

Organizational Development: Some Problems and Proposals*

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Examination of the rapidly increasing body of OD literature reveals that much of its research is redundant and without refinement or validation, that the term "Organizational Development" itself remains scientifically undefined and hence primarily a convenient label for a variety of activities, and that the OD literature as a whole is more autobiographical than organizational in focus and scope. Work toward resolution of these three problems, the author suggests, would help the practice of OD base itself on a more mature and usable set of principles and procedures for organizational change. The author also questions the adequacy and utility of the traditional dichotomy between organizational process and structure evident in the literature and discusses a reconceptualization of organizational structure that permits clarification of key issues in the practice and theory of organizational change.

INTRODUCTION

Advising people in power about how they can better attain their goals is a very old occupation. Organizational Development (OD), on the other hand, is a new label for a conglomerate of things an increasing number of consultants do and write about. What that label refers to depends to a

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considerable extent upon the doer or writer. Among the critics and practitioners of organizational development, who are often the same people, there is a continuing argument over the state of the art, its proper definition, and the requisite skills for practicing it.

For example, Harry Levinson (1972), a clinical psychologist writing in the journal *Professional Psychology*, criticized OD practice for its neglect of diagnostic procedures, along with other theoretical and methodological shortcomings. He was answered in the same journal by Marshall Sashkin (1973a) and by Warner Burke (1973b), both of whom undertook a specific rebuttal of Levinson's criticisms and a general defense of organizational development as practiced. Levinson (1973a) responded; Sashkin (1973b) replied; Burke (1973a) commented further; and Levinson (1973b) offered summary comments, which included the inarguable observation that the discussion had reached the point of diminishing returns.

Such exchanges reveal a good deal about current practice and preference, and several recent review articles provide more comprehensive statements. One of the most instructive is Friedlander and Brown's chapter in the current *Annual Review of Psychology* (1974), which is built around the familiar dichotomy between people-oriented ("human processual") and technology-oriented ("technostructural") approaches to organizational change. Comprehensive reviews have also been written by Alderfer (1974), Sashkin *et al.* (1973), Strauss (in press), and Hornstein *et al.* (1971). Leavitt's earlier chapter (1965) speaks in terms of applied organizational change rather than development, but the difference is more terminological than substantive and the review remains a useful commentary on what is now called OD.

More numerous than such reviews are books and articles that describe a preferred approach to organizational development, sometimes in very general terms, sometimes with a good deal of theoretical elaboration, occasionally with some substantiating empirical data. Examples are provided by Argyris (1970, 1971) on intervention theory, Blake and Mouton (1968, 1969) on grid organization development, Schein (1969) on process consultation, Schmuck and Miles (1971) on the OD Cube, and others.

My present purpose is neither to rehearse the OD arguments nor to review the reviews. Much less do I wish to dispute whether the practice of organizational development has been nearly completed, is well en route, or has barely begun the long transition from being a miscellany of uncertain devices to becoming a mature, usable set of principles and proce-

dures for organizational change. I want instead to cite some problems, the resolution of which will facilitate that transition and thus make organizational development better than it is, in theory and practice.

OMISSIONS AND REDUNDANCIES

No reasonable person can complain that written material on organizational change and development is meager. In 1962 Everett Rogers (Rogers & Shoemaker, 1962, 1971), working in an admittedly broader area, the communication of innovations, generated a bibliography of some 1,500 items. In 1968 Ronald Havelock (Havelock *et al.*, 1968, 1972) found almost 4,000 titles in the area of planned innovation, and offered the terrifying bibliographic projection that the number was increasing at the rate of 1,000 per year. Such volume invites specialization; in the more circumscribed area of organizational development, Jerome Franklin (1973) lists about 200 books, chapters, and articles. That is the body of material most relevant for our present purposes; what does it tell us?

About 15 years ago, March and Simon (1958) observed that rather little had been said about organizations, but that little had been said repeatedly and in many different ways. In the years since then, I believe that caustic judgment has become less accurate for organizational research in general, but it remains unhappily true of writings on organizational development. A few theoretical propositions are repeated without additional data or development; a few bits of homey advice are reiterated without proof or disproof, and a few sturdy empirical generalizations are quoted with reverence but without refinement or explication.

For example, Kurt Lewin's (1947 a,b) suggestion that the process of planned change be conceptualized in terms of three successive phases—unfreezing, moving, and freezing—is often quoted or paraphrased as a preamble to research, but seldom with any clear indication of how that formulation determined the design of the research that follows its invocation. The Lewinian concept of quasi-stationary equilibrium (1947c) is also frequently mentioned, but without any systematic conceptualization or measurement of the alleged opposing forces. Gordon Lippitt (1969) presents this model as “force-field analysis,” and 40 driving and restraining forces are represented by opposing arrows in a diagram (p. 156). The forces, however, are unidentified; their identification and measurement is left to the reader. Such presentations are common. The Lewinian schema thus remains not only unelaborated and untested, but really unused. It deserves more serious attention.

The OD literature contains other slogans, less theoretical but recited no less often. Consider, for example, the advice that the “change agent” should “start at the top” of the organization he intends to change. Beckhard (1969) makes “management from the top” one of five defining characteristics of organizational development; Blake and Mouton (1969) assert that “to change a company, it is necessary for those who head the company to lead the change of it.” Argyris’ recent cases (1971) begin with discussions between the author and chief executives of the companies described.

I have neither experience nor data to challenge the advice that one should start at the top, and certainly it has a pleasant ring. Nevertheless, it would benefit from specification and test. Does it mean that the top of the organization must change before any other part can do so? Does it mean that the people at the top of the organization must actively support the proposed program of change without necessarily becoming “trainees” themselves? Or does it mean merely that some degree of top-echelon sanction for the new enterprise of organizational development must be visible in order for others to accept the proposed changes? One can readily imagine research to answer these questions, but it has yet to be done. Nor can it be, until the homily about starting at the top is stated with enough specificity to be tested.

As an example of a third sort of redundancy without development, empirical generalization, let us take the proposition that organizational changes are more likely to be accepted by people who have had a voice in determining their content. This is the principle of participation, perhaps the best established and most widely accepted empirical generalization in the literature of organizational change. The research pedigree for this principle dates back at least to 1948, when Coch and French published their classic article on overcoming resistance to change. Their experiment demonstrated that varying degrees of employees’ participation in changes of work methods were related to their expressed acceptance of the new methods, to the rapidity with which they learned those methods, and to their decision to remain as employees of the company.

As good research should, the Coch and French experiment not only answered old questions; it raised new ones. Some of them—the interaction of participation with individual personality differences, for example—have been the subject of subsequent investigations (Tannenbaum & Allport, 1956; Vroom 1960). Others remain unstudied; for example, the important question of distinguishing the motivational effects of participation from substantive effects of participative decisions.

It would be exciting to see an organizational development program that included research designed to obtain separate estimates of the effects of identical substantive changes generated under participative and non-participative conditions. Such data could be provided, I think, by means of a design using "master" and "slave" groups. (I use the terms only in their figurative, mechanical sense.) Workgroups would be chosen in sets of three, one in each set randomly designated master, one slave, and one control. If one of the master groups decided in the course of an OD program that the group should have the authority to set its own standards or choose its own methods of work or have access to current cost data, these same changes would be initiated in the slave group, but by conventional managerial instruction. An increase in productivity or satisfaction in the master groups, as compared to control groups, would be interpreted as the combined consequence of the participative experience and the substantive participative decisions. An increase in productivity or satisfaction in the slave groups, as compared to control groups, would be interpreted as the effect of the substance of the decisions without the motivational effect of participation. The difference in criterion changes in a master group as compared to the matched slave group would be interpreted as reflecting the effect of participation alone, the effects of decision content having been held constant experimentally within each such pair of groups.

Whether or not readers share my enthusiasm for this particular case of unexplored research is not important. The foregoing examples of omission and redundancy in the literature of organizational development and change are not intended to urge some particular research project. Rather, they are intended to rouse in the reader a thirst for movement—for elaboration and strengthening of old theoretical formulations, for systematic test of old injunctions, for the refinement and extension of old empirical generalizations. Argument by example, however, is always judgmental; let us state the criticism of redundancy in more objective terms. Of the 200 items in the Franklin (1973) bibliography of organizational development, only 25 per cent include original quantitative data; the remaining 75 per cent consist for the most part of opinions, narrative material, and theoretical fragments. No branch of science can long afford such a ratio. Ideas and personal impressions need desperately to be tested by collision with facts. The mill of science grinds only when hypotheses and data are in continuous and abrasive contact.

PACKAGES AND CONCEPTS

Organizational development is not a concept, at least not in the scientific sense of the word: it is not precisely defined; it is not reducible to specific, uniform, observable behaviors; it does not have a prescribed and verifiable place in a network of logically related concepts, a theory. These statements hold, I believe, in spite of some serious efforts to provide a workable definition and a meaningful theoretical context.

Lawrence and Lorsch (1969) provided one such example, building on their earlier work on differentiation and integration (1967) and describing organizational development in terms of activities at three interfaces—organization and environment, group to group, and individual in relation to organization. Argyris (1970) provides another example, in his sustained effort to conceptualize and describe his own experience in organizational change. His emphasis is on the autonomy and “health” of the client organization, and on OD as a means of increasing those valued characteristics by increasing the capacity of the organization to generate and utilize valid information about itself.

Argyris, like Lawrence and Lorsch, is stating his own definition and theoretical position; he is not attempting a formulation that accommodates everything that goes by the name of organizational development. Attempts at such broader and more eclectic statements sacrifice a good deal in precision and theoretical connectedness. For example, Bennis (1969) says that “organization development is a response to change, a complex educational strategy intended to change the beliefs, attitudes, values, and structure of organizations so that they can better adapt to new technologies, markets, and challenges, and the dizzying rate of change itself.” His co-editor, Richard Beckhard (1969) says that “organizational development is the name that is being attached to *total-system*, planned-change efforts for coping with the above-mentioned conditions.” (These conditions include four assertions about “today’s changing world,” five about “today’s business environment,” and six about “today’s changing values.”) I find those definitions too inclusive to be helpful, and others go still farther. Margulies and Raia (1972) offer a definition broad enough to include everything from market research to industrial espionage. They define organizational development as consisting of “data gathering, organizational diagnosis, and action interventions.”

Other authors give us other descriptions, and their variety serves to underline my assertion that *organizational development* is not a concept.

This assertion is in itself neither praise nor damnation; it merely reminds us that the term is a convenient label for a variety of activities. When we remember that fact about the term *OD*, we benefit from its convenience, as we do from the convenience of other colloquial terms—*mental health* and *illness*, for example. Scientific research and explanation, however, require concepts that get beneath convenient labels and represent explicitly defined and observable events and behaviors. The literature of organizational development is disappointing in this respect; it is tied too closely to the labels in terms of which the varied services of organizational development are packaged and marketed.

Moreover, this criticism holds even when we consider more specific terms. *Sensitivity training*, for example (also known as *laboratory method* or *T-Group training*, and partially inclusive of such variants as *encounter groups* and *personal development groups*), is itself a convenience term for a number of activities that probably vary as much with the preferences of the trainer as with anything else (Back, 1972). *Grid organization development* is another such term; it refers to those consulting and training activities marketed by Blake and Mouton and their colleagues (1964, 1968, 1969). Indeed, their firm has registered the term as a trademark or brand name—the antithesis of scientific conceptualization.

One of the persisting problems with research on organizational development is that it has incorporated such colloquial and commercial terms as independent variables. I have noted that of the more than 200 bibliographic entries on organizational development, about 25 per cent (53) present quantitative data. Within that subset, more than 65 per cent (35) utilized independent variables that must be considered packages rather than concepts. In most of those, the package was “the T Group,” variously employed and mingled with lectures, skill-practice, and other training activities. In about 10 per cent of the data-reporting articles, the “independent variable” was “Managerial Grid Training.”

In a few cases the experimental treatment was simply—or rather, complicatedly—“Organizational Development.” And a few others offer as the independent variable a sort of omnibus treatment in which the social scientist and management seem to have done a variety of things—T Groups, consultation, lectures, surveys, explicit changes in formal policies, and the like, which they hoped might produce wanted changes in employee attitudes and behavior. Evidence of such changes is presented, but we are left in doubt as to the potent ingredient or synergistic combination of ingredients that produced the effect.

Such aggregate treatments need not be bad, but they can be made scientifically good only when the package treatment is sufficiently described to permit replication and "dissection" of its ingredients. I have found no examples of sustained refinement of independent variables in the articles that make up the bibliography of organizational development, although some beginnings have been made from time to time. In 1965, Bunker, for example, showed that conventional T-Group experience produced changed interpersonal behavior in the back-home work situation, as measured in terms of the perceptions of co-workers, not merely the perceptions of the trainees themselves. Shortly thereafter, Bunker and Knowles (1967) replicated those findings, with variations in the duration of the experimental treatment. They compared the effects of two-week with three-week T Groups, and found that the latter generated the greater perceived changes in behavior. We could wish for more work along these lines, especially in view of the tendency toward shorter and more intensive use of T Groups and encounter groups—a tendency that appears to be based on administrative convenience rather than evaluative research.

There is another encouraging sign in the research that uses packages as independent variables: a few experiments or quasi experiments have compared packages. Perhaps the best example of such comparative work is Bowers' (1973) article "OD Techniques and Their Results in 23 Organizations." This research is based on data from 14,000 respondents in 23 industrial organizations and reports gain scores (before and after treatment) for four patterns of developmental activity—Survey Feedback, Interpersonal Process Consultation, Task Process Consultation, Laboratory (T Group) Training—and two "control" treatments, "data hand-back" and "no treatment." Bowers found that "Survey Feedback was associated with statistically significant improvements on a majority of measures, that Interpersonal Process Consultation was associated with improvement on a majority of measures, that Task Process Consultation was associated with little or no change, and that Laboratory Training and No Treatment were associated with declines."

These findings are not definitive, nor are they presented as such. There are the now-familiar problems with raw gain scores (Cronbach, Gleser, *et al.*, 1972), although Bowers has done supplementary analyses to control for initial differences in the several treatment groups and to test for the plausibility of the alternative hypothesis that his results merely reflect the regression of extreme scores toward the mean. There are other explicit

limitations: the treatments are defined only approximately; there is confounding of change agents with treatment differences (since each change agent conducted the treatment of his choice); there is some self-selection of treatments by populations as well as change agents; and there is the absence of hard criteria of organizational change (productivity, profit, turnover, and the like). Finally, a sociologist of knowledge might express some lurking skepticism that Survey Feedback had been discovered by its proponents to be the most effective form of organizational development. Nevertheless, the comparison of treatments is most welcome; I applaud it and only wish that it were more frequent.

Friedlander and Brown (1974), in a careful review article of some 18 pages, require only three paragraphs to summarize comparative studies of OD interventions. Moreover, of the three studies summarized, two (Greiner, 1967; Buchanan, 1971) do not evaluate alternative interventions; the third study cited is that of Bowers.

Even such comparative studies, however, leave us with needs for explanation that can be satisfied only by research that clarifies the nature of the independent variable, the experimental treatment itself. Such conceptualization and explicit definition of the experimental treatment is well illustrated in three field experiments that are widely regarded as classics in organizational change—Coch and French's (1948) work on the effects of participation, Morse and Reimer's (1956) on hierarchical locus of decision-making power, and Trist and Bamforth's (1951) on changes in sociotechnical structure. There are other and more recent examples, of course, but the list remains short. Let us hope that it will lengthen.

AUTOBIOGRAPHY AND ORGANIZATION

My third criticism of research on organizational development may seem to include a contradiction in terms: the research on organizational development is not sufficiently organizational. It is too autobiographical a literature, too concentrated on the experience of the trainees and change agents. It is a literature of training episodes, and those episodes are often nonorganizational or extra-organizational.

Research that carries the term *organizational* in its title often consists of the group experience of a few people, far from the organizations that are allegedly being developed. More often than not, the criteria by which the success of the developmental process is judged are the reactions of these participants to the temporary group experience. Let us be specific:

of the projects (in the Franklin [1973] bibliography) reporting empirical data interpreted in terms of organizational development, about 60 per cent are based on data from the training episode only; 40 per cent include some measure of the persistence of the training effect to some later time. A much smaller proportion, 15 per cent, trace the training effect in terms of behavior in the organization itself. Forty per cent measure the effect of the experimental treatment only in terms of self-report, and an equal proportion include no control or comparison group, either as part of an experimental design or in the statistical analysis of a larger population.

Friedlander and Brown (1974) report similar conclusions in the three kinds of process-oriented intervention that have been most researched—survey feedback, group development, and intergroup development. They find “little evidence that survey feedback alone leads to changes in individual behavior or organizational performance,” but considerable evidence for reported attitudinal change, at least in the short run. They speak of research on group development intervention in comparable terms: “There remains a dearth of evidence for the effects of team building external to the group developed.” As for intergroup relations development—

. . . there is very little systematic research on the effectiveness of such interventions in the field. Case studies abound (e.g., Blake, Mouton, and Sloma [1965]), but they leave many questions about the efficacy of the intervention unresolved.

Most OD activities seem to emphasize process rather than structure as the primary target of change, and most research describing the effects of structural changes in organizations seems to exemplify a different tradition from organizational development. However, definitional distinctions are difficult to make when definitions are unclear. If one includes in the realm of organizational development those studies that Friedlander and Brown call “technostructural,” the evidence for persisting organizational effects increases. Certainly such effects were attained in the coal-mine experiments of Trist and his colleagues (1951, 1963) and in the textile-mill experiments of Rice (1958, 1969). Thorsrud (1969), working in the same theoretical tradition, reported still broader and more ramifying changes in a series of Norwegian field experiments. In all these cases, the primary aim was to improve the goodness of fit between the social and the technical aspects of the work organizations. The improvement involved changes in both organizational aspects.

Significant increases in performance, attendance, and satisfaction have also been accomplished by organizational changes that begin with the division of labor, the definition of individual jobs. Such approaches include job design, job enlargement, and job enrichment, the distinctions among which are not always clear. All three share the assumption that many industrial jobs have been fragmented beyond the point of maximum efficiency, and that gains in performance and satisfaction are obtainable by reducing the fragmentation and increasing the variety of content. Results of such work are described by Davis and Taylor (1972) and Ford (1969), and are summarized by Stewart (1967) and Friedlander and Brown (1974). The findings are not uniform, nor are the changes in job content that serve as independent variables or the organizational circumstances in which the experiments were attempted. One must conclude, nevertheless, that the content of the job makes a difference, and that intervention in terms of job content is likely to have effects.

Our present point, however, is not where OD intervention should begin but rather where it should end. As the term *organizational development* reminds us, the organization is the major target of change. Persisting change in the organization must therefore be the criterion of OD success or failure.

STRUCTURE AND PROCESS

The penultimate problem that I wish to raise about organizational development is the separation of structure and process. It is a familiar enough distinction in organizational theory and in writings on organizational change. Friedlander and Brown (1974) classify all efforts at organizational development as either "technostructural" or "human processual." Leavitt (1965) had earlier proposed a trichotomy: a similar distinction between structure and process, and an additional distinction between technological structure and social structure. As classifications that remind us of the different emphases or starting points of various approaches to change, I find these schemes useful. As classifications that imply the separation of organizational process and structure, however, I find them misleading.

We have argued elsewhere (Katz & Kahn, 1966) that human organizations be viewed as a class of open systems which lack the usual properties of physical boundedness and therefore lack structure in the physical or "anatomical" sense of the term. The structure of an organization can thus be said to consist in the pattern of interdependent events or activi-

ties, cyclical and repetitive in nature, that in combination create the organizational product or service. Organizational structures can therefore be well described in terms of roles, those activities expected of persons occupying certain positions in a network of such expectations and behaviors.

The structure of the living organization is not the charts and job descriptions and work-flow diagrams usually employed to describe those roles and the relationships among them. The structure of an organization is the pattern of actual behaviors as that pattern is created and re-created by the human beings we call members of the organization.

It may be useful for some purposes to distinguish this actual organization from some idealized or preferred structure, to distinguish the paper organization from the living organization, for example. There are many ways of making such distinctions—formal versus informal organizational behavior, role prescriptions versus role elaborations, and the like. But the central point remains: the structure of the organization *is* the pattern of actual recurring behaviors.

If we are agreed on that point, the issue of structural versus processual approaches to organizational development takes on a new and clearer form. To change an organization means changing the pattern of recurring behavior, and that is by definition a change in organizational structure. For example, suppose that an OD practitioner somehow gets all the supervisors in an organization to tell employees in advance about any developments that may affect their jobs. Suppose that the giving of such advance information becomes a continuing supervisory practice, a norm among supervisors, and an expectation on the part of subordinates. It will then be part of the role structure of the organization.

Providing such information may or may not be written into the job descriptions of supervisory duties, may or may not be included among criteria for promotion of supervisors. Likelihood of providing such information may or may not be built into the selection procedures for supervisors. To the extent that these things are also done, the giving of advance information becomes more a part of the “formal” structure of the organization, by which I mean management’s representation of the organization. The incorporation of some change into the formal structure of the organization—that is, into the management-prescribed roles—has its own significance. But the main issue is change in the enactment of roles; if there is change in those recurring behavior patterns, then the structure of the living organization has changed. Organizational devel-

opment, if it implies change at all, must change those behaviors, regardless of whether it calls itself structural or processual.

I believe that these issues become clearer if we discuss separately the target and the means of organizational change, and if we do so in terms of role concepts. There is by now some consensus about these concepts and their definition. A role consists simply of the expected behaviors associated with a particular position or office. The people who occupy positions somehow interdependent with that office typically hold and communicate expectations about the behaviors they want enacted by its occupant. The job description thus becomes a special case of such role expectations, probably sent to the occupant by someone in the personnel department acting as a surrogate for upper management. The actual behavior of a person in a role is likely to be a complex combination of responses to the expectations of relevant others and spontaneous activities that are neither prescribed nor proscribed by others. These latter activities are referred to as role elaborations, in contrast to role prescriptions.

Now let us use these terms to discuss first the target of change and then the means of change. I assume that when people speak of the target as structural change, they mean changing the roles and the official or formal expectations associated with them. The number of jobs, their formally prescribed activities, and their prescribed relationships to other positions are examples of such changes.

When people speak of changes in process rather than structure, I assume that they are referring to aspects of role behavior that are not usually prescribed but left to the discretion of the occupant—role elaborations rather than role prescriptions. For example, in most organizations, the extent to which supervisors express consideration and interest toward workers is a matter of role elaboration—unspecified in the formal description of the supervisory job and not included in the role expectations expressed by the workers themselves. If an OD program increases supervisory consideration, the changes might ordinarily be referred to as processual. It might be argued, of course, that what the OD program has done is to move certain consideration-expressing behaviors from role elaborations (options) to role prescriptions (managerial expectations, in this case), and that the target is therefore the formal structure of the organization, after all. I would not disagree, except to point out that the observation illustrates my point about the special meaning of structure in human organizations, and the ultimate fusion of structure and process.

A similar distinction and unity with respect to structure and process is involved in the means of organizational change. One can, for example, attempt to bring about change by altering the formal role prescriptions, introducing new technology, new written policies, new division of labor, and the like. Or one can attempt to bring about change by means of process interventions—counseling, consultation, encounter groups, and the like.

But in the means of change, as in the target, we see some blurring of the usual dichotomy between structure and process. Even Frederick W. Taylor (1923), that classic exemplar of the structural approach to organizational change, began with process-like persuasion and interaction, first at the top of the company and then with the immortal Schmidt. Morse and Reimer (1956) used counseling, role playing, T Groups, and other process-emphasizing activities to bring about and anchor the systemic organizational change they sought.

On the other hand, the most process-oriented OD practitioner necessarily enters the organizational structure in which he hopes to encourage change. He creates a role for himself in that structure, and probably changes the role expectations and prescriptions of the people with whom he meets—if only because they are expected to speak with him, attend the group sessions he arranges, and the like. Moreover, his processual interventions, to the extent they are successful, are likely to lead to changes in formal policies, role prescriptions, and other representations of organizational structure.

The process-oriented OD specialist is likely to avoid specifying what structural changes he prefers; he expects those decisions to emerge from the heightened sensitivity and problem-solving abilities of the individuals and groups with which he has worked. He thus illustrates a certain complementarity with his structure-emphasizing counterpart. One OD specialist speaks in terms of process and says little about the structural end-state that he hopes to see the organization attain. The other concentrates on advocated changes in formal structure (job size, division of labor, and the like) and tells us little about the process by which changes are to be attained.

If there is a lesson for us as researchers and as observers of organizational development, it is to avoid being too absorbed with terminological distinctions and to concentrate instead on what is actually done by the change agent, what subsequent behavioral changes in the organization can be identified, their duration, and their ramifying or receding effects.

CHANGING THE UNWRITTEN CONTRACT

I believe that the body of material on organizational development, with its distinctive strengths and weaknesses, is itself the product of special conditions—the concentration of developmental experience in business and industry, and the nature of the role relationship between managements and researcher-consultants. A management, typically concerned with the productivity and profitability of its enterprise, with secondary interests in job satisfaction and the meaningfulness of work, pays a specialist in organizational development to do certain agreed-upon things in expectation of improved productivity and profit. If these results can be brought about with concomitant gains in satisfaction and worker identification with task and mission, all the better; hence the special appeal of approaches that promise some explicit linkage of satisfaction and productivity. Management also assumes in most cases that the process of organizational development will not alter or infringe traditional managerial prerogatives in matters of personnel, resource allocation, and the like.

The change agent, in his writings about organizational development, gives more emphasis to humanistic values in organizational life. He accepts his role in relation to management in the hope of contributing to the realization of those values, in the hope of increasing the satisfaction and meaningfulness of work. He also wishes to add to the store of things known about human organizations and to learn how they can be influenced. He usually enters into the relationship with management as a paid consultant, and with implied agreement about areas of activity and reservation. Too often, in my view, that implies agreement to induce some change in satisfaction, motivation, and productivity without becoming involved in resource allocation, availability of equipment, choice of supervisors, content of jobs, allocation of rewards, and the like. In the extreme case, the organizational developer agrees to leave the role structure alone and to induce changes in role elaboration—those activities and stylistic characteristics that are left to the discretion of individuals.

Progress in organizational change and the knowledge about change has been made sometimes in spite of those limitations, and sometimes because of welcome exceptions to them. Now there are numerous signs that the contract between management and behavioral science is being redrawn. Managements are less insistent on tangible results from intangible manipulations, and behavioral scientists are less willing to attempt such legerdemain. As one visible sign of this tendency, I welcome the work on job design (Davis & Taylor, 1972), which approaches organiza-

tional improvement through change in one of the major aspects of organizational structure—the division of labor. I hope that more research is done on job design, and that comparable lines of research develop on other aspects of formal organizational structure. Not only will this strengthen the practice of organizational development; it will also bring the language of organizational development into the larger realm of organizational theory and research. It is a long awaited convergence.

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