

FROM HUMAN RELATIONS TO ORGANIZATIONAL BEHAVIOR: REFLECTIONS ON THE CHANGING SCENE

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This article is an interpretation of the last fifty years of behavioral science research in industry. The author, who has been active in the field since the early 1940s, describes the development since the late 1960s of two distinct streams of organizational behavior research: a main stream, characterized by a sharp separation between research and practice and rigorous specification and measurement of variables, and an alternate stream, with more applied research and sometimes active involvement in organizational change. Labor and management practitioners today are far more interested in behavioral research—at least that of the alternate stream—than they were in the 1940s and 1950s. From an analysis of the literature on the relationship between worker participation and productivity, the author concludes that the alternate stream offers more promise for advances in both science and practice.

WE are now in the midst of the most drastic and fundamental changes in industrial relations of any era since the great CIO organizing drives in the 1930s. I was in college (1932–36) at the height of the CIO campaigns, but I began my first study in industry not long after (in 1942) and I have continued active in this field since that time. Thus, what I am presenting can be considered the report of a participant observer on what we behavioral scientists have been doing in studies of industrial relations and on how we have related our work to that in other disciplines and to labor and management.

When and How Did It Begin?

I trace the beginnings of behavioral science research in industrial relations to

Elton Mayo and his colleagues at the Harvard Business School who worked on the Western Electric research program in the 1920s and early 1930s, culminating with the publication in 1939 of *Management and the Worker* (Roethlisberger and Dickson). Many sociologists prefer to trace the origins back to Max Weber, who indeed did make his important contributions decades before Mayo, but Weber's writings on bureaucracy did not lead directly to the development of a new field of study. It was the Harvard–Western electric collaboration that launched research in the field first called human relations in industry or industrial sociology and now more generally known as organizational behavior.

I first became acquainted with the Western Electric program in a seminar with Elton Mayo in the fall of 1937. I read and discussed with him his books on the Hawthorne plant, which presented earlier

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interpretations than the more solid and systematic *Management and the Worker*. It was this experience with Mayo, reinforced by associations with social anthropologists Conrad M. Arensberg, Eliot D. Chapple, and F. L. W. Richardson, that led me away from community studies (Whyte 1943) and into industrial relations.

In 1940 I moved from Harvard to the University of Chicago to study under social anthropologist W. Lloyd Warner, then known particularly for his Yankee City studies. As I later learned, while at Harvard he had led the Mayo group from the famous experiment of the test room girls in the Hawthorne plant to a study of a work group in its natural factory setting: the bank wiring room (Roethlisberger and Dickson 1939). This still seems to me one of the finest work group studies ever carried out. The test room experiments excited public attention because of the surprising finding that the young women continued to increase their production when the conditions of work were made more favorable or less favorable. Since Roethlisberger (1977) himself later offered seventeen full or partial explanations of that result, the scientific significance of the test room studies seems to me to have been greatly overrated. On the other hand, the bank wiring room study suggested the importance of examining a group of workers in its natural hierarchical structural working conditions—a line of research more compatible with social anthropology than with the psychological orientation of Mayo.

In fact, Mayo himself derailed these promising beginnings in what I have called a monumental misinterpretation of the practical implications of the Hawthorne plant studies (Whyte 1978). I like to startle my students with the claim that, though there may be a phenomenon such as "the Hawthorne effect," it did not appear in the Western electric research at the Hawthorne plant. The Hawthorne effect interpretation is based on the notion that the increasing productivity of the test room women was a response to the friendly interest in them and in their work by the research observer and therefore

indirectly by management. As Arensberg (1951) pointed out, however, the men in the bank wiring room, like the women in the test room, were provided by management with a friendly and sympathetic observer, and yet their productivity ran in a straight line throughout the observation period.

How can we explain these productivity differences? We can believe either that women are more gullible than men or (as I think more likely) that the explanation lies in the markedly different *structural conditions* within which the two cases were embedded. The bank wiring room was designed to operate just like a regular department—with the constant presence of the observer being the only deviation from that norm. The men worked under the close supervision of the foreman and had frequent and predominantly negatively tinged interactions with their inspector. In contrast, no inspector was ever present in the test room, and the foreman only appeared to deliver materials and pick up the output. In today's terminology, the test room functioned as an autonomous work group.

Unfortunately, Elton Mayo never sought to explain the striking output differences between the two cases. The hypothesis of the Hawthorne effect to explain the results of the test room study was attractive to him because it fitted in with his previously stated convictions that the human problems of industry arose out of boredom and obsessive reveries suffered by workers in repetitive jobs. The company and the workers, he believed, would benefit by the establishment of a personnel counseling program to enable individuals to achieve catharsis by unburdening themselves to a sympathetic non-directive interviewer.

In effect, the inauguration of the personnel counseling program blocked off social research at Western Electric. Whatever benefits workers or the company gained through the 20 years and many millions of dollars Western Electric invested in personnel counseling, that program bore little resemblance to research. Social research had also been on the shelf

in the Harvard Business School for a decade or more, while the professors went about gathering case materials for teaching.

Revival came in 1943 under the leadership of two social anthropologists who formed the Committee on Human Relations in Industry at the University of Chicago. Picking up the neglected leads from the bank wiring room case, they committed themselves to studying industrial life in its natural settings. W. Lloyd Warner chaired the Committee and Burleigh B. Gardner served as its executive secretary until 1946, when he left the University to form his own research and consulting organization. Warner and Gardner were joined by Everett C. Hughes (Sociology Department), Allison Davis and Robert J. Havighurst (Department of Human Development), and George Brown (School of Business). In 1944 I joined the Committee to direct a study on human relations in the restaurant industry, and I stayed on to become executive secretary from 1946 to 1948.

I believe the Committee was the first instance of an interdisciplinary group of professors and students planning and carrying out a research program in what we then called human relations in industry. The program was initially supported by six industrial companies providing \$3,600 a year each. Shortly after the beginning, Sears, Roebuck and Company joined in supporting the Committee, and small pieces of support were picked up elsewhere.

The Chicago initiative was followed up shortly by the Labor-Management Center of E. Wight Bakke and the Technology Project of Charles Walker at Yale and the work of Douglas McGregor and others at MIT.

Reactions Among Sociologists and Economists

This sudden opening of a new field of study attracted wide attention and enthusiasm among students. It was greeted with some apprehension, however, among our colleagues in sociology and economics at

Chicago and elsewhere. Some sociologists concluded that they too should move into teaching and research in this new field, which they preferred to call industrial sociology. Tracing the origins of the field to Max Weber, they focused attention on broad studies of management and labor, with emphasis on the macro- or societal-level aspects.

Meanwhile, those of us pursuing human relations were involved in intensive field work in factories and even with small work groups. Some sociologists and labor economists attacked us for being both unethical and unscientific. Since we were then entirely supported by management, they claimed we were not really engaged in scientific pursuits but were rather creating a managerial sociology, helping management to manipulate workers and to undermine unions or avoid unionization. And since workers and union leaders knew that our work was supported by management, they would not talk frankly to our interviewers. Therefore, we could only get a one-sided and unscientific view of the topics we were studying.

We were also accused of disregarding the role of unions. When we did study rare cases in which local unions and management had resolved their conflicts and developed cooperative relations, critics claimed that our conception of cooperation meant simply having union leaders and workers agree to do whatever management wanted done. Our critics also charged us with believing that all conflicts between labor and management could be resolved through "good communications"—that panacea being provided by the management people who learned from our research.

Academic Turf Problems

Before the human relations boom, the field of industrial relations had been monopolized by labor economists and industrial psychologists, who were concentrating on the measurement of individual skills and aptitudes. As the human relations people opened up the new approach, many industrial psychologists joined in the

trend and began calling themselves industrial social psychologists.

Although some labor economists joined in the attack on the ethics of the human relations researchers, there were others who were more puzzled than hostile. In fact, some of them—particularly Frederick Harbison and Charles Meyers—went out of their way to try to fit us in, and leaders of the Industrial Relations Research Association have periodically tried to encourage behavioral scientists to get involved with IRRA, which still remains predominantly an association for labor economists.

The difficulty in fitting us in can be traced in part to a problem of the differing scale of studies on the two sides of the divide. In the 1940s and 1950s, when labor economists thought of doing a case study, they would have in mind something like labor relations in the steel industry. If they wanted to really narrow the field down, they might settle for a study of labor relations in United States Steel. In contrast, behavioral scientists approached the field with a much more micro focus. For us, a case study was likely to be limited to a particular factory or even to a single department in that factory or a single work group. Instead of studying an international union as a whole, we were more inclined to do intensive studies of local unions (Sayles and Strauss 1951).

The mixed reactions to what we were then doing were well expressed in a 1948 Cornell summer institute on industrial relations. When I had finished my report, a labor economist expressed his concern about the lack of a firm theoretical framework guiding our research: "You people in human relations are certainly doing interesting research, but do you know enough yet to teach courses?" I replied rather undiplomatically, "In your studies of labor relations you are constantly having to point out that the orthodox theories of economics don't apply. We are trying to build theory from the ground up in the field."

Common Misinterpretations of Management Interest in Human Relations Research

Many years later, perhaps it is possible to state with some detachment that such attacks were largely based on misinterpretations of management's interest in and utilization of our research, as well as on a misunderstanding of the way we went about our work. To be sure, the people who decided to contribute to the financing of our research must have thought their companies would get some benefits out of the relationship, but their initial expectations were both modest and vague. In a period of high profits during World War II, \$3,600 a year was a trifling sum. Some executives who had intellectual and cultural interests beyond the bottom line may have been attracted by the part of the program that brought them together for dinner every six weeks with the professors to engage in high-level discussions of labor, management, and society.

If executives had an immediate practical interest in our research, it concerned the problem of absenteeism and labor turnover. In the very tight wartime labor market, they had difficulty filling their orders because workers were not showing the expected discipline in coming to work or were simply leaving in search of openings elsewhere. Even that interest, however, did not at first open any doors for research. It was a year from the beginning of the Committee before its members were able to gain permission for in-plant research from any of the six companies supporting the program.

There were two common answers to researchers' requests for permission to launch in-plant studies. One answer was that things were going so smoothly in the plant that management did not want any outsiders to come in and ask workers how they felt about their jobs because such probing might get workers to think about reasons to be unhappy. The other answer was that the situation was so tense on the shop floor that the introduction of any outsider could set off an explosion.

Lacking access to the plants, our re-

searchers could learn about worker-management relations only through house-to-house interviews in working-class neighborhoods. The data collected in this way were miscellaneous, but at least the interviews did furnish stories that might have some bearing on what led workers to consider a job good or bad, a supervisor good or bad, and so on.

The breakthrough to in-plant studies occurred suddenly in one of the evening meetings shortly before I joined the Committee. After expressing his concern and frustrations about his company's labor relations, Walter Paepke, chief executive officer of Container Corporation of America, said, "The situation in our 35th Street plant is so fouled up that no outsider could possibly make things worse. Why don't you come in and see if there is anything you can do?" Burleigh Gardner went through this open door and began the first of a series of field studies that was eventually extended to several of the other supporting companies.

Paradoxical as it may seem, in those years with the Committee, we found many workers and local union officers and even some international representatives more open and interested than most managers in what we were doing. Since we always told them at the outset about our source of support, they initially viewed us with suspicion. But once a skillful field worker had spent some time getting acquainted with workers on the factory floor and talking with local union leaders, and they had encountered no negative consequences from our presence in the plant, the barriers began to go down. Many workers came to express themselves freely and frankly. Why? Again and again we got the same explanation. Workers and local union leaders had tried to get management to take action on problems they were facing. Either they could not get anybody in management to listen or else people listened but then nothing happened. So they would say to us, "You talk to the management people. Maybe you can get them to understand our problems."

My entry into the program in 1944 was made possible by a \$10,000 grant from the

National Restaurant Association. But the giving of that grant did not mean that members of the association had any strong interest in understanding human relations in their industry. They had approached the University of Chicago to negotiate for the establishment of a master's program in restaurant administration. George Brown reported to the Committee on Human Relations in Industry that the School of Business was prepared to accept financial support from the National Restaurant Association to build such a program, but only if a small portion of the funds were set aside to support research. Brown reported that no one in the School of Business had any interest in research in the restaurant industry. Did the Committee members have anything to suggest? I believe it was Everett Hughes who suggested to the School of Business that I direct a study of human relations in the restaurant industry.

Toward the end of our field work, I was invited to give talks in several cities to groups of restaurant management people. For that purpose I focused on what I was calling "human elements in supervision" in the draft of a book, and that subject did evoke some interest. When the NRA members read the first draft of the manuscript, however, they were distressed by my discussion of the low prestige of the industry and the low social status of waiters, waitresses, counter-men, dishwashers, and so on. Regarding the motivation of the NRA sponsors for supporting the grant to the University of Chicago, the most revealing comment on my manuscript was phrased in three blunt sentences: "I thought that the reason we wanted to work with the University of Chicago was to raise the status of our industry. If this book is published it will have the opposite effect. Therefore it should not be published." If the University's contract with the NRA had not included a clause protecting the author's right to publish, I doubt that it would have been possible to arrive at any agreement for revisions of the manuscript that would have satisfied the NRA people and would have satisfied me and our Committee.

The Management Resistance Problem

In the early postwar years, I saw no change in the general indifference or even hostility of most operating managers to our human relations research. To be sure, there were notable exceptions. For example, James C. Worthy (1950, 1984) of Sears, Roebuck and Company had a lively intellectual and practical interest in our research and worked with people from our Committee in developing the Sears attitudinal survey program and on further projects. But such exceptions were few.

At first I attributed our failure to stir up more interest and support from managers to our inability to talk their language, but now I think that was a misdiagnosis. Some of us were able to speak and write in rather clear and simple terms. I think the problem was rather that top management people saw no need to change their styles of management. These were the years following World War II in which the U.S. "great arsenal of democracy" had achieved an enormous international reputation. Productivity teams from all over the world were visiting the United States to learn the secrets of our know-how. As late as 1968, when Japan was already beginning to make serious inroads into our industrial dominance, the French journalist Jean-Jacques Servan-Schreiber published the best-selling *The American Challenge*, in which he claimed that U.S. management people were so much more efficient than those in Europe that the United States was taking over economic dominance of that part of the world.

Since top management people were being told by their admirers elsewhere that they had all the answers, why should they listen to people from the ivory tower who might point out problems they were not mastering? In that era, we barely got to talk to a plant manager, let alone to any line manager at higher levels. We had better access to personnel administrators, who were looking for some new gimmicks that might help them gain status in their companies. But when personnel administrators would say, "What we want you to

tell us is how can we make the workers feel they are participating," we had to answer that we were not into impression management. And when we explained that the way to make workers feel that they were participating was to open up opportunities for them to exert influence on decisions of importance to them, the personnel administrators generally lost interest in further discussion.

The situation in Japan was dramatically different. Having suffered the disastrous loss of World War II, Japanese intellectuals and business and government leaders were looking around the world for new ideas that might help them to rebuild. They discovered the growing academic human relations literature and translated and read the writings of Douglas McGregor, Rensis Likert, Chris Argyris, and others. This led to what Robert Cole has described as "a creative misunderstanding" (personal communication). The Japanese assumed that the participative management styles espoused by these authors were actually being implemented by the leading U.S. companies. They concluded that if Japan wished to compete with the United States and other industrialized nations, it must develop its own system of participation. Thus, whereas the works of the university people were viewed in U.S. management circles as more ornamental than practical, the Japanese went to work seriously to reshape industrial management and labor relations.

For example, Hideo Kawabuchi in 1951 was one of the first contingent of Japanese students to come for graduate study in the United States following World War II. He remained only one year at the New York State School of Industrial and Labor Relations at Cornell, where I had moved in 1948, but in that year he became an enthusiastic convert to human relations and participative management. Returning to Japan, he persuaded an old friend to defect from a regional government program promoting scientific management to enlist with him in converting Japanese management to the human relations approach. They founded the Japan Human Relations Association—which still goes by

that title, untranslated. Today, more than 4,000 industrial companies belong to JHRA. The association has a central office staff of about thirty people, publishes a monthly supervisor's journal, and conducts conferences and publishes other studies designed to promote what the Japanese still call human relations.

But I should not exaggerate the impact of my tangential relationship, through my student Kawabuchi, with these Japanese developments. Probably the Japan Federation of Employers' Associations and the Japan Union of Scientists and Engineers were more influential than JHRA in stimulating and guiding Japan's development of its program of employee involvement (Cole 1985).

In the 1940s and 1950s, the prevailing relationships between unions and management were sharply adversarial. Many managements were still trying to avoid unionization or to undermine their unions. Those managements accepting collective bargaining in principle nevertheless seemed to act as if the relationship were a necessary evil. They were trying to run the companies as much as possible as if unions were not there.

This meant that the New York State School of Industrial and Labor Relations, which had been created by the state government in 1945 to gain knowledge to help both labor and management, was necessarily suspect. In those early years, in some management quarters in New York State this new college was called "the cardboard Kremlin"—"cardboard" because of the temporary quarters we occupied until 1961, and "Kremlin" because of the school's general commitment to the institution of collective bargaining (which, of course, never had any place in the Soviet system).

In that era, professors of industrial relations were assumed to be either pro-labor or pro-management. It was hard to find practitioners who could accept the idea that a professor might be interested in helping the parties to develop mutually advantageous ways of working together.

The Withering Away of Human Relations

In the 1940s and 1950s, those identified with human relations had a dominant influence in laying out what was becoming our field of study. Why did "human relations in industry" give way to "organizational behavior" and other labels? I trace the roots of that shift back to the academic debates of the 1950s, particularly reflected in an exchange of views I had with labor economist John Dunlop (Dunlop and Whyte 1950). Emphasizing the great influence of the institutional framework in shaping the human relations we studied, Dunlop conceded only that our micro-level studies could cast light "in the area of the relation of individuals to organizations" (p. 391). He wrote that "the 'human relations' approach is more or less identified here with the study of communications" (p. 383). He added, "The communication and human relations approach seems to proceed from the premise that conflict can be reduced in industrial relations if individuals have more accurate information" (p. 392).

At that time, I rejected the Dunlop critique as based on a well-meant but misguided interpretation of our work. I had never believed that communication of more accurate information was a major force in reducing conflict. My interest in communication was focused not only on interpersonal interactions but also on the *actions* that followed the interactions. We expected to observe frequent occasions when managers initiated actions for workers, but did workers and union leaders also initiate actions for managers? If so, how often, in what circumstances, and on what types of problems?

I now have a clearer view of the weaknesses of my 1950 position than I did then. I had argued that

[w]e must have a means of dealing with influences from outside the plant. But at the same time, we are not dealing with influences in general. We must study them at the point of contact: where they actually enter the plant. (p. 400)

The weakness of that position is that it

fostered a concentration on human relations as if the interactions were occurring in an economic, technological, and structural vacuum. That is, we had to deal with the external elements when they became so obvious that we could not ignore them; but we dealt with elements outside our social systems framework on an ad hoc basis, having no systematic means of integrating them into our thinking.

Perhaps that confession overstates the case against us. Through the pioneering work of Charles Walker, Robert Guest, and Arthur Turner (1952, 1956) we recognized that the automotive assembly line was one of the most oppressive systems of getting work done that had ever been devised, but we assumed that this mode of production was so economically efficient that it would be impossible to produce efficiently under any other system of work organization. We knew it would be impossible economically to go back to the earlier methods in which craftsmen had built cars, but we did not see ahead to the more flexible systems of organizing work that developed years later. In other words, we treated technology as a constant rather than as a set of variables. (Nor did we learn until many years later the importance of treating *ownership* as a set of variables rather than as a constant.)

We did give some attention to the impact of economic incentives on workers, in our studies of piece rates (Whyte et al. 1955), but we had no way of integrating the economics of the firm into our framework. We knew that the behavior of managers was influenced by the way they interpreted the numbers that purported to reflect the performance of the firm, but, with few exceptions (Argyris 1952), we did not focus on the point where economic analysis and behavioral analysis come together.

In discovering that the formal organization structure did not determine behavior, as the exponents of scientific management had argued, we concentrated particularly on what we then called "informal organization." We neglected the importance of the formal hierarchical and departmental

and divisional structures in shaping behavior.

It was the growing awareness of the limitations of "the human relations approach" that led even some of us old-timers to accept other labels, such as "organizational behavior" or "complex organizations."

From the 1950s to the 1980s

During the past thirty years or so, we have seen a growing interest in organizational behavior research on the part of practitioners. We have also seen a great proliferation of research articles and books.

Do we now have more to offer to science and practice than we did in the days of human relations? Any attempt to answer such a general question within the scope of an article will necessarily be more provocative than comprehensive and balanced. Here I opt to sketch out what I see as major trends in the hope of focusing debates on the future of our field.

What has become the main stream of research in *organizational theory* contrasts with the human relations approach in several important respects:

1. A shift of emphasis from the micro to the macro: from interpersonal relations to formal organizational structures, technologies, and the impact of markets and other environmental factors on the organization.

2. A shift from the study of general patterns of relations toward the definition of variables, and the specification of hypotheses to be tested by rigorous quantitative methods.

3. A shift from intensive interviewing and observational studies toward questionnaire or survey research.

4. A sharp separation between theory and practice, with the researchers generally avoiding any linking of research to practice.

Main stream researchers do not all share the same interests and methodologies, of course, but they have enough in common to be identified as a group by most scholars studying the development of

organization theory (see, for example, Hall 1982).

There is also an alternate stream of development that groups together those concentrating on *organizational change*. In a sense, this research stream arose out of the human relations approach instead of representing a sharp break away from it. Like those of us active in the 1940s and 1950s, the alternate stream researchers reject the separation of theory from practice, arguing that science can best be advanced when the two are linked together. The difference is that today's organizational change researchers have sharper action tools and better theoretical frameworks than we did in those earlier decades.

Although the main stream and the alternate stream are clearly different in some ways, they also have certain interests in common. Researchers of both kinds accept the influence on organizations of formal structure, markets, and other environmental conditions, but the alternate stream researchers go on to study how organizational performance can be improved within those limiting conditions. Both streams have strong interests in worker participation, but they pursue that interest in quite different ways (a point I discuss below).

Until Joan Woodward (1965) came on the scene, the only theory of organizational structures was that handed down by the scientific management school: that for any organization of a given size there was just one best way of designing its structure. The studies of the Woodward group demonstrated that plants with different technologies and work processes required distinctively different organizational structures.

For the main stream (organization theory), the Woodward studies set off a flood of research on organizational structures. Paul Lawrence and Jay Lorsch (1967) extended this analysis into the relations of the organization and its markets. Howard Aldrich (1979) went further to argue that the environment tended to select those organizational characteristics that best fit it.

Those developing the alternate stream (organizational change) picked up the Woodward lead on organizational structures to work on the theory and practice of changing structures, technologies, and social systems so as to fit them more fruitfully together. Having begun his research career in England long before Woodward, Eric Trist went on to formulate the concept of socio-technical systems (Trist 1981). The idea is basically simple: the most effective organizations will be those in which the technology and the organization structure and social processes are designed to fit together. If this idea now seems obvious, note that it departs radically from all of past practice and most current practice. In the past, designers of organizations laid out the technology and assumed that work and social processes must be designed (generally by other people) to fit the requirements of the technology.

From England to Scandinavia and America, Trist and his social systems framework have had an enormous influence on organizational change theory and practice. In Norway, Einar Thorsrud created and guided for many years the Industrial Democracy project, bringing together Norwegian and foreign social scientists with Norwegian workers, managers, and union leaders to learn how to design (or redesign) organizations so as to enhance both economic efficiency and the quality of working life (Thorsrud 1977; Elden 1979). In Oslo in June 1987, hundreds of social scientists and practitioners gathered to honor the memory of Thorsrud and to discuss how best to contribute to the flow of research and action on socio-technical systems.

That the main and the alternate streams have diverged sharply from each other is most readily demonstrated by picking up a textbook from a mainstream social scientist. Consider, for example, Richard H. Hall's *Organizations: Structure and Process* (1982), in which leading figures in the alternate stream are almost completely overlooked. Eric Trist is mentioned only briefly in reference to his earliest study, and Einar Thorsrud and other leading

figures in the alternate stream do not even rate footnotes. Or consider the leading action researchers focusing on the problems of changing leader behavior in organizations: Donald Schön (1983) is not mentioned by Hall, and the only mentions of Chris Argyris (1986) are in connection with his critiques of main stream research.

Can these two streams be brought together? Since researchers in both streams continue to study worker participation in decision making, let us examine the prospects provided by that focus.

Solving the Participation-Productivity Puzzle

Those in both streams would like to believe that increases in worker participation are positively correlated with increases in productivity. Research does support the conclusion that job satisfaction is favorably affected by worker participation, but main stream studies on the participation-productivity relationship have yielded mixed results (Brett and Hammer 1982; Hammer 1983).

What accounts for the ambiguous findings? Apart from the technical difficulties of measuring productivity and generalizing across a wide range of technologies and organization structures, we face three major problems: how to define participation; how to determine to what extent participation has taken place; and how to link a particular form of participation with a specific target for productivity improvement (or cost savings).

Regarding the first problem, participation can and does occur in many different forms, determined both by the particular problems being addressed and by the means chosen to handle those problems. For example, instead of just giving orders, the boss may informally consult individual subordinates before acting. Or he may hold group discussions and allow his decisions to be influenced by those discussions. In practice, there are enormous differences in time devoted to employee involvement activities. In the typical quality circle in the United States, worker members may hold a one-hour meeting

every week or every two weeks. At the other extreme, Xerox study action teams of workers and management people spend full time for up to six months working out concrete solutions to pressing cost and productivity problems (Lazes and Costanza 1984).

The scope of the problems addressed by worker participation may also vary widely. At one extreme, worker involvement may be limited to housekeeping issues (company parking lot, vending machines, cafeteria, etc.); at the other extreme, workers may be involved in decisions on key industrial relations issues (incentive rates, gains sharing, work redesign, work rule changes, and productivity/cost issues).

Regarding the second problem, most main stream researchers have finessed the task of determining whether participation has taken place by measuring participation subjectively: asking survey respondents to what extent they *feel* they have been participating. The measurements can be refined further by asking how they *feel* about their participation regarding specified issues. This approach gives us a rich yield of numbers, but those numbers do not tell us what *behavior* has given rise to the subjective responses.

With exceptions that are just beginning to occur, research has not focused on participation projects designed to produce specific and measurable productivity improvements. The research design has been based on the assumption that participation will have an *indirect* effect on productivity. Researchers cannot hope to accurately measure direct effects on productivity when the parties have not established any target for their efforts. In that case, if productivity increases occur following a particular form of participation, we may assume only at our peril that participation led to this result, since it may well be that other variables intervened to produce a spurious relationship.

Under these conditions, it should not be expected that the main stream research strategy will yield any meaningful results. Tracking the impact of participation through measures keyed to any single

variable or any set of variables is bound to be fruitless.

What other research design is more promising? We can seek answers through *patterns* rather than through discrete variables. We begin with the recognition that, although participation could conceivably occur in an almost infinite variety of forms, by now the work of practitioners and researchers has sorted out a small number of forms that appear to have promise (Lawler 1986).

Consistent with those considerations is the following research design. We examine a case in which people in labor and management are implementing a *participatory action research strategy* designed to achieve certain concrete objectives in productivity improvements or cost reductions. We then describe systematically the actions and interpersonal interactions constituting that strategy and, finally, measure whether and to what extent the objectives have been achieved. If the strategy has been successful in those terms, we have established a direct relationship between participation and productivity. (To be sure, it is possible that the outcome could be produced by factors not observed by the researchers, but such an event is much less likely when we are studying the attainment of explicitly specified goals than when we are studying the indirect impact of participation on productivity.)

The example cited above is not simply hypothetical. Such a case occurred in the wire harness department of a Xerox factory (Lazes and Costanza 1984; Kochan et al. 1984:13-33). In the wire harness case, the challenge to a labor-management study action team (working full time for six months) was to save 180 jobs through reducing departmental costs by \$3.2 million—more than 25 percent. People in higher management could not believe that such an outcome was possible, yet the team overshot the target, producing a plan to save \$3.7 million. While implementing the wire harness plan, Xerox and the Amalgamated Clothing and Textile Workers Union proceeded to carry out (with similar ultimate success) the same participatory action strategy with other depart-

ments in which Xerox's costs substantially exceeded those available from vendors. Xerox's experience has led Cornell's Programs for Employment and Workplace Systems (PEWS), under the leadership of Peter Lazes, to make this general strategy the central thrust in PEWS projects currently being carried out with labor and management in other companies.

The significance of this participatory action research strategy is not limited to establishing a relationship between participation and productivity, important as that may be. These cases demonstrate the importance of linking up human relations with the economics of the firm. Not only are we incorporating cost measures into our research, we are also strengthening our understanding of the way management and union people react to economic measures. Of course, this breakthrough simply points to future theoretical and practical problems to be studied—but that is the nature of scientific progress. We can describe the social processes leading to the economic results, and we can report the economic figures, but to behavioral scientists the technical economic and engineering analysis that went into the determination of the cost savings figures remains a black box. A next step in the advancement of organizational science is to open that black box.

A colleague in managerial economics tells me that there have been no basic advances in teaching or research in accounting in business schools since 1929 (personal communication from Alan McAdams; see also Anthony 1986). Those conventional methods are clearly not well adapted to solving the cost accounting problems of modern industry in a highly competitive environment. The Xerox study action teams have apparently developed innovative methods of accounting and engineering analysis that have made it possible to save millions of dollars and preserve hundreds of jobs. Though we now know the results in terms of dollars and jobs saved, the technical and financial analyses generating those results are filed away in company reports. A university team could work with members of the

study action teams to abstract from their reports the methods of technical and financial analysis they developed. The data thus recovered could serve as the basis for future teaching and research in accounting, engineering, managerial economics, and organizational behavior, opening up exciting new fields of interdisciplinary study.

For such new research, the alternate stream has clearcut advantages over the main stream in several respects. First, main stream research tends to be discipline-bound. The planning process begins with a review of the literature, which may go somewhat beyond the scholar's discipline but tends to be concentrated within that discipline. Variables are specified and hypotheses formulated so as to maximize the chance that the research will build on the body of knowledge within the discipline. Since very few problems in the complex field of modern industry lend themselves to solution with the tools of any single discipline, this disciplinary focus is bound to be counterproductive, theoretically and practically.

In contrast, alternate stream researchers approach the field with a general interest: for example, the problem of reconciling management's aim to cut costs with the union's aim to maintain employment. The researcher then looks over the scene to find cases in which the people in the organization are trying to find new and interesting ways of coping with their problems. He or she approaches the gatekeepers to see whether the researcher's involvement in the search and coping process promises to be mutually advantageous. If so, the researcher then seeks to work out a role that combines gathering of data with participation in the change process. The researcher must then stretch his or her mind to grasp at least some of the general lines of analysis in other disciplines—or else collaborate with specialists in those disciplines. As this type of research breaks through traditional disciplinary barriers, it is bound to have an unsettling but stimulating effect across a wide range of disciplines.

The alternate stream strategy also

makes it possible to study some change processes that would not have occurred without the researcher's personal involvement. For example, it was Peter Lazes, serving as consultant to both union and management at Xerox, who first proposed the idea of study action teams as a means of balancing management's aim to cut costs and the union's aim to preserve jobs.

Industrial Relations Research in the 1980s

In the 1980s, we do our research and consulting in an industrial relations environment drastically different from that of earlier decades. To be sure, there are many managers who see weakening of the union movement and pressures of international competition as enhancing opportunities to maintain a "union free environment" or to undermine existing unions. Those academics who choose to do research or consulting with such companies cannot escape the charge that they are anti-union. On the other hand, we find a number of companies—and even major companies—whose managements have decided not only to tolerate unions but actually to try to work jointly with union leaders and workers on cooperative activities designed to strengthen the firm's competitive position and thereby to save jobs.

Those who study jointly developed cooperative programs find that they are now no longer regarded by the parties as either pro-labor or pro-management. Accepting money from a company for research or technical assistance does not automatically brand a professor or student as anti-union. Even a project wholly financed by the company can be accepted by workers and the union—provided that the union leaders are fully involved with management in providing access and guidance to the researchers and know that, if they refuse to endorse it, that project will not go forward.

The turbulence of the industrial relations scene in the 1980s is also accompanied by unprecedented changes in the ownership of corporations, ranging from battles to take over corporations to the

rapid growth of employee ownership in various forms. When industrial relations and managements and unions are in such a state of rapid change, there is an urgent need for social scientists to go into the field so as to document new lines of development and help practitioners to understand trends and possibilities.

In organizational behavior, we can no longer say that we could help management and labor to solve their problems if they would only listen to us. They are now ready to listen and to work with us. The question is: will we be up to the challenge of providing them with the help they so urgently need?

A distinguished sociologist, reviewing the current state of his discipline, has written,

the dominant mood today is one of discouragement—a feeling that researchers go around in circles, that conceptual clarity is lacking, that theory is uninformed by empirical findings, that blind empiricism is rampant, that knowledge fails to accumulate, and that the former consensus about the core of the discipline has largely broken down. (Inkeles 1986)

I believe the same judgment applies to research in the main stream of organizational behavior, whether the research is done by sociologists or psychologists. The refined measurement of discrete variables at the micro level and the further exploration of macro level elements appear to be adding constantly to our knowledge—until we ask what use can be made of that knowledge. If asked how they can help union leaders or managers to solve the problems of their organizations, main stream researchers would have to fall back on broad, general statements regarding

the institutional and environmental constraints within which the practitioners are working. Since the practitioners are likely to be more attuned to their organization's particular constraints than are researchers, they are unlikely to find such orientations useful.

I am not arguing that every research project should yield significant practical implications. But even those most dedicated to pure science goals would hardly deny that advancing science should *eventually* yield payoffs in practical applications. As I see it, the main stream has been running now for about 30 years. Rather than generating useful knowledge, the main stream seems to me to be running dry.

I see the alternate stream in a disorderly but vigorous period of growth. When the alternate stream is ignored by those in the main stream, that is unhealthy for the future development of organizational behavior. Criticisms of aspects of theory and methodology used in the alternate stream can be healthy, but they should not be based on the implicit assumption that there is just one best way to advance a science of organizational behavior. Is it possible to do acceptable scientific research focused on participatory action strategies? If the answer from main streamers is negative, then clearly the two streams can never come fruitfully together. If the answer is affirmative, then the quantitative skills and theoretical sophistication of the main streamers can greatly strengthen the scientific base for the study of participatory action strategies, to our mutual advantage.

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