension, he can keep both theory and reality fully within his field of vision" (1948). Alfred North Whitehead, still another great mind with similar tendencies, commented pungently about the difficulty of braiding theory with practice: "In this modern world," he wrote, "the celibacy of the medieval learned class has been replaced by a celibacy of the intellect which is divorced from the concrete contemplation of complete facts" (1947).

This problem: how to translate knowledge into action contains complexity and depth, as well as a chronic elusiveness. This may explain why the question is either studiously avoided or, worse, written about in such a boring, monotonously shallow manner, uniformly ending up with bromides about "dire straits," dilemmas, and resistances of all kinds. The literature is studded with a litany of restraints and resistances which is not necessarily wrong, only badly skewed, and to such an extent that one would wonder whether there's even a distant hope of an applied behavioral science. Whether this is due to the obvious

difficulties inherent in translating knowledge into action, the unruliness of the field, or the numbing masochism of those us employed in this pursuit, one can only speculate. What I'm impressed with is not the reality of obstacles (which is self-evident) but the challenge, excitement, and promise of a theory of practice.

Between the blur produced by saying too much at once and the banality which comes from dismissing mysteries, there remains the possibility of articulating just what it is that useable knowledge is all about. This chapter will be organized around 3 questions which, when answered, will, at the least, illuminate some of the darkness around this topic and, at most, advance our understanding of a theory of practice. Question 1 is: "What's so?" in which I will examine the present context of applying knowledge to organizations. Question 2 is the "flip side" of Question 1: "So what?" Here I will ask the readers to examine two short cases, state-of-the-art cases on the uses of knowledge to improve organizational behavior which will lead to a better understanding of the implications of current practice. Finally, I will turn to the third question, "Now what?" which will make an attempt to advance the field somewhat closer to a theory of practice.

I

"What's so?"

Knowledge Utilization

Lester F. Ward was one of the earliest social scientists in America to proclaim that "modern men" must extend scientific approaches into the planning of changes in the patterns of their behaviors and relationships. He was well aware that we were already utilizing our

accumulating collective and scientific intelligence deliberately to induce changes in the nonhuman environment. And he saw a major role for the emerging social sciences for extending a similar planning approach into the management of human affairs:

Man's destiny is in his own hands. Any law that he can comprehend he can control. He cannot increase or diminish the powers of nature, but he can direct them...His power over nature is unlimited. He can make it his servant and appropriate to his own use all the mighty forces of the universe.... Human institutions are not exempt from this all-pervading spirit of improvement. They, too, are artificial, conceived in the ingenious brain and wrought with mental skill born of inventive genius. The passion for their improvement is of a piece with the impulse to improve the plow or the steam engine.... Intelligence, heretofore a growth, is destined to become a manufacture.... The origination and distribution of knowledge can no longer be left to chance or to nature. They are to be systematized and erected into true arts. (1950)

Ward's proclamation seemed foolish boasting, if not downright sacrilege, to many among his contemporaries. William Graham Sumner was one of the leaders in sociology who emphasized both the folly and sacrilege of prophecies like Ward's:

If we can acquire a science of society based on observation of phenomena and study of forces, we may hope to gain some ground slowly toward the elimination of old errors and the re-establishment of a sound and natural social order. Whatever we gain that way will be by growth, never in the world by any reconstruction of society on the plan of some enthusiastic social architect. The latter is only repeating the old error again, and postponing all our chances of real improvement. Society needs first of all to be free from these meddlers--that is, to be let alone. Here we are, then, once more back at the old doctrine, laissez-faire. Let us translate it into blunt English, and it will read: Mind your own business. It is nothing but the doctrine of liberty. Let every man be happy in his own way. (Emphasis mine 1950)

It may be fortunate or unfortunate that American controversies today over the direction and management of social change seldom take the form of sweeping societal prescriptions and counter-prescriptions or ideological debates--a form which Ward and Sumner, along with their

contemporaries, gave to them. In any event, the form of the controversies has shifted. In large measure subsequent events have foreclosed the factual basis for Sumner's argument. Laissez-faire has been widely abandoned in practice, even by wide-eyed supply siders, as a principle of social management, whatever ghostly existence it yet enjoys in political platforms and pronouncements. Human interventions designed to shape and modify the institutionalized behaviors of people are now familiar features of our social landscape. "Helping professions" have proliferated since Ward's and Sumner's day. Professions of industrial and public management have taken shape. "Organization Development" is as firmly established as social work.

The reasons for being of all of these is deliberately to induce and coach changes in the future behaviors and relationships of their various "client" populations. This is most apparent in "new" professions such as psychiatry, social work, nursing, counseling, management, and consultation in its manifold forms. But older professions, too, such as medicine, law, teaching, and the clergy, have been pressed increasingly to become agencies of social change rather than of social conservation. Resistances to assuming the new role have, of course, developed along with the situational pressures that advance it.

Behavioral scientists, neo-Summerians among others, have been drawn, with varying degrees of eagerness and resistance, into activities of "changing," such as consultation, training, and applied research. "Helping professionals," "managers," and "policymakers" in various fields of practice increasingly seek and employ the services of behavioral scientists to anticipate more accurately the consequences of prospective social changes and to inform more validly the processes of

planning designed to control these consequences. Sumner's ideological advice has been widely rejected in practice.

But is it equally true that Ward's millennial hope seems far--indeed very far--from realization today. Attempts to apply knowledge in planning and controlling organizational changes tend to be fragmented by the division of contemporary agents of change into specialized and largely noncommunicating professions. These attempts are thwarted too by noncommunication and noncollaboration among policymakers and action planners in the various institutional settings where planning has become familiar practice: industry, government, welfare, health, and education. Advocates and students of planned change have become more cautious in their claims, less millennial in their hopes than Ward tended to be.

Nowadays, ironically, the exaggerated, if not absurd, claims and counterclaims come from the detractors who view the social sciences as some sort of aberrant techno-barbarism or as impotent nonsense. The latter was best expressed by Arthur Schlesinger Jr. in his famous review of The American Soldier where he made sneering reference to sociologists as "fanatical in their zeal and shameless in their claims" (1949). That's the "flip side" of the radical/humanist critique perhaps best represented by Richard Sennett's counter-claims. Sennett believes that "organizational scientists" have trained a generation of industrial leaders to become therapeutic managers," a benign father/mother substitute who can, almost by-the-numbers, co-opt employees by controlling and creating "versions of reality" which can shift employees' attention to "his own feelings instead of dollars and benefits he might receive." (1979) By engaging in a "lot of hocus-

pocus, these so-called change agents make workers feel helpless and vulnerable through the exposure of their feelings."

Closer to home, one can observe a parallel split within the family of applied behavioral scientists, between those who view the applied arm of organizational sciences, OD, as either meaningless stardust or as pernicious coercion. Consider, for example, David Bakan who views the historical-developmental relations between the social sciences and military as encouraging a "positivistic science, on the one hand, and a hierarchy-obedience-force military orientation, on the other...The positivistic handicap in the social sciences keeps them from properly serving in the solution of political, social, and economic problems, thus exacerbating the world crisis and increasing the likelihood of war" (1982). Faucheux, Amado, and Laurent, writing a chapter, "Organizational Development and Change" in the influential Annual Review of Psychology (1982), complain that OD, is an activity that engages in "fine-tuning and tinkering with the system" which inevitably gets stuck on a "human processual approach," one which excludes fundamental change and simply maintains the status quo. (Here, they not only parallel Sennett's concern, but echo them.)

Getting impaled on the other side of the false dichotomy are those such as McKelvey and Aldrich (1983) who write:

Organizational science (or, alternatively, macro-organizational behavior or organization studies) is much less visible on the applied front. The National Academy of Sciences, a body formed to offer advice to the Federal government, does not include organizational scientists. No Presidents' council of organizational scientists exists, and organizational scientists do not frequent the halls of Congress. At UCLA, 100 teams of MBA students act as consultants to Los Angeles organizations each year and find numerous opportunities to apply their knowledge of their accounting and finance, marketing, industrial and labor relations, and operations research, but almost never find ways to apply ideas

or findings from organizational science. In the 25 years since the founding of the Administrative Science Quarterly, Ouchi's (1981) Theory Z is the only book about organizational structure or form to reach the best-seller lists."¹

Robert K. Merton observed a long time ago that when two widely divergent views are held by groups of people, what he referred to as "alchemy," that a deep prejudice must exist; you know, "Jews are clannish...notice how they spread into Gentile communities!" This alchemy is nowhere more noticeable than McKelvey/Aldrich versus Bakan. The latter sees the social scientists' methodological preference for logical positivism as a marching song propelling us toward a world holocaust. McKelvey/Aldrich view organizational science as sadly lacking--"abysmal" is how they put it--in fulfilling any of "the three criteria of the scientific method--classifiability, generalizability, and predictability...." (1983) The alchemy: "Our science is leading us to war" versus "We have no science capable of leading us anywhere."

While I respect their views, I do get slightly vertiginous when I'm told by thoughtful scientists that we applied behavioral scientists are either utterly useless or crypto-Dr. Strangeloves. I suspect that the problem has to do with the scientific masochism of those fear it too much or embrace it too readily.

This ambivalence (alchemy!) was no where better stated than by Kurt Lewin, writing on these issues in 1944:

The relation between scientific psychology and life shows a particular ambivalence. In its first steps as an experimental science, psychology was dominated by the desire of exactness.... Experimentation was devoted mainly to problems of sensory perception and memory, partly because they could be investigated through setups where the experimental control and precision could be secured with the accepted tools of the physical laboratory.

The term "applied psychology" became--correctly or incorrectly--identified with a procedure that was

scientifically blind even if it happened to be of practical value. As the result, "scientific" psychology that was interested in theory tried increasingly to stay away from a too close relation to life.

It would be most unfortunate if the trend toward theoretical psychology were weakened by the necessity of dealing with natural groups when studying certain problems of social psychology...close cooperation between theoretical and applied psychology...can be accomplished...if the theorist does not look toward applied problems with high brow aversion or with a fear of social problems. (1951)

Kenneth D. Benne clarifies and elaborates this ambivalence in role terms and which I find particularly useful (1976). His typology dichotomizes the cognitive worlds of behavioral scientists and of social practitioners and action leaders. He argues that effective collaboration requires recognition and affirmation of epistemological differences on both sides of the social divide, not denial of differences on the ground that they are both persons of good will or polarization of differences into an impassable gulf between "theoretical" persons and "practical" persons.

Table 1

Over the past two decades a substantial literature has been developing with examples of how these cognitive polarities have been transcended as well as other examples of successful utilization of organizational behavior knowledge. Rather than itemize all of these references separately, the interested reader can turn to the following five compendia for a complete listing of references.²

While the literature is abundant, the net findings are inconclusive. In a recent article, Beyer and Trice argue that one of the problems is that the literature is innocent of convincing empirical data. "In the hundreds of sources we pursued, we did not find a single thorough review of the empirical literature on utilization." (1982) In their wide-ranging article, they point out that for the most part the literature on knowledge utilization has focussed on the deficiencies of research whereas they believe that the problem of lack of utilization stems primarily from characteristics of organizations. It might be useful to reproduce a diagram which depicts their conceptualization:

Table 2

Beyer and Trice do manage to redress this by correctly focusing on the organizational processes that facilitate or deflect the utilization of organizational knowledge. And, in so doing, they manage to embrace most of the variables connected with effective utilization. Where they fall short, it seems, is ignoring two other fundamentals: the quality/characteristics of the research and the nature of the relationship between researcher and client system.

A more complete listing of factors has been proposed by a number of scholars: H. Davis (1971) has proposed the acronym, A VICTORY, a convenient memory device for encompassing the eight factors he considers necessary and sufficient to account for organizational behavior related to the utilization of promising new knowledge:

A=Ability,	the resources and capabilities of the organization to implement and subsequently evaluate the innovation; sanctions of decision-makers to adopt the innovation.
V=Values,	the degree of accord with the organization's philosophy and operating style.
I=Idea,	the adequacy of knowledge about the innovative procedure and the proposed action steps.
C=Circumstances,	features of the organization environment relevant to successful adoption or adaptation of the innovation.
T=Timing	readiness to consider the innovation; the particular combination of events at a given time that might affect the likelihood of implementation.
O=Obligation,	the felt need to change from existing modus operandi--or at least to try the proposed change.
R=Resistances,	Inhibiting factors--the organizational or individual disinclination to change, for whatever reasons.
Y=Yield,	the benefits or payoff from the innovation as perceived by potential adopters and by those who would be involved with implementation at the operating level.

Another acronymic list of characteristics of a research finding that may affect its adoption has been proposed by Glaser (1973) under the title, CORRECT. It entails the following attributes, some of which were previously identified by E. Rogers (1962):

1. Credibility--stemming from the soundness of evidence or from its espousal by highly respected sources.
2. Observability--the opportunity for potential users to actually see demonstrable evidence of the knowledge's effectiveness.
3. Relevance--to coping with persistence problem of major concern to influential people.
4. Relative advantage--least cost/high benefit compared to other options.
5. Ease in understanding/installation.
6. Compatability--with user's values, norms, facilities, policies.
7. Triability, divisibility, or reversibility--which permits, less costly experiments and pilots.

Zaltman et al. (1973) offer the following list of attributes that are relevant to describing, explaining and predicting responses to knowledge:

1. Cost.
2. Return on investment.
3. Risk and Uncertainty--on the part of early adopters, less on later adopters.
4. Efficiency.
5. Compatability (same as Glaser's)
6. Communicability--ease of dissemination and clarity of results.
7. Complexity--of ideas and in actual implementation.
8. Scientific status--reliability, validity, generalizability, etc.
9. Perceived relative advantage--its visibility and demonstrability.
10. Point of origin--whether from within or without the organization.
11. Terminality--point beyond which adoption becomes less rewarding, useless, or even impossible.
12. Status quo ante--reversability and divisibility.
13. Commitment--prior attitudinal or behavioral acceptance.
14. Interpersonal Relationships--integrative or disruptive.
15. Publicness versus Privateness--availability to all members of the system.
16. Gatekeepers--number of approval channels.
17. Susceptibility to Successive Modification--ability to refine, elaborate, or modify knowledge.
18. Gateway capacity--opening of avenues to other knowledge uses.
19. Gateway innovations--instrumental setting of stage for large-scale innovations.

There are other lists that are useful. Not to be out-acronymed, Havelock and Lingwood (1973) proposed a rating schema for diagnosing problems in the communication of new knowledge with the designation, H-E-L-P S-C-O-R-E-S which lists ten factors. Glaser (1976), prepared a table which conveniently summarizes 4 of the most widely used and previously mentioned models:

Table 3

The other 3 elements in knowledge utilization, glanced off or touched on, but needing formal notice and elaboration, in this "what's so?" section are: 1) practitioner/researcher (or change-agent/client) relationships, 2) resistances to change, and 3) stages/phases of organizational knowledge-utilization.³

1) Producer/User, Researcher/Practitioner, Change-Agent/Client Relationships.⁴

In their recent paper (1982), Mohrman, Cummings and Lawler argue that "useful information cannot be produced for organizations, but must be generated with them." They argue that if "organizational research is to be useful, researchers and organizational members must become partners in the research effort. Such research should be action-oriented, jointly controlled, and involve relevant stakeholders from both researcher and user committees. Attention must be directed at the transactional contexts of the research." That about sums it up. Adumbrating the principle of with not for is a chorus of voices, both

empirically and/or pragmatically based and which can be summarized with the following six "rules":

1. The research focus must reflect the interests and concerns of the client system.
2. The practitioners should be involved in all phases of research.
3. The research team should include members of the client system, the more influential (within the client system), the better.
4. Frequent and honest communication between researchers and practitioners reduces the likelihood of resistance.
5. Early and continuous clarification of expectations between researcher and practitioner must be engaged in.

Beckhard (1971), one of the most prominent organizational consultants, describes the process of consultation as including the following requirements;

1. It is necessary to establish a relationship with the several parts of the system before any effective problem solving can get under way.
2. It is important to establish a climate and procedures for feedback, both between the helper and the client system and among the parts of the client system.
3. The readiness and capacity of the client system to change needs to be assessed by the consultant.
4. Since the change situation is primarily one of learning, the consultant should create conditions that favor learning.
5. Help offered should be in terms of client, not consultant need.

6. The consultant should be able to withdraw from the relationship, if necessary, to permit independence.
7. Provision should be made for evaluation.

These requirements or provisions may sound on the edge of bromides; the fact is that they are more often honored in the breach and not in practice. In short, they are easier said than done. What appears to be the centerpiece of the change agent/client relationship or, in other terms, the researcher/practitioner relationship, is a deliberate, collaborative process. The outcome of any successful knowledge-utilization activity appears to hinge on that, on how well the giver and receiver of help, if you will, understand and participate in that relationship. Over twenty years ago in The Planning of Change, I wrote:

A number of features distinguish the "deliberate and collaborative relationship": (a) a joint effort that involves mutual determination of goals; (b) a "spirit of inquiry"--a reliance on determinations based on data publicly shared; (c) an existential relationship growing out of the "here-and-now" situation; (d) a voluntary relationship between change-agent and client with either party free to terminate the relationship after joint consultation; (e) a power distribution in which the client and change-agent have equal or almost equal opportunities to influence the other; and (f) an emphasis on methodological rather than content learnings. (1961)

The basis for this stress on collaboration, which virtually every scholar/writer/practitioner have since extolled, was not only the important ethical considerations, but, more important, the pragmatic considerations. The only way any client will successfully adopt new knowledge, knowledge at an angle to the organizational culture, is by providing enough positive support so that the opposing forces in the client's situation can be re-equilibrated on a new and desirable level. This can come about only by facilitating the client's communication with him or herself--or, in more general terms, by making the client (as well

as the change agent) aware of the relevant data necessary to diagnose the situation. The source of much of these data is in the client-system itself, if only the client can make it publicly available. Without trust, generated in and by collaboration, the change-agent and client must work with limited and occasionally distorted data as children in the dark.

Field researchers studying natural organizations and cultures have long acknowledged their reliance on "trust" to counter the strong resistances on their subjects' parts to yielding important data. The rules and techniques of "establishing rapport" found in methodology textbooks also testify to this need. Clients are no different from research subjects in this respect. Though, in most cases, they sincerely and seriously want help (want to change, want to use the knowledge constructively), powerful forces exist that tend to work against that change.⁵

One last point should be made about collaboration. The process of developing a collaborative relationship between client and change-agent may in itself provide a crucible for understanding the problems the client faces in his or its ordinary work and life environments. To this extent the collaborative relationship provides a cognitive support as well as an effective prop. Many consultants, change-agents, and researchers utilize problems in the evolving relationships they have with their clients as existential exemplars of the other relationship problems the client must deal with. To this extent, the collaborative relationship represents a microcosm of all other relationships and, as such can augment the help the client may require.

In reality, it is difficult to find a purely collaborative relationship; the best to be hoped for is a commitment on the part of both the knowledge provider and the seeker to work toward building such a relationship. Along with that recognition, ambiguities and irregularities almost always exist. All the same, I view collaboration as a necessary condition of the successful use of organizational behavior knowledge. It is necessary not only because it generates the necessary trust and facilitates the collection and interpretation of data, but also because the positive aspects of the relationship qua relationship are vitally necessary in order to overcome some of the strong fears of and resistance to change in the client-system. Lets turn to that now.

2) Resistances to Change.

In an overall analysis of factors accounting for "resistance to change," Watson (1973) distinguishes between "resistance in personality" and "resistance in social structure." Under the former, he groups the following factors: homeostasis, habit, primacy, selective perception and retention, dependence, illusion of impotence, super-ego, self-distrust, insecurity and regression, deprivation, and anxiety. Factors in social systems that contribute to the resistance of change are listed as: conformity to norms, systemic and cultural coherence, the sacrosanct, rejection of "outsiders," hierarchy, affluence and leeway, restricted communication, and the nature of innovation. That's a useful distinction and one that must be kept in mind before anything further is stated. Another useful distinction, too often forgotten, is that what the change-agent construes as "resistance" may well be a realistic preservation of the system's values, and not "defensive" at

all. Klein (1966) reminds us that the change agent may often define change in such ways that those who do not agree are seen as blind resisters of change. It is as if the change agent is saying, "We have values; the clients have psychological mechanisms." It seems more appropriate to view the occasion as an opportunity of working with the internal roles of the system, including those who defend (not "resist") the status quo, to foster mutually desirable change.

Having said that, it may be useful to review some of the fundamental generalizations scholars have proposed as the determinants of resistance to the acceptance/adoption of knowledge.⁶

1. Perhaps the most frequently found generalizations regarding resistance is that resistance occurs when those affected by the change perceive it as threatening. In this context, resistance is viewed as a device that functions to protect the individual against fears and anxieties aroused by the implications of the proposed change. Almost all of the following conditions of resistance stem from this overarching factor.

2. Fear of loss of status or prestige or power--or just about any loss that will lower self-esteem. This one is simple to understand and basic to the understanding of resistance: People who have benefited the most from an existing order or norm are unlikely to welcome a major change if they perceive that in some way or another, they will lose something.

3. Because any novelty may threaten either devaluation of the knowledge and skills presently required or require knowledge not presently known or seen as difficult to acquire, new knowledge may be perceived as threatening job security.

4. Resistance is aroused when proposed change threatens or challenges currently held beliefs and values. Some persons cannot seem to hear or understand proposals that appear to run counter to long and firmly held beliefs. Berlin (1969) points out, for example, that "learning new methods of working and especially using new models like public health concepts, are threatening to our established and already learned theoretical frameworks and practices." Since the practitioners' framework is essentially their professional value system, they are likely to protect all components with some fervor. Moreover, innovations backed by research findings may be especially threatening to professionals--especially when the theory has not itself been tested empirically. (Glaser, 1976)

5. Fear of loss of self-esteem or sense of competency and/or fear of exposure of weak points can arouse very strong resistance. One study in a mental health setting, for example, reports that resistance emerged because of the researchers' failure to directly acknowledge currently successful efforts of the client system. (Poser, Dunn, & Smith)

6. Another fundamental generalization is that people resist changes they don't understand. Many investigators have found a high correlation between the unknown and high resistance. LaPiere (1965) points out there is a pervasive fear of the unfamiliar among humans. "Fear of the unknown," he states, "can even override the certainty of acute physical pain." Repeatedly, it has been found that rejections occurred because of lack of adequate information. Sometimes it was because the knowledge (or innovation) was itself too complex.

7. In some enterprises there is a kind of resistance--often waggishly referred to as N.I.H., "Not Invented Here." Fox and Lippitt

(1964) among others found that teachers felt it would be beneath their status to borrow from others.

Perhaps the best summary of factors that affect resistance was compiled by Zaltman et al. (1973):

"1. Among the possible determinants of resistance are: a) the need for stability; b) the use of foreign jargon; c) impact on existing social relationships; and) personal threat; e) local pride; f) felt needs; and g) economic factors.

"2. Structural factors affecting resistance include: a) stratification; b) division of labor; and c) hierarchical and status differentials.

"3. Individual resistance factors include: a) perception; b) motivation; c) attitude; d) legitimization; e) accompaniments of trial; f) results of evaluation; g) actual adoption or rejection; and h) manner of dissonance resolution."

As far as reducing resistance to change is concerned, it is important to remember that most persons and client systems are in a "quasi-stationery equilibrium" with some forces driving them toward change, others resisting. To reduce the resistance creates forward movement with less tension than if effort is made only to override. That's often hard for the advocates of change to understand; even harder, it seems, is for those advocates of new knowledge to empathize with, let alone understand, the importance of the social role of the defenders who try to preserve the valuable elements of the old in the face of a tumult of change. (Klein, 1966)

In discussing resistance to change, Watson (1973) outlines the life cycle of resistance to an innovation and the psychological factors of individuals and groups that affect resistance:

1. Undifferentiated resistance.
2. Differentiated resistance.
3. Mobilized resistance, resulting in a "showdown."
4. Sufficient success so that only conspicuous error could re-mobilize the resistance, with supporters of change taking power.
5. One-time advocates of change becoming resisters of emerging change.

Kanter, in her new book, The Change Masters (1983), finds it "interesting that organizational theorists have produced much more work, and work of greater depth and intellectual sophistication, on the recalcitrance of organizations and their people--how and why they resist change--than on the change process." I suspect that she's right about that for a number of reasons, not the least of which is the inherent difficulty in measuring the elusive concept of change. Having said that, there are a number of behavioral scientists who have attempted to outline the phases of change processes within organizational settings. Which brings us to the final domain of "What's so?"

3) Stages in the Process of Knowledge Utilization

It should be stated at the outset that virtually all of the writing on stages and phases of organizational adoption of knowledge is fragmentary, speculative and, with few exceptions, based on single-cases; which is not to say that the following generalizations are invalid or implausible. It's that they have to be taken if not with a grain of salt, then, most emphatically, with some suspension of belief.

Glaser, (1976) has provided the most complete and thoughtful summary of the writings on the subject, ranging from Dewey's classic five-stage analysis of problem solving to Zaltman et al.'s more recent two stage process. Glaser's table furnishes a matrix of the various, most cited writings on the topic:

Table 4

As Glaser points out, there are differences in terminology, but the parallels, as shown in Table 4, are substantial . All begin with a drive or need--a concern or a problem or some discrepancy between an ideal and reality. From there, each moves to some form of diagnosis or analysis. All theorists recognize a need for acquiring valid knowledge on the basis of the earlier diagnosis and move to an implementation state, followed by a "follow-up" or evaluation stage.

There's far more to the complex world of translating knowledge into organizational reality--of knowledge utilization--than I've managed to cover so far. Some other areas--neglected here--will be covered in later sections. My feeling, as I finish this first section on "What's so?" is very similar to a respondent in a study of research utilization by mental health agencies who said:

You know, just the questions you've asked make me painfully aware of what has not been done to report these findings to the groups who could use them. I'm behind schedule now, and I'll do the NIMH final report and the some journal articles, but I know I'll not get around to the rest of what should be done. I'm pressed for time as it is.... My intensions were good...why I can rattle off dozens of agencies who would want this information right now...next time.... (Glaser, 1973, P. 144)

The problems connected with utilization of knowledge, both from the point of view of those who are producers, such as the one quoted above, and those, like the author of this chapter, who write about it, appear to be similar, if not identical. I am also "painfully aware of what has not been done," but equally aware that there may be no "next time." Which leads to the second question and section: "So What?" What are the implications of the present state of the art regarding knowledge utilization?

II

"So What?"

Why, with all that has been written about the application of knowledge to organizational settings, with all that has been said about the issue, why then, am I still left with a feeling bordering on wistfulness, of something badly lacking, missing. The truth is that I'm not altogether clear about, nor can I justify, that something is seriously missing. With that void in mind, it's just possible that by examining two case studies, some further illumination of the darkness of this exquisitely complex area may come about. We shall see.

Some of the issues dividing the thinker from the doer may be illuminated by brief descriptions and appraisals of 2 applied social science projects. The significance of these projects is reflected in their multimillion-dollar budgets and the attention they have received, one notorious, the other meritorious. Aside from their monumental proportions, they were ambitious in design, intended to influence, in one case, the policies of a large, multinational firm, and in the other, less successful case--some would say it was a "failure"--national and international policy. Though the so-called "failure" cannot be considered an "orthodox" OD case, it poses significant questions for any OD practitioner or theorist. Both cases have been under attack and both widely praised.

The first and most spectacular is Project Camelot. The cause celebre, Camelot, was an action-research study, as described by its task title, of "methods for predicting social change and internal war potential." Camelot was to take three to four years and to cost,

roughly, \$6 million. The research areas were those where there was considered to exist a high potential for internal revolution; the starting point was Latin America and proposed future research areas included several countries in Europe, Asia, and Africa. In the first of four phases it was proposed to examine existing data on internal war, and it was during this period that the project was interrupted.

The beginning of the end occurred when an invitation to many American and foreign social scientists to a four-week planning conference stated the objectives of the study and the identity of its sponsor, the U.S. Army.

One of the recipients was Johan Galtung, a Norwegian sociologist teaching in Chile at UNESCO's Latin American Faculty of Social Science, whose area of research is conflict and conflict resolution in underdeveloped countries. According to a fellow social scientist, Irving Louis Horowitz, Galtung gave the following reasons for refusing the invitation:

He could not accept the role of the U.S. Army as a sponsoring agent in a study of counterinsurgency. He could not accept the notion of the Army as an agency of national development; he saw the Army as not managing conflict but even promoting conflict. Finally, he could not accept the asymmetry of the project--he found it difficult to understand why there would be studies of counterinsurgency in Latin America, but no studies of "counterintervention" (conditions under which Latin American nations might intervene in the affairs of the United States). (Bennis, 1970, P.2)

In April, 1965, Hugo Nutini, an Assistant Professor of Anthropology at the University of Pittsburgh, made a trip to Chile on other academic business. Dr. Nutini offered to speak to his friends in the Chilean academic community about Camelot, and the Camelot authorities accepted his offer. Although Chile was not intended to be one of the countries where research would be done, it was hoped that Chilean social

scientists would participate. According to Chilean reports, Dr. Nutini met with Alvaro Bunster, Vice-Chancellor of the University of Chile, and discussed the study without identifying the Army as the sponsor or making it clear which social scientists were involved in the study. At a second meeting, Dr. Nutini was confronted with a copy of the invitation that Galtung had received. Dr. Nutini protested that he knew nothing of the sponsorship, that he had been misinformed and would protest to Washington. At the same time, the letter was turned over to the Chilean press and to members of the Chilean Senate. The time was dramatically inopportune: shortly after the United States' intervention in the Dominican Republic.

Some American sources report a different course of events. Nutini was not given the opportunity to explain who the sponsor was, not to discuss the study. According to Camelot authorities, the "brouhaha" was Communist inspired and that Communist dominated organizations and individuals were making "a mountain out of a molehill."

It is true that leftist newspapers, especially, played up the incident, but not only leftist sympathizers were disturbed about it. In Chile, and throughout Latin America, people of all political opinions were aroused.

Latin Americans were not the only ones concerned about the Pentagon's role in foreign affairs. Congress questioned the disparity between Defense and State Department budget allocations which gave the Defense Department a much greater funding ability for research than the State Department had. State expressed concern that this kind of research was being done by the Defense Department and that such sponsorship might have a damaging effect on foreign affairs. And the

State Department was accused in some governmental and academic circles of deliberately leaking the crisis to the press to emphasize the questions of appropriate sponsorship. Both Senators and Representatives who expressed themselves on the subject--and there were many--questioned the role of the military in the area of foreign affairs and social science research abroad.

For their part, academicians were concerned over the image of social science research and its future. They protested censorship and questioned the ability of the State Department to evaluate research.

Few parties were satisfied with the situation as it stood after Camelot.

What appears to be an OD success story started at the Baton Rouge refinery of what was then called "Esso," now Exxon, when some key members of Exxon management became favorably disposed toward "sensitivity training" and felt that this experience-based learning could help to open up communication and develop trust within the organization. (Rush, 1973)

The beginnings of this OD program came about and can be traced to 1956 when the company asked behavioral scientists about the action-research method of using sensitivity training for managers. At that time, sensitivity training ("T-groups" or "lab training" or "Encounter groups" were used interchangeably) was a relatively new development in the business community. A highly placed and influential corporate executive had been "through a lab" and was receptive to the idea of this activity for management development. At the same time, he specified that participation in this type of training should be the option of each operating plant's management, in keeping with Exxon's

decentralization policy. After some other key executives had gone through the basic two-week sensitivity sessions--so-called "stranger groups" offered by the National Training Laboratories at Arden House in Harriman, N.Y.--they returned to several plants enthusiastic about the potential that this kind of "laboratory training" held for what Exxon then called "organizational improvement."

At the Baton Rouge refinery, local management decided that sensitivity training was just what was needed to help the organization cope with changes then taking place in the operation of the refinery. Automation, union-management problems, manning practices, and personnel reductions were causing some major problems at Baton Rouge, then the flagship refinery for the entire company. Underlying these problems was a fundamental problem of all the refineries, then as well as today: how to maintain a competitive cost position. If the refinery were to retain its profitability, so management figured, it would have to make changes with as little upheaval in the organization as possible.

Beginning in 1957 and continuing into the early '60s, the refinery had over 700 supervisors, managers, and scientists participate in what became known as a "classic, 14-day sensitivity training lab," conducted at Gulf Manor, a deluxe resort-hotel. Exxon had decided to use outside, typically university-based "trainers" to conduct the training. The company figured, correctly, that it would be far more expensive, not as company-relevant, and too time-consuming if all of these managers were shipped out to attend "stranger labs." To the best of my knowledge, these training sessions for teams of managers was the first example of "in-house" laboratory training for management.

The company was more than satisfied with the results. It accomplished its goals: reduction in work force with no disruption (or at least fewer upsets with the union), carried out job enlargement and enrichment programs, reduced costs, and its independent unions maintained their strength. (Rush, 1973, P. 60)

Despite that apparent success, sensitivity training began to fade in the early '60s because management believed that while it was extremely effective and had high value for the individual manager, it was not designed to accomplish work-related objectives.

It then turned to the Managerial Grid as its main source of "organizational improvement," later to be called OD, and was undertaken to improve the effectiveness of yet another refinery, in Baytown, Texas, and "to validate the concepts and the hypotheses of quantifiable changes in the culture of a functioning organization with multiple internal and external influences (as contrasted with a pure laboratory environment.)" (Rush, 1970, P. 61) Thus, the introduction of the Grid at Baytown was an action- research project with normative values. As such, the project was followed and measured throughout. (Blake, Mouton, Barnes, & Greiner, 1964)

All told, about 800 managers at all levels of the management hierarchy participated in the Grid OD experiment. An evaluation study was conducted and indicated that the organization changed in the direction posited by Grid theory. Exxon continued to use The Managerial Grid and the program was extended to 600 unionized workers, one of the first times that this kind of training fell below the middle-management line of hierarchy. Shortly after that, sometime around 1966 or so, no more formal OD activities, such as the Grid or lab training have been

employed, although, according to Rush (1973), the historian of this event, group process type training is still used but "only on a selective basis...or in special circumstances." (Rush, 1973, P. 61)⁷

Exxon did not use Grid or lab training at other facilities either. A staff specialist, then an internal "change agent" at Exxon, told Rush:

We were convinced that Grid was appropriate for the Baytown culture at that time, but since we have found we are able effectively to use other techniques of organization improvement, such as rational methods of problem solving and goal setting in a modified managing-by-objectives program." (Rush, 1973, P.61)

The causes for failure in the Camelot project and the relative success of the Exxon project were identical and were hinted at in the preceding section: sponsorship, clearance, communication, and collaboration.

The Camelot project was sponsored by the American government; such sponsorship indicated an acutely one-sided, pragmatic purpose. In their criticism of Camelot, almost all Latin American countries mentioned the sponsorship as cause for doubting the credibility of the approach. In the Exxon case, not only was top management behind it, the decision to pursue OD was made (not in Houston or NY) but at local plants with local option. When the union was involved in Baytown, it, too, was consulted and maintained "joint ownership" of the program with management.

The proposed host countries of Camelot apparently did not understand the project and its intent. Although statements were made that these parties had been adequately informed, the greater number of assertions emphasized that henceforth no such research would be done in a foreign country without the country's prior knowledge and consent. As Senator William Fulbright put it, "The reason for its (Camelot) offensiveness is obvious to anyone with an iota of common sense and it

seems to me it should also have been obvious to the highly trained "scientists" at American University, as well as to the Army. At a time when U.S.-Latin American relations are complicated by our intervention in the Dominican Republic, it is not surprising that a project like Camelot should be interpreted as having some pertinence to a possible U.S. military intervention in Chile in the event of a revolution." (Bennis, 1970, P. 3)

The failure to go to the top for commitment, as well as to gain the cooperation, clearance, opinion, and advice of all relevant parties to the research effort, both subjects and clients, betrays the prevalent naivete which cannot be explained solely by the fact that social scientists have had very little experience and no preparation for applying their knowledge. The behavioral scientists, working for Exxon, were no better prepared and certainly no better trained than the Camelot social scientists.

Unlike the Exxon project, in the Camelot project, foreign colleagues and field representatives were not taken into a collaborative relationship; they were not party to the sharing and exchanging of ideas and opinions at all stages of research. In many countries, stability and dictatorship go hand in hand with oppression of the people and the absence of progressive government. Perhaps we believe that "internal revolution" is always Communist inspired and results in a seizure of power. Or is this what we are perceived to believe? Senator Fulbright and others felt that Project Camelot denied the possibility that "internal revolution" could be a change for the better, something to be promoted rather than squashed.

Lack of collaboration is always a disadvantage in a scientific undertaking; it can be fatal in an undertaking which is designed to explore sensitive areas and areas in which the researcher hopes to influence or teach or help his or her subjects. (The word, "subjects," probably connotes the general impersonal attitude scientists hold toward their informants. The rub is that subjects, these days, are not necessarily docile college sophomores or pliant white rats; they can and do talk back--and create "international scenes.") Giving help is always easier than receiving help, for the former implies some expertise or superiority while the latter smacks of weakness or inadequacy. The psychology of giving and receiving help is beyond the scope of this chapter but clearly must be understood if U.S. foreign policy is to work, or at a less exalted level, if our knowledge is to be useful to policy makers.

Those behavioral scientists working with Exxon, starting in the mid-fifties and for a decade thereafter, seemed to "intuit" the delicacy of their task and managed to use sponsorship (top management), clearance, communication, and collaboration in such a manner that the client system, by the end of a decade, internalized (institutionalized) the capacity to make deliberate choices of its own about future training needs and also had developed the internal staff to implement them.

To get at some of the other implications of an applied behavioral science, I want to focus now exclusively on the Exxon case. What becomes immediately apparent is that the normative goal of OD--using OD as the exemplar of "knowledge utilization in organizations"--is to "humanize" bureaucracy.⁸ Almost no one, save for some cynics and cranks, would argue with that goal, and certainly some improvements have

been made, if not always in practice, then at least in theory. Such terms as "participative management," "Quality of Work Life," and "Socio-Technical Systems" reflect this tendency. These terms also reflect democratic values held by most social scientists toward their clients. But values (or normative goals) are only and perhaps not the most important consideration. There is a pragmatic issue at stake as well, for as organizations grow in size, as they increase their complexity and scope, and as they diversify and spread into sprawling behemoths, the problems of leadership, coordination, collaboration, and communication force themselves on our attention. Which leads to the following conclusion: Most knowledge utilization efforts have to do with maintaining the virtues of bureaucracy--its speed, precision, predictability, and efficiency--while trying to preserve an adaptability to change and a climate of creativity, personal growth, and satisfaction for the work force.

More specifically, an analysis of the such efforts usually includes all of the following general objectives:

1. To create an open, problem-solving climate throughout the organization.
2. To supplement the authority associated with role or status with the authority of knowledge and competence.
3. To locate decision-making and problem-solving responsibilities as close to the information sources as possible.
4. To build trust among individuals and groups throughout the organization.
5. To make competition more relevant to work goals and to maximize collaborative efforts.
6. To develop a reward system which recognizes both the achievement of the organization's mission (profits or service) and organizational development (growth of people.)

7. To increase the sense of "ownership" of organization objectives throughout the work force.
8. To help managers to manage according to relevant objectives rather than according to "past practices" or according to objectives which do not make sense for one's area of responsibility.
9. To increase self-control and self-direction for people within the organization.⁹

All of the above represent the normative goals of applied behavioral scientists and they are, more or less, recognized as such by those who practice and teach that craft. What also turns out to be true, based on my own informal content analysis of the OD and OB literature, is that those goals can be linked to certain conceptual referents, most favored and against which the work of OD is evaluated.

Using the book of readings, edited by Katz, Kahn, and Adams (1982) as a reliable guide to my conceptual search, it appears that the following are the major concepts in use: organizational characteristics; culture, role, and climate; performance/effectiveness; motivation; communication; leadership/decision-making; work/health; conflict/adaptation/change; and managing the environment. As the authors say in the introduction to their splendid book:

The study of organizations has many residences, but no single home. Its problems cut across a number of disciplines, both basic and applied, and make difficulty for the traditional separations between psychology, sociology, administrative science, public administration, social work, educational administration, and business management. (Katz, Kahn, & Adams, P. 1)

"Some fields," the authors continue, "can afford the luxury of long separations between science and practice. Organizational study cannot.... We need models that, while less general, include relationships between variables that are identifiable and measurable in ongoing

human organizations and that specify conditions under which such organizations must operate." (P. 2, emphasis mine)

That observation leads to an interesting assertion about OD: its phenomenal growth and its decreasing relevance to the specific "conditions under which...organizations must operate." Lets examine both sides of the paradox: OD's growth and its irrelevance.

Perhaps the most striking feature about OD has been its extraordinary growth over a period of 2½ decades, using as an arbitrary benchmark the first large-scale application at Exxon. Recently, Spier et al. (1980) published a survey of a national study of expert OD practitioners and researchers and pointed to the "astonishing growth" of the field based on the "exponential rise in the quantity of research and writing on OD." (P. 13) They report a "nearly sevenfold increase in citations observed during the Sixties over that observed during the Fifties.... During the Seventies there was a fourfold increase in relevant references over the number that appeared in the Sixties." (P. 14) The authors go on to say that "OD 'took off' during the Sixties and has been expanding ever since. To judge by our growing literature, OD is alive and well." (P. 15)

The authors of the study then go to mention "several surprises." They note that historians of OD have emphasized three roots of the field: laboratory training, action research and survey feedback. What they found was a spurt of OD-related laboratory training articles in the Sixties, almost no OD-related action research articles and, flying against their expectations, virtually no survey feedback articles. They also reported that the number of theoretical articles was also "surprisingly small." (P. 16) Even in the Seventies, theory articles

were ranked seventh, the lowest category. They were also "surprised" by the small number of references to socio-technical systems, as well as fewer articles than expected on the subject of conflict." (P. 16)

What their survey also showed was that the articles of the past ten years were characterized by an emphasis on the consultation process and attempts to capitalize on the popularity of OD by relating it to many types of training, a focus on management's role in the change process, and the widespread diffusion of OD into nonbusiness organizations.

I would add to this trend analysis one of my own which their findings reinforce: that the creative energies of the OD professional have been concentrated on the development of instruments and techniques which have found a profitable market to the relative neglect of theory development and its companion, research.

Equally ominous are the other trends reflected by the Delphi respondents in the study just cited. Their modal responses indicated a general lack of interest in "macro-system interventions," in the development of "OD technology for dealing with economic turbulence, for the development of OD applications on non-rational ways of knowing, for further integration of OD with traditional management training, human resource developments and personnel functions, in OD as a line manager's function." (P. 18-37) There was also a lot of disagreement as to whether OD would be responsive to problems of productivity and profits.

These findings raise the relevance issue: the lack of overlap between organizational concerns and the practice of OD; what appears to be the case--and the Delphi study seems consistent with this--is that the practice of OD has become more and more remote from the basic institutional dilemmas organizations are nowadays confronting.

The major issues of the past several years have been virtually ignored by OD. Take mergers and acquisitions as one convenient example. In the three years alone, between 1977 and 1979, over \$100 billion went into acquisitions. I suspect that the following three years easily doubled that amount. Other "hot" issues would include union-management relations (note the interest in and preoccupation with the air controllers' strike, the professional football and baseball players' strike), multi-national organizations and their relation to their host countries, plant closings, the growth of techno-information organizations and the need to redesign their organizational structures. I could go on. While there are a number of theoretical and research papers on these topics, there has been no discernable shift of OD practice to involve itself with these "operational" concerns Katz et al. referred to earlier. OD has not only lost touch with these concerns, thereby losing its touch, but has tended to overlook a more ominous change standing by.

I'm referring to the changes that have taken place and are taking place in the environment of organizations. So much has happened and so much of what we took for granted has been or will be challenged. Since the Kennedy assassination in 1963, we have been swept down a wicked slalom--through ghetto fires, the revolution of women and minorities, Viet Nam, Watergate, corporate crimes and all the rest. We began to learn in the late '60s that the nation cannot solve its social problems simply "by throwing money" at them. And we realized as well that space age technology--even though it did put men on the moon--was not a magic wand to wave away our worries. During the '70s, we learned that there are no quick fixes, instant cures or simple solutions to our most

troubling problems: debilitating inflation, spiralling energy costs, persistent environmental hazards, political instability abroad and deep-seated social ills at home, famine, and the threat of nuclear war.

The late H. L. Mencken, American author and critic, once observed: "There's an easy solution to every human problem--neat, plausible, and...wrong." Recent years have served to bear out that observation. Especially with respect to OD.

To be more specific, the context within which organizational behavior has evolved over the past two decades--coterminous with the growth of OD--has dramatically changed. Those responsible for governing the enterprise are spending more and more of their time managing external relations.¹⁰ All organizations are surrounded by an increasingly active, incessant environment--one that is becoming more compelling and dominant in all kinds of decisions which affect the institution.

Leadership (and its companion, decision-making) has become an increasingly intricate process of multilateral brokerage, including constituencies both within and without the organization. More and more of the decisions made are public decisions; that is, they affect people who insist on being heard. Organizations will have to reckon with the growing role of media as a "fourth arm" of government available for use by the people who oppose a particular decision and the people who support it. The idea of a relatively small group of "movers and shakers" who get things done is obsolete. Increasing numbers of citizens and stakeholders (and even those who are only indirectly involved in an issue) are interesting themselves in its outcome--and when the decision goes the "wrong way," very noisily so. This state of

affairs has led a colleague to describe the organization of today as "a jungle of close decisions, openly arrived at." (Cleveland, 1979)

The bigger the problem to be tackled, the more power is diffused and the more people have to be involved. Thus decisions become more and more complex, more and more specialized, affecting more and more different (sometimes conflicting) constituencies.

Inevitably there will be frustration, not only among leaders but among followers who ask: "Who's in charge here?" as more and more people/groups have to be consulted before a decision is reached. Which prompts leaders to ask: "How do you get everybody in the act and still get some action?"

The name of the game nowadays is ambiguity and surprise and organizations have to operate under uncertain, ill-defined and risky conditions, just as they are now becoming aware of the newer competencies needed to cope with the politicization of our institutions--by which I mean that our institutions are becoming the focus for a new kind of politics: mobilizing public opinion, working more closely with external--especially state, local and federal agencies and legislatures--shifting constituencies and demographics, and the character of their stakeholders.

No longer can "managing external relations" be left exclusively in the hands of the public affairs department. Top leadership, and OD practitioners, must be involved-directly. In short, the political role of organizations must be reconceived. These trends, these changing characteristics of the organizational environment that we are now living with, will become even more pronounced in the years ahead.

Now none of the above is new; the analysis of our society by pop-sociologists and others, revved up by the ubiquitous living room TV sets, has made the lay public all too aware of mega and mini trends. And, most certainly, OD practitioners are not insensitive to these sources. Indeed, a number of perceptive authors, including many of those responsible for chapters in this Handbook, have made significant contributions to the interactions between the organization and their turbulent environments. Fewer have tackled some of the issues mentioned earlier. And virtually all of the major organizational interventions stay with the mild and gentle interpersonal and human processual techniques.

The fact is that OD practice is locked into a Sixties model which continues to "sell" and, partly because of that receptive market, departs each day, more and more, from the vital functions of organizational existence.

The paradox raised earlier is easily resolved: the popularity of the "goods" is inversely related to their significance.

III

"Now what?"

I have come across men of letters, who have written history without taking part in public affairs, and politicians, who have only concerned themselves with producing events without thinking of describing them. I have observed that the first are always inclined to find general causes where the others, living in the midst of disconnected daily facts, are prone to imagine that everything is attributable to particular incidents, and that the wires they pull are same that move the world. It is to be presumed that they are both equally deceived.

Alexis de Tocqueville¹¹

From "A Fairly Concise Science Dictionary"

Knowledge n. Things you believe.

Theory n. System of ideas meant to explain something, chosen with a view to originality, controversialism, incomprehensibility, and how good it will look in print.

G. Storr (1983)

The concept of ambivalence in psychology refers to the experienced tendency of individuals to be pulled in psychologically opposed directions, as love and hate for the same person, as acceptance and rejection. The concept leads directly to distinctive problems: How is it that these opposed pressures persist? Why doesn't one or the other prevail? What psychic mechanisms are triggered by ambivalence? Such problems are not my concern here. My concern here stems from Tocqueville's remarkably sensitive observation--about those who pursue knowledge and truth and those who engage in action--and is with sociological, not psychological, ambivalence. Sociological ambivalence refers to incompatible normative expectations incorporated in a single role of a single social status. In the most extended and restricted sense, the ambivalence about which I speak is located in the social definition of roles and statuses, not in the feeling-state of one or another type of personality. To be sure, as one might expect, sociological ambivalence is one major source of psychological ambivalence. Equally so is that all who engage in the interstitial world of knowledge utilization are afflicted by a sociological ambivalence insofar as we occupy two domains, at least, that of seeker of truth and that of applier.

In the role of using knowledge on organizations, the already substantial degree of ambivalence is raised for two additional reasons. First, because, unlike an engineer or pharmacist or, even a surgeon, who

can dispense their knowledge without much, if any, human contact, those of us who practice OD must be deeply involved with our clients. Neutrality, despite pious disclaimers to the contrary by those who espouse a "blank screen" approach--a point of view, finally abandoned by undaunted Freudians--is impossible when profound human changes are at stake. The fact is that the classical realm of science, the underpinnings of which is best expressed by the Hemholtz School of science and its robust heir, logical positivism, is at odds with the messy, unwieldy, deeply human findings of the social sciences. In the former case, one can "do" science on their subjects; an applied behavioral science cannot subject its subjects to very much of anything. Its "subjects" must become co-investigators if the research is to have any meaning.

The second, more arguable factor, exacerbating the ambivalence, comes about because of the strong idealism most change agents bring to their task. Which is not to say that there aren't somewhere in the ranks a number of calloused, jaded types who ply their wares mechanistically; even those who do are not safely divided from their "other half" which clings steadfastly to Higher Ideals. To George Orwell, there was no higher ideal than the humanistic one and his inner tension between idealism and humanistic values led to his disdain of Gandhi. "The essence of being human," he wrote about Gandhi, "is that one does not seek perfection, that one is willing to commit sins for the sake of loyalty, that does not push asceticism to the point where it makes friendly intercourse impossible, and that one is prepared in the end to be defeated and broken up by life, which is the inevitable price

of fastening one's love upon other human individuals." (Malcolm, 1981, P. 80)

I suspect that those of us engaged in applying knowledge to organizations are all failed saints in that our practice and ideals are all too frequently are out of "sync."

The role-ambivalence of which I speak, finally, is deepened because there are essentially two strategies for truth gathering. One, the "exoteric" mode--exoteric is an esoteric word meaning "knowledge generated for the public interest" and the other, esoteric knowledge, means literally, knowledge produced for "one's learned colleagues." The exoteric mode springs out of the direct experience of immediate, intimate relationship to the sources of data, and the other is consciously more detached, socially disengaged, and remote. Most change agents and OD practitioners of virtually every stripe or persuasion were trained esoterically and have to practice exoterically. That is the major determinant of the inevitable ambivalence we must reckon with, if not resolve.

In the remainder of this chapter, I shall set forth some recommendations, first on the knowledge/research and then on the policy side, which will not quiet the stirrings of the role ambivalence of which I speak, but may, at least mediate it.

1. Knowledge/Research Recommendations

Our society cannot delay dealing with its major social problems. We cannot consume our resources and pollute our environment and then hope to replenish and restore them. We cannot permit international relations to deteriorate to the point of resorting to nuclear weapons. Social unrest, a result of rising expectations and frustrated hopes, will eventually reach a point of no return. The social sciences will provide no easy solutions in the near future, but they are our best hope, in the long run, for understanding our problems in depth

and for providing new means of lessening tensions and improving our common life.

(National Academy of Sciences, 1969, P. 17)

There is a fable, carefully nurtured over the centuries by the basic scientists, particularly those who see basic as pure, about the relation between the scientist who acquires information and the problem solver who applies that information. The fable is that scientists acquire the knowledge, that this knowledge goes into the public domain, and that when a problem solver needs some knowledge to solve his problem, he extracts it from the public domain, uttering words of gratitude as he does so, and solves his problem. The actuality that the scientist has provided knowledge needed by the problem solver occurs in some mysterious fashion. Mysterious though the process is, it is so effective that no tampering must be allowed, and in fact, the less contact the scientist has with the problems of the problem solver, the more apt he will be to fill the public domain with knowledge of ultimately greatest import to the problem solver. This is the fable, but like all fables, it is a myth. It does not work that way at all.

W. Garner (1972)

The above quotes bespeak another type of ambivalence based on our tradition that goes back to the Enlightenment--the faith that, just as the natural sciences lead to technology which will make us all healthier and wealthier, so the social sciences, if applied, can solve our social problems. Garner's demystification of this fable, reveals the thinking which underlies the spate of pronouncements about the necessity for drawing upon the social sciences to solve the social ills that plague the nation and the world. According to Weiss (1980, P. 13), the thinking goes something like this:

- "1. Social science research produces knowledge about human institutional behavior. Knowledge has connotations of fact, truth, and replicability.
- "2. Action based upon knowledge is more rational than action based on experience, judgment, or intuition. Rational connotes the apt fit of means to ends, an efficient use of resources, and an increase in the predictability of outcomes.

"3. Rational action by institutions will lead to good outcomes. Good means that the consequences of beneficial to society.

"4. The good effects of rational action by institutions will be shared uniformly and equitably by all groups in the society."

The fact is that it doesn't happen that way. We social scientists have not, as yet, come up with eternal truths and laws that are robust and good for all seasons or reasons. And since each advance we do make seems to uncover unsuspected complexities and new sources of variability, the quest for elegant and parsimonious laws of social behavior, on the model of the laws of physical sciences, may never be successful. The fact is that most of the speculation about knowledge utilization is based on biased hopes or hopeful biases. What follows now by way of recommendations on research is nowhere free of the same biases.

The intellectual task of developing a valid framework for an applied social (or behavioral) science is only beginning to be accepted by behavioral scientists and social practitioners, and we can take only the first step or two of the task here. But, given the conditions and problems spelled out by the various authors quoted above, we can sketch out--at least in broad outline--the desiderata of what can be called valid knowledge:

1) An interdisciplinary applied social science that takes into consideration the behavior (including attitudes, feelings, etc.) of persons operating within their specific institutional environments.

2) An applied social science capable of accounting for the inter-related levels (person or self, role, group, and macro-system), within the social-change context.

3) An applied social science that in specific situations can select from among variables those most appropriate to a specific local situation in terms of its values, ethics, and moralities.

4) An applied social science that in specific situations can select from among variables those most appropriate to a specific local situation in terms of its values, ethics, and moralities.

5) An applied social science that is pluralistically "real"; accepting the premise that groups and organizations as units are as amenable to empirical and analytical treatment as the individual.

6) An applied social science that can take into account "external" social processes of change as well as the interpersonal aspects of the collaborative process.

7) An applied social science that includes propositions susceptible to empirical test, focusing on the dynamics of change.

The above is only a "once over lightly" introduction to some of the elements required in a vigorous and viable applied social science. The horizon is distant and visible. What may bring the horizon somewhat closer is a consideration of some of the strategies of truth-gathering for an applied social science, some methodological considerations. There is no comprehensive answer available at the present time, but in order to develop useable knowledge, the following values (biases) should be taken into account in all action-research undertakings:

1) Research is a collaborative undertaking and can be enhanced by including members of the client system in the team effort.

2) The image of organization stems from a preference for observing process and change rather than order and continuity. Thus, it shouldn't be disconcerting to confront contradiction and conflict.

3) The researcher's most productive stance is curiosity and dissatisfaction with current paradigms for understanding organizational life.

4) Findings should be important--not just interesting--and demonstrable in terms of larger social relevance.

5) research reports should contain a vivid description of the experience of researching. "Values" should be squarely faced in these reports. Research should report not only the findings, but the questions raised by the research.

The following list, adapted from Shulamit (1979, P. 11-12), contrasts the traditional methodological model of the social sciences with an applied methodology:

Traditional Social Science Research Model	An Applied Social Science Model
---	---------------------------------

Rational, with emphasis on classifiability, generalizability, and predictability	A mix of rational, serendipitous and intuitive phenomena in research and analysis
Scientific	Accurate, also artistic
Oriented to carefully defined structures	Oriented to process
Impersonal, detached, remote from phenomena	Relational, interactive
Oriented to prediction/control	Oriented to understanding
Validity/replicability	Interested in relevance of findings to users and scholarly communities
Objective	Objective and personal knowledge
Capable of producing laws and generalizations	Capable of producing specific explanations

Emphasis on replicable events and procedures

Emphasis on the unique, though frequently re-occurring events

Capable of complete analyses

Capable of producing partial discoveries of ongoing events

Interested in addressing problems with predefined concepts, hypotheses

Interested in developing constructs stemming from direct field experience

The list is, itself, an exaggeration, but it should serve to underline a point alluded to earlier in connection with the McKelvey/Aldrich paper which, mistakenly, proposes that useable knowledge stems directly from the brow of the paradigm of the natural sciences.

2. Policy Recommendations

With so many valid ideas missing their mark, with social science articles (written in the foreign language of the professional social scientist) mildewing in inaccessible journals, and with policy makers ignorant or indifferent--if not antagonistic--to pivotal facts, it is inappropriate, if not dangerous, to be obsessed by the perils of closer cooperation between the "two-culture" split mentioned in the introduction of this chapter, between the realms of science and action. The ally of power is not necessarily the "servant of power." (Recently, for example, the California legislators, responsible for drafting new legislation on the control and rehabilitation of drug addicts said that their opinion was large formed by their friends, druggists, family doctors, and lobbies. They reported being unaware or antagonistic to the findings of the specialists/experts who have produced a prodigious literature on this issue.)

My specific recommendations, regarding policy issues, fall into six headings:¹²

1) Deepen and broaden mutual understanding between scientists and policy makers. What we must have if this increasing alienation between the two cultures is to be diminished and finally obliterated is an understanding by each of the other's system of values.

This is not limited to the social scientist. James Reston, writing in the New York Times (1969) advocated (in connection with Project Camelot):

The most creative minds in America on most of these questions are not in the government today, but in the universities, the foundation, and elsewhere in private life. They would respond to an appeal by Senator Fulbright to testify before the cameras and could in the process help bring about a much wider understanding in Congress and the nation if the problems facing the President.... The American experts and scholars on China have not been heard in open hearings on this subject since the Sino-Soviet split. What do the old China hands and the new Orientalists think of this convulsive new force in the world? It is not at all clear that the Executive or the Congress knows.

2) Develop the science of science utilization. It is not original or interesting to say that the work being done by social scientists is valuable and should be useful; nor is it particularly innovative to say that there should be more research in the social sciences because such research can produce helpful data. However, what few people have considered and what seems to merit increasing and vital attention is research on the utilization of knowledge. I believe that the social sciences must focus on the research area of knowledge utilization immediately; without such research, all data loses some of its potential effectiveness at the pace we are acquiring new knowledge. It is a horrifying waste of human and material resources not to incorporate what is being learned into our way of life.

3) The yield of social science must be loud and clear--and useful. To exercise influence and effect, social scientists must make

their achievements visible; if not quite an equivalent of the Manhattan Project, most certainly communicated and reified in a way that the public can get behind. Worth is often measured by tangible product.

4) The public must lend vigorous support to larger social science efforts. Before massive social science yields can be regularly produced, the public must invest in its future on a scale larger than anything we know of today. Research activity accomplishes many purposes aside from the main one of adding to the store of certified truth. It creates a bold, risk-taking culture. The hum of active research attracts brighter young people; it develops confidence in its various publics. The federal government is in the position to grant greater research funds than private or public foundation or university. yet a government which can grant--with very little soul-searching--billions of dollars for work on weapons systems grants haltingly and on a year-to-year basis precarious millions to the social sciences.

It is important to acknowledge that a good deal of progress has been made over the years with respect to federal attention to the financial needs of the social sciences--especially of the applied social sciences. Starting with the National Academy of Sciences Advisory Committee on Government Programs in the Behavioral Sciences in 1969, there has been a stream of encouraging reports (with less encouraging results) coming out of various governmental and professional association commissions.¹³

5) Social scientists must be social as well as scientific. The practice and vision of social science, too, is predominantly Victorian, having been nourished in the great European universities of the latter half of the nineteenth century. We see social scientists subject their

subjects to the damndest tricks, games, deceptions, tortures, to say nothing of psychological mayhem, with authoritarian detachment--as if subjects did not have intelligence, feelings, hypotheses, and expectations as well as some urges to subvert the whole experiment. (Argyris, 1980) There must always be understanding of the people with whom the social scientist works--especially given the methodological slant proposed above--whether they are subjects or clients. There must be a strong commitment and responsibility to the idea of collaboration and mutual benefit. Indeed, this attitude is not only appropriate and fitting to the scientific ethic, it is essential. Without trust and commitment to the research task, the data generated are often phony, stilted, and incomplete--if not downright misleading.

The solution to this problem is not completely within the grasp of the individual social scientist. Rather, it is in the realm of those institutions which educate Ph.D.s in the social sciences. It is astonishing that apprentice social scientist at no time in their graduate education obtain any formal instruction in one of the primary tasks, teaching. It is equally shocking that they receive no systematic practice or supervision in the human side of the research enterprise. Understanding these matters is not a flash of lightning or a divine gift. It is learned the hard way, through guided experiences.

6) Social scientists must re-examine and modify their own values. Social scientists must aim for and achieve to some degree complete honesty in their research. They must not attempt to conceal the motives or the sponsor of the research, since the eventual denouncement is inevitable (as we noted in Project Camelot) and can destroy the research beyond repair. Similarly, the sponsor must respect the social

scientists and honestly, thoroughly, and thoughtfully consider their objections, altering the plan of action if those criticisms are merited.

The recommendations proposed underlay a metaphysics for an applied social science. By way of conclusion and summary, I'd like now to make a final point.

We tend to think of applied social scientists as experts, analysts, advisors, specialists, consultants, theoreticians (at times), designers and sometimes merely temporary "help." For the most part the term is used to cover a myriad of relationships, many of which obscure and confuse the value which an applied social science exists to provide. I want to suggest that at its most impactful and professional level, an applied social science is profoundly important to what is occurring in the world today and is essential to fully realizing the potential which organizations represent for our lives. The context for that aspiration can be derived from the following two propositions:

1. Organizations are self-referencing systems and are thereby inherently lacking vision with respect to themselves. The role of applied social science is essentially to provide the possibility of an organization knowing itself, being visionary about itself.

2. Applied social science can be a major and very important force in the world today because the management of our human institutions is one of the most significant problems facing the world today and because, at its most professional and powerful level, an applied social science exists not as a "thing to do" or as a set of tools or techniques, but as a relationship between an organization and a body of knowledge.

As we develop more advanced theoretical and methodological skills, perhaps those of us who trade between the sciences and application will

be better able to empower our client organizations more successfully. With all of the disclaimers and problems and obstacles elucidated ad nauseum throughout this chapter, the truth is that the social sciences have made a difference. Change does take place via the appropriate application of the social sciences and through the efforts of OD. Organizations have enhanced the quality of work lives. Success stories outnumber the fright stories of which Camelot is a constant exemplar. It isn't always clear, nor do we fully understand the conditions under which the changes do take place, but certainly as the Katz et al. reader demonstrates (1982), a lot of positive gains have been made in applying knowledge to organizations.

Having started with the melancholy of Freud, it may be fitting to end with a quote from one of his heirs, a practicing psychoanalyst who, in talking about the divine mysteries of patient's positive changes says:

At the end of A Midsummer Night's Dream, the human characters wake up and rub their eyes and aren't sure what has happened to them. They have the feeling that a great deal has occurred--that things have somehow changed for the better, but they don't know what caused the change. Analysis is like that for many patients.

(Malcolm, P. 162)

Perhaps we are all the Pucks and the Oberons who make things happen, sometimes without even knowing why. Which in no way should diminish our need to understand, far better than we do now, how and why those remarkable things which occur to humans in the institutions with which we work, actually happen.

FOOTNOTES

1. Later on I will examine in some depth the effectiveness of "science" as an instrument of knowledge utilization; for now, I merely want to correct some errors in the McKelvey/Aldrich quote above. To begin with, the National Academy of Science does include a number of organizational scientists in its roster--Herbert A. Simon and James G. March are only two out of a total of ten (depending on how narrowly the term, "organizational scientist" is defined). And there have been best-sellers before Theory Z. Peter Drucker's Management and Robert Townsend's Up the Organization are two dated examples, but on the very day this page is being written, there are at least three others: John Naisbitt's Mega-Trends, Peter and Waterman's In Search of Excellence and Blanchard and Johnson's The One-Minute Manager--all with that dubious distinction.
2. These 5 references contain virtually the entire empirical base for all the generalizations contained herein. Administrative Science Quarterly, Special Issues, Parts 1 and 2, Janice M. Beyer (ed.), Dec. 1982, March 1983, Volumes 27 and 28, Numbers 4 and 1 respectively; The Planning of Change, Fourth Edition, W. Bennis, K. D. Benne, & R. Chin (eds.), Holt, Rinehart & Winston, N.Y., 1983; Putting Knowledge to Use, Human Interaction Research Inst. & N.I.M.H. (eds.), E. M. Glaser & H. R. Davis, 1976; R. G. Havelock, Knowledge Utilization and Dissemination: A Bibliography, University of Michigan, Inst. for Soc. Research, Ann Arbor, MI, 1972; Social Science Research and Decision-Making, C. H. Weiss with M. J. Bucuvalas, Columbia University Press, 1980.
3. Organizational change strategies would normally be included in any "thick description" of knowledge utilization but will only be peripheral in this chapter. See Nadler elsewhere in this volume.
4. These terms will be used interchangeably throughout this chapter.
5. Psychoanalysts have a difficult time--as do organizational researchers and change-agents--with clients when they are eager and change-desiring; enthusiasm for the "secondary gains" may provide strong forces against fundamental change. Freud also warned against "resistance via partial incorporation," a defense so real and inviting that it often eludes well-intentioned change agents.
6. In this section just as with others in this chapter, I will not cite all the sources for the generalization. The major references are those cited earlier, the 5 basic compendia on which all generalizations are inspired and supported.
7. Rush has provided a handy reference guide in his monograph on which this section is based. It is a glossary of OD terms from "Authenticity" to "Transactional Analysis" (1973, P. 64-68).

- 8 The acronym, OD, has been used throughout and will continue to be used henceforth as a portmanteau word, an umbrella term, that encompasses all those efforts whose general purpose it is to enhance organizational functioning through the application of knowledge. Those who are uncomfortable with that term should not be slighted; indeed, those practitioners of the policy sciences or Quality of Work Life or Socio-Technical Systems or whatever are not excluded from the scope of this chapter regardless of their different nomenclature.
9. I have found the work of Burke (1977, 1978) and Burke and Goodstein (1980) as well as Mirvis and Berg (1977) especially useful in writing this section.
 10. In my study of CEOs, they reported that the biggest change in their role has been "managing external relations." (1984)
 11. Quoted in Bennis (1973), from Tocqueville's Democracy in America, 1835.
 12. While the following is aimed especially at applied social scientists, the recommendations hold equal significance for other scientists who have a stake in the dissemination and use of their findings.
 13. Implementation still remains wanting. Ironically, the task force and commission reports get better as the problems get worse. In any case, for the interested reader, note National Academy of Sciences report (1969), the National Science Foundation Board report (1969), the President's Task Force on Science Policy (on which I served, 1970) and the following more recent articles: Lawler (1982), Fishman & Neiger (1982), and Tournatzky et al. (1982).

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TABLE 1
(K.D. Benne, 1976)

The Cognitive Worlds of Behavioral Scientists	The Cognitive Worlds of Social Practitioners and Action Leaders
<p>1. People and human systems which they study are not of interest as particular cases but as instances to confirm or disconfirm generalizations about people and human systems. Knowledge is organized around verbally (and/or mathematically) articulated generalizations.</p>	<p>1. People and human systems are clients or constituents. The social practitioner and action leader are concerned with particular cases, situations and practical difficulties in order to help, improve or change these. Knowledge is organized around kinds of cases, situations, and difficulties and takes the form of effective ways of diagnosing and handling them.</p>
<p>2. The occasion for inquiry is some gap or discrepancy in a theory or conceptual scheme. "Success" in inquiry is measured by attainment of more warrantable statements of variable relationships which fill the gap and/or obviate the discrepancy.</p>	<p>2. The occasion for inquiry is some difficulty in practice, some discrepancy between intended results and the observed-consequences of actions or excessive psychic and/or financial costs of established ways of working. "Success" in inquiry is measured by attainment of ways of making and/or doing which are more effective in fitting means to ends and/or in reducing costs of operation.</p>
<p>3. Scientists try in the course of their researches to reduce or to eliminate the influence of extraneous values (values other than "truth" value) from the processes of collecting data and determining and stating the meaning of the data within the research context. Their knowledge is relatively independent of the uses to which it may be put.</p>	<p>3. Practitioners and action leaders try to find and interpret data which enable them to serve the values which they are committed to serve-- "productivity," health, "learning (growth) and, in more political contexts, the "power," "freedom," and "welfare" of the "clients" or "constituents." Their knowledge is consciously related to use for some purpose or set of purposes.</p>

TABLE 1 (Continued)
 (K.D. Benne, 1976)

The Cognitive Worlds of Behavioral Scientists	The Cognitive Worlds of Social Practitioners and Action Leaders
<p>4. Scientists set up their researches to reduce the number of variables at work in the situations they study, by controlling the effect of other variables. Experimental results take the form of statements about the relationships of abstracted and quantified variables.</p>	<p>4. Practitioners and action leaders (like historians and anthropologists) work in field settings where multiple and interacting variables are at work. Their understanding of situations tends to be holistic and qualitative, though they may of course use quantitative methods in arriving at their "estimate of the situation." Unlike historians and anthropologists, they do not attend to all the variables involved in the full understanding of a situation but rather to variables which are thought to be influential and accessible to their manipulation in handling the situation in the service of their chosen values.</p>
<p>5. Time, in the form of pressing decisions, does not influence their judgments and choices so directly as it does those of practitioners. They can reserve judgment, waiting for the accumulating weight of evidence. A longer time perspective operates in their judgments of what needs to be done now and later. Their statements of what they know are more qualified, less impregnated with their own hunches and insights as to what incomplete evidence means for purposes of action.</p>	<p>5. Time presses the practitioner to decide and act-judgments cannot wait. He or she must judge in order to meet deadlines, whether the evidential basis for judgment is "complete" or not. They must depend on their own hunches and insights in attributing meaning to incomplete or contradictory evidence. Their knowledge is impregnated with their own hunches and values. It is more personal, more dependent on their own ability to read a situation than the more impersonal knowledge which the scientist professes and communicates.</p>

TABLE 2
(Beyer and Trice, 1982)

Correspondence between Components of Behavior, Organizational Processes,
Specific Behaviors Involved in Utilization Process in User Systems²

Components of Behavior Phase	Organizational Processes	Specific Behaviors Involved in Utilization Processes	
		Adoption Phase	Implementation
Cognitions	Information Processes	Sensing, seach	Diffusion
Feelings	Affective bonding	Affective reactions	Receptivity, Commitment
Choices	Strategy formulation & control	Selections	Evaluation,
Feedback			
Actions	Action generation	Adoption	Use, institutionalization

TABLE 3

Factors Influencing the Likelihood of Adoption or Adaption of a
Seemingly Promising Innovation by an Organization:
Integrated Findings

H. Davis (8 Factors)	E.M. Glaser (20 Factors)	G. Zaltman et al. (Condensation of 19 Factors)	R. Havelock et al. (10 Factors)
Ability to carry out the change	Capability and resources	Financial and Social costs	Structuring Capacity
Values or self-expectancy	Compatibility	Compatibility Publicness vs. Privatness Impact on interpersonal relations	Homophily Empathy
Idea or information about the qualities of the innovation	Credibility Ease in understanding and installation Observability Triability Divisibility Reversibility	Communicability Divisibility Reversibility Complexity of concept or implementation Susceptibility to successive modifications Scientific status Point of origin Terminality	Openness
Circumstances which prevail at the time	Willingness to entertain challenge A climate of trust Structural re-organization		Proximity
Timing or readiness for consideration of the idea	Sensitivity to context factors Early involvement of potential users Suitable timing		Linkage Synergy

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Seemingly Promising Innovation by an Organization:
Integrated Findings

H. Davis (8 Factors)	E.M. Glaser (20 Factors)	G. Zaltman et al. (Condensation of 19 Factors)	R. Havelock et al. (10 Factors)
Obligation, or felt need to deal with a particular problem	Relevance Widespread felt need to correct un- desirable conditions Shared interest in solving recog- nized problems	Degree of commitment	Energy
Resistance or inhibiting factors	Skill in working through resis- tances	Risk or uncer- tainty of various kinds Number of gate- keepers or approval channels	
Yield, or perceived prospect of payoff for adoption	Relative advan- tage An incentive system	Efficiency of innovation Perceived rela- tive advantage Gateway to other innovations	Reward

TABLE 4
Stages of Successful Organizational Change

Author	Date	Concern: Aware- ness	Diagno- sis: Know- ledge Search	Consider- ation of Alter- natives	Action: Implemen- tation	Follow- through: Evaluation
Lippitt et al.	1958	Need for change Get con- sultant	Clarifi- cation	Examina- tion of alter- natives	Actual change	Stabilize
Jenkins	1962		Analyze	Determine	Make the change	Stabilize
Jung, Lippitt	1966	Identify concern	Diagnosis	Retrieve relevant knowledge Formulate alterna- tives Determine feasi- bility (tests)	Adopt the innovation	Diffusion
Watson	1967	Sensing problem	Diagnos- ing	Inventing possible solutions Comparing Weighing Deciding	Implementing	Evaluating Revising
Greiner	1967	Pressures Arousal Interven- tion Reorien- tation	Dianosis	Specific problems Invention Commitment	Experiment	Search for results Reinforcement Acceptance
Rubin	1968		Diagnosis	Alterna- tive Selection	Strategy situation Action Initiate Install	Support transition link to permanent system