

EDITOR'S NOTE: The following article was drafted in the context of a two-part conference at the Harvard Business School on the topic of organizational behavior (OB) in the 1990s. At the initial meeting, participants presented their own thoughts about where the field of OB was and where it needed to go. In the following essay, the agenda proposed by the author, Karl Weick, appears as an appendix. At the second meeting, reactions to themes in the agendas were developed. One such theme was "Different Approaches to Research." Discussion of this theme was anchored by two quite different approaches, Paul Lawrence speaking on behalf of a problem-focused approach and Karl Weick speaking on behalf of a theory-focused approach. Paul Lawrence's statement appeared in the Vol. 1, No. 2 issue of *Journal of Management Inquiry* and a contrasting position is stated below. The stimulus for this contrast was Harrison White's observation at the first meeting that Weick's agenda sounded like it came from social science on the East Bank of the Charles River, while Lawrence's sounded like it came from the business school on the West Bank. This is the comparison to which Weick refers in the second sentence of his article. —Craig Lundberg

Agenda Setting in Organizational Behavior A Theory-Focused Approach

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Theory-focused research, portrayed as an exercise in sense making that is driven by curiosity rather than compassion, outsider rather than insider views, divergent rather than convergent thinking, and knowledge growth by extension rather than intention, is contrasted with problem-focused research. A research agenda for organizational behavior, written from a theory-focused perspective, illustrates how the perspective is actualized.

When I was asked in March 1990 to construct an organizational behavior (OB) research agenda for the 1990s, the document I produced (see appendix) looked quite different from the one Paul Lawrence produced. That difference was summarized by Harrison White as the difference between the east bank of the Charles River at Harvard, where arts and science thinking predominates, and the west bank where business school thinking predominates. Harrison restated the contrast as

one between a theory-focused approach to research and a problem-focused approach to research.

The purpose of this article is to flesh out one side of that contrast. I will try to highlight how I work, treating it as an idiosyncratic example of an approach that is driven more by existing theories than by existing problems. Lawrence and I both entered into this comparative exercise in agreement that any effort at agenda setting, such as the gathering at Harvard in December 1990, becomes more meaningful if people

are more self-reflective about, more honest in appraising, and more deliberate in choosing the approaches they use to make sense of the world. Approaches have strengths and weaknesses, they are overdetermined, and they can sometimes turn on their purveyors and mock their efforts to gain understanding.

My attempt to provide a compact, personally grounded description of a theory-oriented approach is divided into three parts. These include (a) the understanding of theory that drives my approach, (b) the activity of choosing problems, and (c) alternative ways to contrast how Lawrence and I work. Advantages and disadvantages of a theory-based approach will be evident throughout.

THE UNDERSTANDING OF THEORY THAT DRIVES MY APPROACH

Although the ways in which I understand theory are most evident in my previous discussions of theorizing about organizational communication (Weick, 1987) and theory construction as disciplined imagination (Weick, 1989), two quotations anticipate the main points I want to make about theory itself.

The first quotation, which appears in Kaplan's (1964) influential *Conduct of Inquiry*, reminds readers that, "theory is of practice, and must stand or fall with its practicality, provided only that the mode and contexts of its application are suitably specified" (p. 296). Although that statement may seem to make trouble for a theory-based approach, I don't think it does at all. Business organizations are certainly not the only settings where practice occurs, nor are they the only place where practicality is an operative criterion. Instead, all theories are about practice and practicality, and the trick is to discover those settings and conditions under which they hold true. This is the crucial insight in McGuire's (1983) work on contextualism, and it is implicit in the final phrase from Kaplan, which counsels investigators to specify those modes and contexts where the theory does apply.

A theory-based approach is often a continuing effort to find those contexts where a theory holds true. The question is not, Is the theory true? It is. All theories are true. Everything possible to be believed is an image of truth (McGuire, 1985, p. 575). The question is, Where is the theory true? In what context do events go together the way the theory predicts?

The second quotation elaborates what I think a theory-based approach involves. It is Peter Vaill's (per-

sonal communication, 7/2/75) description of art as "the attempt to wrest coherence and meaning out of more reality than we ordinarily deal with." Theory has too seldom been treated as an art form, in the provocative ways suggested by Nisbet (1962). That alone is enough to keep Vaill's statement exactly as it is. However, I want to make that implication explicit by substituting the word *theory* for the word *art*, so the revised statement reads, "theory is the attempt to wrest coherence and meaning out of more reality than we ordinarily deal with."

To me, the goal of a theory-based approach is to find patterns that edit particulars into a more compact summary that allows people (including theorists) to anticipate and thread their way through the complexities of everyday social life. For example, the summary statement, "people act their way into meaning," comes in part from my observation of the pattern in which people who agree to work on an assignment, with some reservations, act intensely to create an interesting assignment that justifies their compliance and makes it a meaningful act (Weick, 1964).

For me, the particular is a pretext to look for a pattern that is more generalizable, more abstract, something that applies to people in general (e.g., Meehl, 1972). This baldly instrumental treatment of the particular makes sense if you assume that people are all pretty much alike (Marceil, 1977). By contrast, I suspect that problem-focused research doesn't look on the particular as a pretext for anything and is much more deliberate in its treatment of the particular as a unique end in itself. In problem-focused work, the particular is a context rather than a pretext and consists of a self-contained story, tied together by its own logic. Different logics will generate different stories, and different stories will suggest different remedies.

The view of theory that lies behind my attraction to these two quotations, can be summarized as follows. Theories abbreviate stories and get rid of data (I am indebted to John Van Maanen for this imagery). People differ passionately in their reactions to what is discarded from the stories and what is retained. Furthermore, some stories are so compelling, so plausible, so narratively rational, that the inevitable editing produced by theory would set back understanding. Exemplary problem-focused research can produce narrative particulars that embody the universal.

But when stories are incomplete and something is missing, we need theories to fill those gaps. So, we tend to find the best theories where we have the worst stories, and vice versa. Said differently, theory-focused

research is an exercise in sense making (Weick, 1989, p. 519). For example, the story of airline deregulation is a puzzling incomplete saga until a theorist such as Bob Cole speculates that the history of airline deregulation recapitulates the history of capitalism (personal communication, June 1988). That suggestion turns an incomplete rendering of something concrete into a more complete rendering of something abstract. And it guides the curious listener back to the concrete events of airline deregulation to see whether they are consistent or inconsistent with the abstraction.

That exercise in abduction (Scheff, 1990) is something that Lawrence and I both do. Where we differ is that I am in no hurry to move from the abstract to the concrete, and when I do, a little concreteness (e.g., one lab study) goes a long way. I suspect that Lawrence has the opposite set of preferences, namely, a little abstraction goes a long way, and he prefers to spend his time close to the phenomenon rather than close to the library.

A brief gloss of the distinction between map and territory (Bateson, 1979; Korzybski, 1958) completes my description of the view of theorizing that drives my work. Theory work is language work and consists in part of efforts to refine and enrich the representations that people impose on territories to make sense of them. Viewed as language work, theorizing classifies, proposes analogies, codifies everyday understanding, and discredits synonyms. Theories partition the world into mechanistic and organic systems. Theories propose that interaction resembles exchange, that a loosely coupled system resembles baseball, that organizations resemble populations. Theories propose new maps such as "normal accidents," which then single out features that had previously gone unnoticed. Theories show that the construction and creation of reality are not synonymous, and to treat them as such is to miss the core of social sense making (Isaac, 1990, p. 26).

If theorizing about organizations is an exercise in cartography, then it is important to remember that there is no one best map; there are many different projections, and it makes no sense to try to discover the preexisting map. Furthermore, if we cannot achieve the one true picture of the world, we must also face the even more unsettling possibility that we can never even be sure if we're closer to it or not. Theories of organizations not only represent the territory but, when taken seriously, may enact and construct the territory (MacIntyre, 1985). In the case of organizations, if the map is not the territory, that may be the fault of the territory and not the map, which inverts the usual understanding of this phrase.

CHOICE OF THE PROBLEM

This section was triggered by Lawrence's statement that he defines problems as areas where human failures, discomforts, and pain occur, with his interest being to learn how human affairs can work better. I certainly do not disagree with that statement or its values, but I am not driven by the same imperatives. Pain, compassion, and betterment are less central as drivers for me, and more central are such things as incompleteness, novelty, counterintuitive implications, puzzlement, and fascination. Those triggers seem to be more cognitive, more internal, more tied to existing explanations, and more comparative than is true for Lawrence. To begin working, I do not need a problem. All I need is some kind of difference, something that attracts attention. An interesting phrase (e.g., "small wins"), a diagram (Maruyama's cause map of sanitation), an inversion (e.g., what if people accomplish impossible tasks because of loose coupling, not in spite of it), a puzzle (e.g., how could two 747 airliners collide on the ground with catastrophic results), a borrowed category (e.g., narrative rationality)—any of these inputs, when glossed, enlarged, instantiated, and tied into existing theories of attention, postdecisional behavior, systems, and interaction, become for me what a problem becomes for Lawrence.

My trigger is "what if," not "now what?" Lawrence's impetus to begin a study is the question, What does the world find troubling? My impetus to begin a study is the question, What do I find interesting? We seem to differ partly because my internal world is a more conspicuous source of questions than is true for Lawrence, who pays more attention to things around him. We also seem to differ because curiosity is more central for me, whereas compassion is more central for Lawrence.

This latter contrast between problem choice based on curiosity versus problem choice based on compassion paves the way for us to consider the question of problem choice more systematically. Webb (1961) argued that investigators routinely choose problems on the basis of six criteria, none of which are as sound or as significant as the three additional criteria of knowledge (choose problems in areas where you have a thorough understanding), dissatisfaction (choose problems that reflect a healthy, active opposition to existing knowledge and methodology), and generalizability (choose variables and situations that are universal and common rather than unique and rare).

The six criteria of problem choice that people commonly use are these:

1. *Curiosity*: Am I interested? Curiosity is a charming urge but no guarantee of nontrivial problem choice.
2. *Confirmability*: Can I get an answer? Confirmability precludes choice of mysterious new areas and is the criterion most likely to result in pedestrian problems.
3. *Costs*: How much? Cost is uncorrelated with value.
4. *Compassion*: Will it help? Webb (1961) cites the following passage from H. G. Wells book *Meanwhile*: " 'The disease of cancer will be banished from life by calm, unhurrying, persistent men and women working with every shiver of feeling controlled and suppressed in hospitals and in laboratories . . . Pity never made a good doctor, love never made a good poet, desire for service never made a discovery' " (p. 225).
5. *Cupidity*: What's the payoff for me? What is good for your promotion or publicity is not necessarily good for others.
6. *Conformity*: What is the current hot topic? Some things people are currently doing are quite worthwhile and some are quite ridiculous.

These criteria, plus those of knowledge, dissatisfaction, and generalizability (Webb, 1961, p. 225), can be used in several ways to distinguish between a problem-focused and theory-focused approach. I have already suggested that compassion may be a more central basis of problem choice in problem-focused research and that curiosity may be more influential when theory-based research is involved (e.g., in my case the curiosity is reflected in questions such as, What would happen if ethnomethodology were applied to organizational theory, if cultural transmission were studied in the laboratory using the common target game, if Bartlett's serial reproduction task were run in reverse?).

I suspect that both Lawrence and myself are equally driven by knowledge and dissatisfaction, although Lawrence's knowledge and dissatisfaction are focused on the world and mine are focused on theory. Generalizability is of primary importance for me and possibly less central for Lawrence. The remaining four criteria—confirmability, cost, cupidity, conformity—seem of lesser salience for both of us.

The point is that people who focus on problems and on theories may choose the things they do for different reasons. The word *choose*, however, may mislead because it suggests a more formal and deliberate process than that which actually operates. Ziman (1987) suggests that the process operates at the level of rationality in everyday life (p. 97), and often "may be no more than a serendipitous intimation that an unconsidered problem, with a possibly significant solution, lies just

behind some curious observation or anomaly" (p. 96). Furthermore, it is crucial to remember that the implied sequence of choosing a problem to do research, is often reversed because the process of research usually becomes the occasion to clarify, formulate, and reformulate the question being investigated.

Having made an effort to talk about problem choice more systematically, I want to comment briefly on biography and problem choice, because I suspect that personal histories and experiences have more impact on problem choice than do more distal categories such as theory and problem. Problem choice is expressive behavior, and we need to reflect on this quality when we think about agendas, because this personal backdrop will affect what we stick to and what we will avoid.

I have been struck, for example, by the parallels between my own style of theory-focused work and the style attributed to people with the Myers-Briggs pattern INTJ (introvert/intuitive/thinking/judging), a parallel that is all the more interesting to me because that is exactly the profile I have.

Mitroff and Kilmann (1978) characterize people with this profile as "conceptual theorists," and relative strengths and weaknesses can be inferred from comparison with the other three basic profiles (analytic scientist, who is sensation/thinking; conceptual humanist, who is intuitive/feeling; particular humanist, who is sensation/feeling). For present purposes, it is sufficient to note their description of a conceptual theorist as someone who prefers to construct bridges between paradigms rather than work within a single, well-defined paradigm; someone who uses paradigms as stimulants to conceptual imagination; and someone who explores large-scale differences between hypothetical representations rather than the details of any single schema. "A potential danger of the AS [analytic scientist] is getting bogged down in infinite details; a potential danger of the CT [conceptual theorist] is ignoring them altogether for the sake of comprehensiveness" (p. 68).

But the paragraph I found most interesting, especially because I uncovered it *after* I wrote all of the text up to this point in the article, is the following one:

Apparently, the CT is a speculative theorist who deeply values broad-ranging novel ideas, and who does not demand that these ideas be tied down to "reality" in the sense of being verified by accepted theories or facts. Indeed, the CT often prefers to challenge known facts and ideas, if only for the sake of speculative argument. Above all, the CT values the creation of novel conceptual possibilities, schemata,

and hypotheses which allow us to revise, rethink, and challenge even the most firmly entrenched and accepted ideas. (Mitroff & Kilmann, 1978, p. 55)

That paragraph is consistent with several themes in the previous text. Among these are the variety of theories I've used, the modest constraint on those theories exerted by existing data, the fact that the work plays off accepted ideas rather than existing problems, the assumption that all ideas are valid somewhere and that the task of theory is to find that somewhere, and the preoccupation with novel conceptual possibilities and hypotheses. Earlier, I mentioned that a little data goes a long way, which is what one might expect from someone who thinks in terms of possibilities (e.g., affirmation as inquiry), imagined worlds (e.g., reverse simulation), and challenges (e.g., amendments to organizational theory). The danger of this style is obvious: "AS science without CT science is narrow, parochial, and confining reducing a potentially creative activity to a set of fixed rules and rituals—ultimately dogmatic scientism of the worst form. CT science without AS science can become merely fanciful speculation" (Mitroff & Kilmann, 1978, p. 61).

I suspect also that personal biography affects not only the form of inquiry but also its content. In thinking about why I chose to study psychology, and what personal question I might be trying to answer by this choice of field, I was struck by the frequency with which I seem to study what happens when people don't understand what is going on. My proximate concern is not *déjà vu* (I've already experienced that), but rather *vujà dé* (I have never been here before and I have no idea where I am). Consider the evidence. I study interpretation, sense making, equivocality, stress, dissonance, and crisis behavior, all of which are associated with the question, What is going on here? I advise people to become more complicated so they can sense more of the complications in their worlds. I sound like a cartographer *manqué* because I keep looking for maps, and my favorite story is one in which people find their way out of the Alps using a map of the Pyrennes. I define technology as material relations that exceed human comprehension. Seemingly ineffective organizational acts such as "hypocrisy," "ambivalence," and "galumphing" surprise us by having unexpected benefits. Small stuff can be satisfying and sensible. And events only make sense after the fact.

It is all well and good to say I'm interested in how people cope with complexity, but that interest is anything but idle curiosity. Apparently, I never survived

my first encounter with bewilderment, and my professional life represents one long Ziegarnik effect to gain some closure on that raw, open-ended initial experience.

Small wonder that I keep looking for ideas, tactics, and determinants of sensemaking at a microlevel of analysis. My work is no less problem-focused than is Lawrence's. It's just that the problems I focus on are more private, closer in, and more hidden by theory-based overlays. But the question, what does it all mean, is certainly not my question alone, which explains why I feel my analyses have generality. Also, because the problem is close in, I also have no shortage of data. Everything I do is empirical. However, I am also deeply mindful of the sentiment attributed to Freud that the only trouble with self-analysis is countertransference.

So what's the point? A theory-based approach, as I practice it, is triggered by incompleteness and puzzles in theory rather than by injustices and pain in people. A concern with theory in no way precludes the latter. It's just that my instantiation of a theory-based approach gives less prominence to injustice and pain than other theorists might. The work is driven more by curiosity than by compassion. And yet, when I look for patterns in actual problems I chose to work on, the work converges on a theme that perpetually churns me as a person—namely, What is going on? And my recurrent answer is, You'll know once you act.

No big deal. But then, as Bergson said, " 'a true philosopher says only one thing in his life-time, because he enjoys but one contact with the real' " (cited in Wagner, 1983, p. 115).

ALTERNATIVE WAYS TO CONTRAST THE TWO APPROACHES

Although there are clear differences between the approaches that Lawrence and I exemplify, I am not convinced that the contrast is best labeled problem focused versus theory focused. Having just concluded the previous section with the suggestion that my theorizing is driven by personal concerns with sense making, I can cite other examples of my work that are problem oriented just as Paul can cite examples of his work that are theory oriented. Among the problems I have addressed are why did the Tenerife disaster occur (Weick, 1990a); what does it mean to have a career in an organization with no hierarchy (Weick & Berlinger, 1989); what are the costs of coping with overload on military battlefields (Weick, 1985b); how can schools

Mode	Primary Purpose of Knowledge-Yielding Activity		Role of Researcher
	Organizational Action	Organizational Inquiry	
<p style="text-align: center;">From the Inside</p> <p style="text-align: center;">↑</p> <p style="text-align: center;">↓</p> <p style="text-align: center;">From the Outside</p>	Coping Action taking Managing Surviving	Situational learning Action research Clinical practice Case research	Organizational actor ↑ Participant observer ↓ Unobtrusive observer ↓ Empiricist ↓ Data analyst ↓ Rationalistic model builder
	Organizational design and engineering Controlled experimentation Social technology	Traditional positivistic science	

Figure 1: **Alternative Modes of Inquiry**
 Source: Evered and Louis (1981). Reprinted by permission.

become more responsive to local conditions (Weick, 1982); and why are computer-driven analyses misleading (Weick, 1985a)? We all work with theories and problems, and cycle between them, although we seem also to linger at one or the other of these poles and hurry to get back to the pole we find more attractive. Theories have problems just as problems have theories.

I think the contrast between our two approaches can be understood more meaningfully using other dimensions than problem versus theory. I want to suggest four possibilities briefly: Myers-Briggs Type Indicator (MBTI), insider-outsider, divergent-convergent, extension-intention.

If a theory-based approach involves an intuitive-thinking style, then the opposite of this style is sensation-feeling or a particular humanist in the categories of Mitroff and Kilmann (1978). Although they repeatedly contrast the conceptual theorist with the analytic scientist, the difference between those two styles is relatively minor, consisting largely of a preference for sensation or imagination. A more significant contrast occurs if a preference for thinking versus feeling as ways to process data is added. Recall that compassion versus curiosity seemed to distinguish sharply between the problems that Lawrence and I chose to study. Thus if it were the case that Lawrence's type is sensation-feeling (SF) then the contrast between our approaches would be more substantial than if he were any one of the other three types—intuitive-feeling

(NF), intuitive-thinking (NT), or sensation-thinking (ST). In any case, adding this dimension to the theory-problem difference seems to suggest more about our distinctive approaches to agenda setting than does use of those global categories alone.

The same enrichment of the contrast seems to occur if we pose our differences as those between an insider (Lawrence) and an outsider (Weick). Debate over the relative merits of perspectives from the inside or the outside, and over whether outside is even a perspective, have raged for some time (e.g., Merton, 1972). The rendering of these debates by Evered and Louis (1981) into Figures 1 and 2, summarizes the contrast between the detachment of my approach and the immersion of Lawrence's style.

The distinction between divergent and convergent research styles has been made by several investigators (e.g., Hudson, 1966), but McGuire's (1985, p. 564) recent attempt to contrast the convergent style of Carl Hovland with the divergent style of Leon Festinger provides at least half a distinction between Lawrence and myself, because I was schooled in and continue to use many of Festinger's ideas.

Several distinctions in McGuire reflect previous content. The divergent style is driven by theory rather than the phenomenon, a difference we have tracked from the start, but what McGuire adds are such additional nuances as (a) the theory is applied to as many phenomena as possible (e.g., commitment is organiz-

Dimension of Difference	Mode of Inquiry	
	From the Outside	From the Inside
Researcher's relationship to setting	Detachment, neutrality	←→ "Being there," immersion
Validation basis	Measurement and logic	←→ Experiential
Researcher's role	Onlooker	←→ Actor
Source of categories	A priori	←→ Interactively emergent
Aim of inquiry	Universality and generalizability	←→ Situational relevance
Type of knowledge acquired	Universal, nomothetic: <i>theoria</i>	←→ Particular, idiographic: <i>praxis</i>
Nature of data and meaning	Factual, context free	←→ Interpreted, contextually embedded

Figure 2: Differences Between the Two Modes of Inquiry

Source: Evered and Louis (1981). Reprinted by permission.

ing), (b) more care is taken in defining the independent variable than the dependent variable (e.g., I worry about the subtle difference between ambiguity, uncertainty, and equivocality, whereas Lawrence worries about subtle differences in measures of effectiveness), (c) small *Ns* predominate (e.g., a little data goes a long way), and (d) interaction effects are trouble (e.g., general simple explanations are my goal). Although McGuire's distinctions are narrowly drawn, because they are phrased in terms of laboratory methodology, they hint at patterns that should be found across more diverse methodologies.

The distinction between knowledge growth by extension (Weick) and intention (Lawrence) is Kaplan's (1964, p. 305), and suggests a difference in approach that often may be confounded with the theory-problem distinction. Knowledge growth by extension occurs when a relatively full explanation of a small region is carried over to an explanation of adjoining regions, as when studies of conditioning are enlarged into studies of more complex learning. The metaphor is of science as a mosaic that is built piece by piece. It is exemplified when I take small understandings of commitment, minimal social situations, jazz orchestra rehearsals, or knot tying under stressful conditions and then complicate those understandings so they generalize to other settings.

Lawrence, however, seems to develop a partial explanation of a whole region and then makes it more and more adequate, a pattern that is labeled "knowledge growth by intention" and is visible in the work

of Freud and Darwin. The metaphor is one of bringing things into sharper focus or gradually adding light to a darkened room. It is this contrast that is perhaps most visible in our differing agendas. I tend to enlarge the small things we know well, Lawrence tends to refine the large things we know poorly.

Although other contrasts could be cited (e.g., Lawrence strives for accurate-simple explanations, whereas I strive for general-simple explanations), the point is that our two approaches are a good deal more subtle and complex in their differences than is conveyed by a simple contrast between a theory-focused approach and a problem-focused approach. I would argue that whatever merits these approaches have, whatever hit rates they generate, whatever interest they generate in readers, and whatever help they give theorists and practitioners lies in those subtleties. And it is the complexities of each of the two approaches that enable both of us to sense some of the complexities that surround us.

The question for research agendas in the 1990s is, Are current research approaches sufficiently subtle and complex to sense and make sensible the subtleties and complexities of organized social action in an increasingly interdependent and turbulent global environment? The answer seems obvious. No one pattern of subtlety and complexity is anywhere near sufficient. The only way we can generate the requisite complexity to grasp the complexity that surrounds us is to become an inquiring community in which there is respectful complementarity, integration, listening to

one another, an ethic of mutual helpfulness, and non-stop conversation. I know that sounds bland, difficult, perhaps even maudlin. But I'm not convinced that chronic ethnocentric conflict between warring paradigms has done much to help us consolidate organizational studies into a shared body of knowledge. We need to be subtle and complex in diverse ways if we want to comprehend significant differences in organizational life. But we also need to match this differentiation with the integration that comes when we articulate connections, themes, and patterns that tie those differences into coherent, memorable guidelines.

An agenda that simultaneously strengthens the community, differentiates the phenomenon, and integrates the representations is what I think we need to work toward. Neither Lawrence nor I were able to craft agendas that fully meet these requirements, but our approximations and the way we arrived at them, provide at least a starting point for others to articulate the approaches and trade-offs they feel will advance the field and coalesce the community.

APPENDIX

Notes Toward an Organizational Behavior Research Agenda

To grapple with a research agenda, is to revisit tacitly, a host of general issues we have been stuck with for years, issues such as the following:

1. What does cumulation mean in a low-consensus discipline?
2. If research findings are incorporated into everyday organizational life, thereby continuously altering that life, then does the size of our uncertainty remain constant while the location of that uncertainty shifts (see MacIntyre, 1985)?
3. Is there something qualitatively different when we move from behavior to organizational behavior? (e.g., if people take organizational roles and do so publicly and volitionally, does that alter their character permanently?).
4. Does organizational behavior (OB) need new concepts, or does it need to understand more fully and pursue with more imagination and commitment the concepts it already has?
5. If there is "fatigue of the spirit" (Weick, 1990a) in OB, is the remedy to beef up (improve data gathering and data analysis techniques), grow up (improve training, weed out careerism, and discourage the pursuit of style over substance), or give up (contribute at the margin with the realization that collective order is transient, local, and situational)? These three options have been discussed by Foddy and Thorngate (1978), but there may be other ones.

Although issues such as these five may bubble up in some form as we discuss specific items for an agenda, they are probably addressed with most sensitivity in the context of substantive topics. Therefore, the remainder of this effort to "prime the pump," both for myself and for the others attending, consists of a short list of things I think we know and a longer list of things I think we need to know. The latter list is a first approximation of an agenda. Although lists are decidedly more cryptic than are narratives, limitations of length and time for this initial paper dictate the use of the more cryptic voice. The joy of the meeting in March will lie in the fleshing out of the richer, more diverse associations we each had to the hints that were present in the individual papers.

Things We Know

There seem to be several distinctions that we are able to make with some certainty and that serve as a set of assumptions from which we move to a newer set of questions that we think need greater attention. Among my nominations for "anchors" are the following:

1. Expectations influence everything (e.g., self-fulfilling prophecies; ideology as discussed by Meyer, 1982).
2. Postdecision processes differ from predecision processes (e.g., Brickman, 1987).
3. Differentiation/integration are central dynamics in individual as well as in collective activity (e.g., Zajonc, 1960).
4. People able to reduce uncertainty surrounding important strategic contingencies gain power (e.g., Hall, 1984).
5. Classification of organizations along a continuum ranging from mechanistic to organic, provides a rough guideline regarding what other things we might expect to observe (e.g., Tichy, 1981).
6. The Yerkes-Dodson law is a plausible starting point to use in the analysis of perception, thinking, and action under conditions of intense affect (Weick, 1990c).
7. Social influence processes, especially conformity, unfold in some form in most social settings (e.g., Moscovici, 1985).
8. Uncertainty absorption is a component of all communication transmission (Campbell, 1958).
9. Technology makes a difference in organizational functioning (e.g., Perrow, 1967).
10. Knowledge structures such as frames of reference prefigure what people report they perceive (Shrivastava & Mitroff, 1984).
11. An evolutionary epistemology underlies most organizational change (Campbell, 1969).
12. Retrospective processes of cognition differ from prospective processes of cognition (e.g., O'Reilly & Caldwell, 1981).
13. Structure consists of a specification of roles that provides a crude coordination of action, and fine-tuning of the coordination requires discretionary action (e.g., Blau & Alba, 1982).

Although this sampler is fragmented and does not derive from a single paradigm, it does indicate some starting points

from which coherent lines of inquiry, having high relevance to organizational studies, can be constructed. That is progress worth affirming.

Things We Need to Know

Again, through the less than satisfactory vehicle of listing, I want to suggest several candidates for an agenda in the 1990s. These lists have been grouped into four categories: concepts, psychological, process, and structure.

Concepts

Although most of what I discuss in this note might be listed here, I have confined myself to just two specific items.

1. *Differentiate the Boulding levels of systems.* Boulding's (1956) influential framework describes increasing degrees of system complexity ranging from clockworks to symbol systems. With the growing sophistication in discourse analysis and information technology, his differentiations seem too crude and to gloss over differences that may make a difference. This agenda item addresses that possible incompleteness.

2. *Import concepts from the humanities.* Zald (1989) and King (1989) have argued to extend analyses in organizational behavior to incorporate themes and styles of argumentation found in such fields as philosophy, aesthetics, ethics, and history. We badly need some concrete examples that show the ways in which organizational analysis deepens when it makes contact with these traditions.

Psychological

There are at least seven issues at the individual, psychological level that seem worth pursuing in the 1990s.

1. *How do people experience and manage polarities?* There is a growing number of concepts, such as paradox, ambivalence, dilemmas, contradictions, dialectics, oppositions, and ambiguity, that share some characteristics and are thought to be an increasingly visible component of organizational life. How people cope with these is much less well understood.

2. *The role of autocommunication and monologues in organizational action.* The preoccupation with dialogue, interaction, and interpersonal communication, has kept attention off the question, How are individuals themselves affected by what they say? A small but growing body of work suggests that the outcomes of planning are affected as much by what people hear themselves say as by what they hear others say (e.g., Broms & Gahmberg, 1987).

3. *What is a mindset?* The language of *mindset* is appearing more frequently in the organizational literature (e.g., the "global mindset"), but there is less discussion of precisely what a mindset is and whether it means something different from other labels we have used to describe cognition (e.g., Pascale, 1990, argues that mindset = paradigm).

4. *Technology in the mind.* Technology is becoming less visible as more of it disappears onto circuit boards and, as a result, it consists more and more of a cognitive representation in the minds of individuals. Intervention thus becomes more dependent on the adequacy of the representation, which means, in turn, that whatever affects cognition will

also affect the representation and the interventions that it triggers (Weick, 1990b). Implications of this transformation of technology from highly physical to highly mental need to be articulated.

5. *Attention as a limited capacity.* Attention is being given more prominence in organizational analysis, but it is also becoming clearer that attention is a scarce commodity that shrinks even further when people become mindful, self-conscious, or are interrupted. If we shift organizational analysis from a preoccupation with intention to a preoccupation with attention, what implications does that have for action?

6. *Reification as organizational structure in the mind.* Key "entities" in organizational behavior may not be tangible organizations so much as presumed entities that are reified and created in order to justify action. What constraints on action are created by reified structures and how does this influence work?

7. *What is the emotional side of information processing?* Descriptions of information processing are much too stark, and emerging studies of emotion hold the promise to improve our understanding of how people process information and find problems (e.g., Csikszentmihalyi, 1988).

Processes

There are at least eight process issues that are candidates for a research agenda.

1. *Problem finding.* Current interest in vision, pathfinding, framing, and repunctuation exceeds the amount of understanding we have about processes of problem finding. Growing recognition of this discrepancy is a clear signal to a research agenda.

2. *Enlargement of micro events.* The ways in which small events can have large effects is a natural history by which micro- and macrolevels of analysis can be linked. For example, at Bhopal (Weick, 1988), growing indifference by management led to an increase in the incidence of small errors, which increased the probability that several of these small errors might occur simultaneously, which increased the probability of a complex interaction among several small errors occurring simultaneously, thereby creating a dangerous condition of interactive complexity (Perrow, 1984). A possible research activity is to look for other archetypes, like this, by which micro events had disproportionately large, disproportionately long-lasting, effects.

3. *Emergents.* The interest in chaos theory, organizational learning, and evolved jobs suggests that much organizational behavior can be understood as the unanticipated, emergent consequences of initially trivial events. These need to be understood more fully.

4. *Process sensitivity.* One of the fascinating outcomes of recent attempts to globalize domestic firms has been the suggestion that groups that are most effective at making this transition are those best able to think in terms of processes rather than structures and to visualize information and action in images of flows (Weick & Van Orden, 1990). Neither practitioners nor researchers have as much facility talking about and representing process as they do structure. An agenda for the 1990s is to make those competencies more equal.

5. *Improvisation.* The emphasis on flexibility, self-design, and self-managing groups suggests that we need to understand more fully how people make do, improvise, normalize the unusual, and so on. It may be that confidence, self-esteem, and self-identity are crucial antecedents of successful improvisation as are minimal structures and recipes such as the melody line that constrains a jazz musician who solos.

6. *Structuring.* Organizational design researchers have little to say about how organizational structures originate and get into place, and there are signs (e.g., Giddens, 1984; Turner, 1987) that processes that generate structures are being understood more fully.

7. *Change via coercion.* Participation has been the predominant mechanism prescribed for successful change, but there is a growing attempt (e.g., Dunphy & Stace, 1988) to articulate the conditions under which coercion as a means of change makes sense. That, along with parallel discussions of revolution (Tushman, Newman, & Romanelli, 1986), cults (Galanter, 1989), and transformation (Westley, 1990) suggests a greatly expanded range of change techniques that are underway and need to be understood more fully.

8. *Close relationships.* Interpersonal dynamics in close relationships, such as love relationships (Berscheid, 1983), appear to have growing relevance to long-term, self-managing, intense groups in organizations. Research on close relationships may open up a whole new range of issues that we can now look for and see because we have the tools that alert us to them.

Structures

There are at least five issues involving structure that are possible candidates for a research agenda in the 1990s.

1. *Routines.* There is growing consensus that routines are the core of an organization (e.g., Winter, 1990), that they constitute the memory of the organization (e.g., Cohen, 1990), that they hold the organization together (e.g., Feldman, 1989), and interestingly, that they might compose a shared interdisciplinary unit of analysis because they resemble what population ecologists seem to have in mind when they talk about structural inertia. A routine may also stabilize an organization because it is a deviation-counteracting feedback loop, as in the case where an increase in complaints leads to an increase in organizational action, which then leads to a decrease in complaints, less action, more complaints, more action, and so on. Routines may become an increasingly important defining attribute of organizations and may be one of the more consequential forms that organizational structure takes.

2. *New organizational forms.* There is an intensifying search for alternatives to hierarchy as a means to coordinate action (e.g., Heydebrand, 1989), and some of the most interesting suggestions are coming from feminists (e.g., Hyde, 1989). Using the feminists discussions, for example, the most dramatic reading of the literature on high-reliability organizations could be captured in the phrase, "patriarchy kills people." The organizational forms found in control rooms in nuclear plants and in aircrews that are insensitive to "cockpit resource management," resemble closely the military organizational form.

3. *Temporary systems.* Collective action forms and dissolves more often and in a greater variety of configurations than before and under tighter time conditions in organizations of the 1990s than was true of organizations that were functioning when systems theory was first developed. The implication is that transient systems such as construction teams (e.g., Bryman, Bresnen, Beardsworth, Ford, & Keil, 1987) may be prototypes for a much bigger proportion of organizational activity in the 1990s. Systems theory needs to be rethought, by relaxing both the assumption that systems are long term and the assumption that systems are tightly coupled.

4. *Interpretation systems.* There is a growing realization that portrayals of organizations as decision-making systems may bypass earlier stages of organizational thought when members try to figure out whether they even face anything that requires a decision. For example, the well-known prescriptions for decision making proposed by Thompson and Tuden (1959) (computation, compromise, judgment, inspiration) presume that members know whether they agree about means and ends. It is that very act of agreement, however, that is variable and that is the outcome of interpretation (Weick & McDaniel, 1989). To understand organization rationality means that we understand more fully the ways in which interpretations are inputs to the decision-making process. Concurrent with this exploration, we should also see whether the growing interest in organizations as "inquiring systems" is synonymous with interpretation systems, or whether it differs.

5. *Professional organizations.* Organizations built around more intensive information technology look more and more like professional organizations. That equation is tentative, in part because our understanding of professional organizations is less complete than it should be. As we understand traditional professional organizations such as law practices or medical clinics, we may understand more about a much wider variety of organizations than has been true in the past.

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