Participatory Action Research and Action Science Compared

A Commentary

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THE DILEMMA OF RIGOR OR RELEVANCE

Whyte, Greenwood, and Lazes frame their discussion of PAR in terms of the desirability of pluralism in social science. They argue for the incorporation of PAR, along with normal science, in the social scientist's "kit of tools." We take a different tack. In our view, social scientists are faced with a fundamental *choice* that hinges on a dilemma of rigor or relevance. If social scientists tilt toward the rigor of normal science that currently dominates departments of social science in American universities, they risk becoming irrelevant to practitioners' demands for usable knowledge. If they tilt toward the relevance of action research, they risk falling short of prevailing disciplinary standards of rigor.

From the action researcher's perspective, the challenge is to define and meet standards of *appropriate* rigor without sacrificing relevance. And for this purpose, action research needs three things: a way of representing research results that enhances their usability, a complementary way of construing causality, and an appropriate methodology of causal inference.

In our review of the Whyte, Greenwood, and Lazes article, we shall explore the meaning of these three conditions. Let us begin, however, by defining *action research, participatory action research,* and *action science. Action research* takes its cues—its questions, puzzles, and

problems—from the perceptions of practitioners within particular, local practice contexts. It bounds episodes of research according to the boundaries of the local context. It builds descriptions and theories within the practice context itself, and tests them there through *intervention experiments*—that is, through experiments that bear the double burden of testing hypotheses and effecting some (putatively) desirable change in the situation.

Hence, action researchers are always engaged in some practice context. In Geoffrey Vickers's phrase, they are "agents experient," and their research results tend to be couched in everyday language—often in metaphors of "optimal fuzziness," such as Kurt Lewin's "gatekeeper" or his distinction between "democratic and authoritarian group climates." Although action researchers may make claims to generalizability across local contexts, their generalizations are unlike the "covering laws" to which normal social science aspires; they do not describe relationships in which the values of a group of dependent variables are uniquely determined by the values of a group of independent ones. Rather, their generalizations tend to describe thematic patterns derived from inquiry in *one* setting the valid transfer to other settings of which depends on confirmation there by further experiment.

Participatory action research (PAR) is a form of action research that involves practitioners as both subjects and coresearchers. It is based on the Lewinian proposition that causal inferences about the behavior of human beings are more likely to be valid and enactable when the human beings in question participate in building and testing them. Hence it aims at creating an environment in which participants give and get valid information, make free and informed choices (including the choice to participate), and generate internal commitment to the results of their inquiry.

Action science is a form of action research that, although it shares the values and strategy described above, places a central emphasis on the spontaneous, tacit theories-in-use that participants bring to practice and research, especially whenever feelings of embarrassment or threat come into play. These theories-in-use we call "Model I." They include strategies of unilateral control, unilateral self-protection, defensiveness, smoothing-over, and covering-up, of which their users tend to be largely unaware (Argyris and Schön, 1974). And these strategies tend, in turn, to undermine attempts to implement inventions based on the discoveries of action research; indeed, they often distort the discoveries themselves—all in ways of which researchers and practitioners tend to remain

unaware, not because of ignorance but because of a skillful adherence to Model I theories-in-use and virtues such as "strength" (construed as dominance, or unwillingness to be swayed by others) and "caring" (construed as unilateral protection of others) learned early in life.

In our commentary on the Whyte, Greenwood, and Lazes article, we shall use the action science perspective to point out certain practical limitations and conceptual gaps. We want to emphasize, however, that we see action science and PAR as members of the same action research family. In the broader world of social science, PAR and action science are aligned in a basic and consequential conflict with normal social science. What they have in common far outweighs their differences. Nevertheless, as we shall try to show, an understanding of their differences illuminates both the potentials and limits of PAR and the future development of action research as a whole.

We shall illustrate these differences by reference to the Xerox case described in the Whyte, Greenwood, and Lazes article. We begin by reviewing its main features.

THE XEROX CASE

The authors place the Lazes intervention within the framework of attempts by organizational researcher-consultants to bring workers and managers together to diagnose and solve organizational problems. Starting with the proposition that interventions "limited to the shop floor" tend to yield only marginal increases in productivity and quickly exhaust participants' interest, the authors ask, "How do you get from the conventional shop floor focus to the much broader sociotechnical economic focus?" and "How do you do so without opening up Pandora's box?"

The breakthrough occurred, the authors tell us, when Xerox proposed to close its wire harness department, outsourcing its production and eliminating some 180 jobs, in order to save $3.2 million dollars per year. Lazes proposed an alternative: creating a cost study team that would "study the possibilities of making changes internally that would save the $3.2 million and retain the 180 jobs." In order to gain the willingness of labor and management to participate in this venture, Lazes built on the "trusting relationship" he had previously established with them and on their prior experience as participants in regular meetings in which management had invited union leaders to discuss its

strategic business plans. Lazes interviewed labor and management representatives to draw out their sense of the risks involved in the CST venture and to help them "think their way through the costs and benefits of accepting his proposal."

Once the CST had been accepted by both labor and management, a joint team of eight people was set up and charged with working on the problem over a six-month period. Lazes then operated as a "consultant/ facilitator to identify the problems . . . caused primarily by blockages, the development of adversarial positions, turf issues, immobilization and a general loss of the sense of control and ownership of the organization's activities."

> To address this "largely reactive behavior" he . . . emphasized "changing the shape of the box"; throwing out a broad array of options and tactics that temporarily disorganized the blocked system and might eventually lead to a sense of joint responsibility, openness and a sense of control and ownership, if properly handled. In a word, he attempted to facilitate a transition to a more reflective and action mode of organizational behavior.

Management offered the team access "to all financial information they might require" and invited them to consider "any and all changes." Solutions were found, we are told, in two areas. First, the team discovered data that revealed the high costs of training new workers—in response to the "bumping" by seniority that was sanctioned by the labor contract—and was able to project a substantial savings through "stabilizing the work force . . . providing the parties would then negotiate contractual changes to make this possible." Second, the team discovered inappropriate allocation of overhead charges to the wire harness division the removal of which would also achieve substantial cost reductions. The two measures, taken along with other changes, promised to achieve savings in excess of the required $3.2 million.

As a consequence, CSTs were established in three other units at Webster, resulting in productive changes that included worker participation in the R&D process and in plant redesign. These successes "made it possible for the union and management to work out a new labor contract providing employment security for workers in the bargaining unit" and led management to commit to establishment of a CST before any large-scale lay-offs were undertaken.

The authors note that this "powerful organizational learning process" did not spread to Xerox plants in locations other than Webster. They cite the "extremely favorable circumstances" at Webster: a history of

labor and management relations "of mutual trust and respect built over years of joint problem solving" coupled with "urgent economic needs" that "had not reached the crisis stage" that "demanded quick and drastic rescue measures."
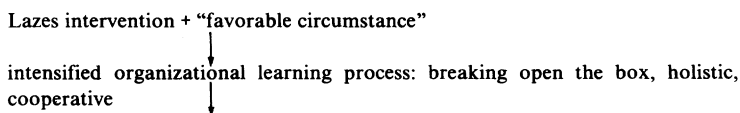
THE XEROX CASE FROM
AN ACTION SCIENCE PERSPECTIVE

At least three kinds of important consequences are claimed for Lazes's intervention. They are (1) cost-savings to Xerox and retention of employee jobs, (2) restructuring of the cost accounting system and labor contract, and (3) an experience that would act to up the ante as to the kinds of changes that would be possible in the future (this is what the authors refer to as "the intensification of organizational learning" at the Webster plants).

We want to raise two main issues about these claims. The first has to do with the inferences, explicit and implicit, by which the authors link Lazes's intervention to its consequences. They do not consider alternate explanations and thereby miss an opportunity to demonstrate the kind of rigor appropriate to action research.

The second issue has to do with certain questions the authors do *not* ask: Why was the "shape of the box" so narrow to begin with? Why was the organizational system so "blocked"? In short, why did the organizational learning system of the Webster plants have the features attributed to it prior to the Lazes intervention described in this article? If this question were taken seriously, we believe, it would suggest important limits to Lazes's intervention and cast doubt on the likely durability of the organizational changes he is said to have facilitated.

There is a distinction between these two issues, but they are also interdependent in certain ways, as we shall try to show.

The authors' treatment of causal linkages. We reconstruct the following causal chain associated with Lazes's intervention, drawing on explicit or implicit elements in the authors' article:

Lazes intervention + "favorable circumstance"
$\downarrow$
intensified organizational learning process: breaking open the box, holistic, cooperative $\downarrow$

achieved (at least one-time) savings of over $3.2 million at wire harness plant, saved 180 jobs, significant increase in organizational productivity

established CSTs at three other Webster departments

↓

further organizational improvements: more efficient R&D cycles through worker involvement, more efficient plant redesign

↓

management commits to worker security at Webster, agrees to CST before lay-offs

What we seem to have here is a threefold claim:

(1) Important shifts in organizational learning occurred: Hidden sources of data were opened up, participants' thinking was broadened to include the entire socio-technical system of the firm, and cooperative problem solving took place among groups (especially labor and management) that were usually in an adversarial relationship to each other.
(2) These shifts in organizational learning caused the favorable first-order results: cost savings, job retention, and the like.
(3) Lazes's intervention, coupled with "favorable circumstances"—an impending crisis clearly visible to all concerned, and a prior history of labor/management relations characterized by "mutual trust and respect"—caused these shifts in organizational learning.

Let us take item 1 as given by observation, although even here we may wonder whether *all* relevant observers—other management and labor participants—saw the matter as Lazes did. We have no information on this point since the authors tell their story only from the interventionist's perspective.

With respect to item 2, the causal attribution is supported in this article only by the authors' claim. There is no consideration, as far as we can see, of other plausible causal accounts of the first-order effects. One possibility is the following: "The change was really a political one; management made a decision to open up hidden data, and so on, in order to achieve labor peace. In other words, the story was really one of adversarial pressure rather than organizational learning." The authors may easily be able to refute this alternative on the basis of evidence available to them. (We shall suggest another version of it in the following section, when we have introduced some additional terms.) Our point here is that the authors do not try, on the basis of their special knowledge of the case, to construct and test plausible alternatives to their "organizational learning" hypothesis. Indeed, they do not treat it

as a hypothesis at all, but as an obvious interpretation of the data of the case. In this, we believe, they follow the practice customary in most published papers written in the traditions of organizational development and sociotechnical systems. We are not arguing that their interpretation is mistaken but that a failure to seek out countervailing evidence leads too easily to a belief in the efficacy of organizational learning—in effect, to an ideology of organizational learning.

With respect to item 3, we are in worse shape. We know very little about what Lazes actually did. We are told that he proposed the structure of the CSTs within which the new organizational inquiry occurred, facilitated the process by which management and labor considered the risks and benefits associated with the CST venture, and thereby increased the likelihood of its acceptance. We are told that he facilitated the work of the CSTs through process consultation—that is, he "provided training in group methods and in problem analysis," and in the early stages of the intervention sat in as an observer in meetings of the CSTs but "never intervened except to help the parties to resolve an impasse."

We do not know, however, *how* Lazes trained the participants, or how he helped to reduce resistances or resolve impasses. Lazes's way of intervening remains, for the most part, a black box, and there is no attempt to present other accounts of his intervention and its role in organizational change at the Webster plants. In our experience, participants and observers of an organizational intervention often see things in very different ways. The "Rashomon effect," whereby different observers tell internally compelling but incompatible stories of organization change, is a hallmark of organizational reality. The surfacing of such stories provides an opportunity for testing the interventionist's story, or reconciling it with others.

The authors make a brief reference to "quasi-experimental method" and to management's interest in testing claims made for intervention. But it is clear that at least what Donald Campbell describes as quasi-experimental method, which very much involves proposing and testing alternate causal accounts of phenomena, has not been carried through here—or at least, not recorded in the article.

The issue of "appropriate rigor" is important from the point of view not only of research methodology but also of learning from the experience of the intervention. Prospective imitators of the Lazes intervention would need both an operational description of what Lazes did *and* a critical inquiry into the causal attribution of his achievements to certain features of his way of intervening.

We wish, finally, to comment on the theoretical advance the authors describe and attribute to the intervention. They were led, they say, to pose a new question and to propose a new theory in answer to that question. The question: "How come . . . no consistent relationships between participation and productivity have been reported in the literature, whereas in Xerox we could report participation leading to cost savings of 25% to 40%." And the answer: an interesting elaboration of various kinds and levels of participation, and a proposed shift in the definition of "productivity" from "productivity per worker" to "organizational productivity," meaning "the relationship between the total costs charged to (a) unit and the value of the output of that unit." The main thrust of the article, as far as Xerox is concerned, is really this shift toward a more broad-gauged, sociotechnical approach to participation coupled with the shift toward a more holistic definition of productivity.

There is certainly an interesting observation, with which we have no quarrel, except for the question of how we know that the shift toward "holism" really did account for the first-order effects described (as above), and how we are to understand the relationship of this theory building to the intervention itself.

Was the new idea about productivity *suggested* by the intervention, or was its development an important part of the intervention itself? Surely there is nothing wrong with the former. But it is the latter that characterizes some of Lewin's best examples. In these instances, the development of the theory is critical to the effectiveness of the intervention, and it is the intervention that *tests* the theory. Did this happen in the Xerox case? The case might be made that it did, but we can't tell for sure. The issue seems to be important for what we intend by the relation between "action" and "research" when we speak of action research (or action science, for that matter). It is not merely a question of *temporal priority* but rather one of the *function* of "research" (theory building and testing) within and in relation to "action" (organizational intervention). On one view, organizational intervention is said to be suggestive or evocative of theoretical insights. On the second, it is said to have a distinctive function as a context for theory building and a means of theory testing.

Question the authors do not ask. As mentioned above, we do not have access to what Lazes actually said and did in the course of his intervention at Xerox. We do not have relatively directly observable data of what actually happened during the meetings. The reason we seek such data is that only by having access to actual behavior can we infer

the interventionist's theory-in-use. At the moment, the most we can infer is what Lazes (and others) believe he did. To use our language, we can only develop a picture of Lazes's espoused theories. Moreover, if our knowledge is limited to espoused theories, we are also limited to the reasoning that underlies these theories. It is difficult to provide an independent test of causal relationships embedded in the case if we are limited to the ideas and reasoning espoused by the interventionist.

As we read the material, we find that we can piece together a more complex causal account than the one described in the case. This account, if valid, suggests that Lazes may well have produced successful first- and second-order consequences, as claimed, but that as he did so he bypassed the organizational contextual factors that helped to create the problem in the first place.

Since we do not have transcripts or notes of Lazes's actions, we will build our case upon the text provided by the authors. We hope to show that our conclusions are plausible by using their statements as our premises and by making inferences that they, and the readers, will find worth consideration. For example,

(1) The authors state, "It was obvious from the outset that such a plan would involve abandoning the rules that declared certain topics of study or discussion out of bounds." They state that the CST was formed with individuals who were not handicapped by "preestablished interpersonal hostilities."

Abandoning rules that declared certain topics out of bounds makes sense. But we would wish to peel the causal onion by at least one more layer. What were the causal factors that made it possible to have rules the effects of which were antilearning? What rules existed in the organization that sanctioned making such topics undiscussible?

If it is possible to abandon the rules, as Lazes was able to do, we would wish to explore how he was able to accomplish this feat. Our inference is that he used the crisis situation to say to both sides, "O.K., if you want to save jobs, or reduce unacceptable costs, then the undiscussibles have to be discussed." This intervention "works" because everyone's back is up against the wall, and is, in our terms, a "Model I" intervention. It does not require the players to explore how they got themselves into such a crisis in the first place.

What were the causal factors that produced the "preestablished interpersonal hostilities"? It makes sense to reduce these factors, as Lazes reports he did. The method used to reduce these factors was, in

effect, to get rid of the old broom and start fresh—again, without leading the players to explore how the "old broom" (or the old "shape of the box") had been created and sustained.

> (2) The team discovered that there were high training costs caused by the bumping procedures required by the existing labor contract. The team was able to show that these costs could be reduced by stabilizing the work force, which could be done by altering the contract. Moreover, the CST identified allocations of overhead charges that were inappropriate, and recommended their removal.

The question arises in both cases, What were the factors that had made these issues undiscussible prior to Lazes's intervention? What did management and labor do that made it likely that these discoveries would *not* be made and that solutions to the discovered problems would *not* be found without the threat of an impending crisis?

In our own research, we find that whenever undiscussibles exist, their existence is also undiscussible. Moreover, both are covered up, because rules that make important issues undiscussibles violate espoused norms of managerial stewardship and union practices (in George Meany's terms, "helping management act more efficiently").

These cover-ups, and their cover-up, are indications of organizational *defensive routines*, which may be defined as any policy or practice that prevents organizations (and their agents) from experiencing embarrassment or threat *and* at the same time prevent them from identifying and reducing the causes of embarrassment or threat (Argyris and Schön, 1985). Defensive routines, at any level, are antilearning.

Yet the Xerox case is described as one in which organizational learning occurred. The puzzle can be solved by introducing a distinction between "single-loop" and "double-loop" earning. In the former, the actions that produce errors are identified and changed. Thus, for example, the action, inappropriate allocation of overhead charges, was changed and costs allocated to the wire harness department were reduced. Double-loop learning would ask, How come the inappropriate allocations were allowed to go on for years? Did the cost accountants know the allocations were unfair? If so, how did they get away with making them? Did the management sense that the cost allocations were arbitrary? If so, what led them to continue the practice?

In the case of bumping, what led the union and the management to continue a process that led to unnecessary and costly training? Was this due to a tacit labor-management agreement to keep the peace? If so,

what theory of industrial relations was served, if its propositions have to be renounced precisely when they are most needed, namely, during a crisis?

There is an interesting feature about organizational defensive routines that stems from their being undiscussible and the undiscussibility being undiscussible. It is very difficult to manage them. They continue to exist and proliferate because they are relegated to the realm of "underground management" and all sides tacitly agree to this state of affairs. As a result, organizational defensive routines often are very powerful, yet there is, to our knowledge, no formal managerial policy protecting them.

Under these conditions, defensive routines can only begin to be managed if they are surfaced. To date, the most likely condition for surfacing them is a crisis that requires that they be engaged. Since it is difficult to get management or labor representatives to admit that they have been colluding in such activities, it is not surprising that in the Xerox case the surfacing occurred when a new team was selected and when corporate management promised immunity.

Now back to Lazes's intervention. It is plausible that he succeeded in getting the two parties to make discussible the hitherto undiscussibles because of an impending crisis plus the apparently genuine commitment on the part of management not to punish local managers or workers for collaborating to solve problems. If so, the basis for his success is an intervention theory that has been practiced for many years. Its premise is, "Wait until there is an undeniable and unhideable crisis, then bring in a fresh team, give them the power to look into the box and change its shape if necessary." Lazes could apply genuine pressure on all sides because there was a crisis that no one could deny. But crisis management is not new (although the kinds of organizational learning described in this case are by no means the only or the usual response to crisis). Moreover, there is little reason to expect that changes effected in anticipation of a crisis would endure for very long beyond that crisis.

The authors frame Lazes's task as one of getting from the conventional shop floor focus to the much broader sociotechnical economic focus "without opening up Pandora's box." It is our hypothesis that the moment Lazes chose not to open up Pandora's box, he chose to bypass the organizational defensive routines we have described above. From a single-loop learning perspective, this choice made some sense, because the players are probably not skilled at dealing with the challenges that arise when double-loop learning is attempted. The irony, however, is

that double-loop skills can be learned, in our view, so that the Xerox case could have been used by those involved not only to save jobs but to help the Webster plant, corporate management, and union leaders to build an organizational system in which continued learning—of single- and double-loop kinds—would be possible.

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