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**Looking Backward and Forward
at Action Research**

CEO Publication
T 93-11 (231)

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University of Southern California - Los Angeles, CA 90089-1421 (213)740-9814

To appear in Human Relations, 1993

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INTRODUCTION

The recent Special Issue of Human Relations on Action Research (February 1993, volume 46, issue 2) generated commentaries by Jean Bartunek, Frank Heller, Iain Mangham, and Peter Reason (October 1993, volume 46, issue 10). This paper, by the authors of one of the papers appearing in the special issue (Ledford and Mohrman, 1993), continues the discussion.

Our focus here is on the larger issues raised in the commentaries. Although we cannot resist responding to a few of the specific statements made about our paper, we will attempt to take a broad perspective on the practice of action research. We find considerable common ground between our views and those expressed by the commentators. We also find some areas of distinct difference. We will attempt to sharpen the dialogue by highlighting areas of agreement and disagreement. In so doing, we should make it clear that we speak for ourselves, not necessarily for our colleagues who contributed to the special issue.

We will focus on several broad topics. First, what is the contribution of the special issue to the field of action research? Second, what does it mean for practitioners to be co-researchers or co-investigators? Third, what do the papers tell us about the action in action research? Finally, what issues do the papers raise about methodology, and more fundamentally, our epistemology of science?

KEY CONTRIBUTIONS OF THE SPECIAL ISSUE ON ACTION RESEARCH

In order to assess the papers presented in the special issue in a balanced fashion, one must appreciate their virtues as well as their weaknesses. However, there is no obvious agreement among the four commentators about the main strengths of the papers as a set.

Bartunek notes that past action research activity has been heavy on action and light on research; in particular, conceptual contributions have been limited. She concentrates her analysis on the conceptual advances offered by the papers. She concludes that action research is maturing, in that most of the projects were based on an explicit conceptual model, and that to varying

degrees the model was expressed in practice and there was learning about the model in various cases.

Heller indicates that the contribution of the papers lies on the action side of action research. He states that the various authors "have documented a potentially very powerful way of creating the propensity for system change through in-depth networking over a number of client entities."

Reason cites the focus of the papers on building "communities of inquiry" as the major contribution. In order for action research to occur, he argues, such a community is necessary and it must be capable of effective communication and self-reflection. Each paper offers an example of human organization coming together for joint investigation.

Only Mangham apparently finds nothing commendable in the issue. He occasionally makes a faintly favorable comment about a particular paper, but on the whole finds the set of papers "disappointing." His tone suggests that the papers are deserving of sarcasm and ridicule.

In our view, all four commentators missed the most important contribution of the special issue papers. That contribution is a significant expansion in the universe of published action research cases. Although action research has been described and practiced for about five decades, publication of research based on this approach has been surprisingly limited. There is no question that action research has become well established as a way of generating action in organizations, and indeed many organizations and even many consulting firms ably practice something akin to action research. However, we do not see action research as an especially vibrant tradition in the *research* literature. Rather, it is a splinter movement in the social sciences.

It is telling that there are so few clear examples of the genre. Whereas "normal" social science articles fill numerous journals each year, the number of solid examples of action research in the literature has totaled far less than one per year during the last fifty years. The number is small indeed when we acknowledge Heller's accurate comment that many prominent examples of what is supposedly action research, such as the Trist and Bamforth coal mine studies, are in fact other forms of field research that generate actionable knowledge.

Thus, the major contribution of the special issue is that it generated five cases of action research. That alone makes 1993 a landmark year for action research (but, we trust, not its high-water mark). The hope of all the authors of the special issue is that it will encourage others to emphasize the knowledge generation aspects of action research, and that it will reconfirm the willingness of our journals to publish action research reports.

The commentaries highlight issues that need to be better explored in the field, including conceptual linkage between action and research and conceptual development related to issues being researched, better methodologies, a clearer epistemology, and a better understanding of how social reality is constructed between organizational actors and researchers. We agree that all of these things are desirable. However, discussion of these topics is mere navel-gazing unless it is played out in the context of specific cases of research. The most urgent need of the field of action research is to increase the number of high-quality exemplars.

WHAT IS A CO-RESEARCHER?

Bartunek raises an interesting question: If the subjects of these papers are also "co-researchers," "co-investigators," or "research partners," why are they not also co-authors of any of these papers? She correctly points out that participants in the research are likely to have different perspectives than the researchers, and can contribute to scholarly understanding by sharing their perspectives. She notes that there was no participant co-author in this set of papers, and that only Greenwood and Levin present evidence of scholarly collaboration with participants in other settings.

We agree with Bartunek's premise that it is desirable to involve our "co-researchers" throughout the research process, including the production of research papers. However, collaboration in the production of knowledge can take a variety of forms. Even when we stop short of co-authoring research papers with practitioners, practitioners tend to be involved in developing and testing ideas in the research. This takes place in such forums as informal discussions, internal meetings of design teams, meetings and conferences held inside the client organization, and joint presentations at conferences relevant to the topic of change, such as the

Ecology of Work conferences. In these forums, we document participant perspectives and subsequently integrate them into interpretations that become part of an iterative, ongoing learning process. Even when we do not have practitioner co-authors, we feel that the insights of our practitioner co-investigators have played a critical role in the development of our understanding, and theirs. We would be startled if that were not the case with all the authors from the special issue.

On occasion, we also have published the results of action research with clients as active co-authors (e.g., Ledford & Bergel, 1991; Ledford, Tyler, & Dixey, 1991). Our experience with practitioner co-authors is positive. In particular, practitioners usually have a much more finely-grained sense than we do of political dynamics, of nuances of cultural meaning represented in particular events, and even of the details of key events. Thus, our experience indicates that collaborating with practitioners generates richer, more accurate case studies. However -- and here's the rub -- we are probably more interested in having practitioners as co-authors than our clients are in being co-authors. A strong rooting in the literature, a focus on theory, attention to methodological details, the blind review process, and the persistence needed to generate one or more revisions seem to hold no fascination for most practitioners.

There also are practical difficulties. The organization can choose to remain anonymous when only the researchers' names appear as authors. This was the choice of our client, for example, in the "Consumer Products" case in the special issue. Practitioner co-authorship automatically surrenders anonymity, which the organization may prefer for perfectly legitimate reasons. This is particularly the case where the client organization is a nested part of a larger organization that may not share the goals or appreciate the mix of costs and benefits generated by the project. Also, practitioner co-authorship may play into unhappy political dynamics in the organization. For example, other organizational members may view a practitioner author as grabbing undue credit through co-authorship. Also, an academic researcher may say things without penalty as an outsider that may jeopardize the career of a practitioner-author, especially

when the paper criticizes organizational peers and superiors. These conditions are part of the reality of organizational life, and must be recognized as such.

Nevertheless, we are sympathetic to Bartunek's theme and we will continue to welcome practitioners as co-authors, when that is a meaningful option. We also would like to see more collaboration between researchers and practitioners in the generation of new knowledge. On the other hand, we do not expect any sudden change in the number of practitioner co-authors in action research reports. Also, we are less concerned about whether practitioners are co-authors than whether they are actively involved in the overall research process. Even here, however, Bartunek's comments imply a useful guideline. It would be helpful for future research reports to explicitly discuss the nature and extent of client collaboration and to indicate how the ideas presented have or have not been shaped by the client.

THE ACTION IN ACTION RESEARCH

The commentaries raise two issues relevant to the action side of action research. Heller raises the first issue by placing action research as represented in the special issue within the context of five research types. He argues that action research is only one of several legitimate types of research that produce actionable knowledge, and that other types are more appropriate in some circumstances. We find his classification scheme useful, and agree with his point. We conduct research that falls at several points along Heller's spectrum. He further argues that some authors appear to reject "traditional" action research in favor of the "emergent" type captured by the papers. The emergent type is characterized by, for example, very large-scale projects involving complex change efforts in loosely organized settings. We endorse both older and newer types of action research, but we especially welcome new exemplars that shed light on how action research needs to be conducted by organizations and learning communities grappling with complex, large-scale change. We expect that such organizations will increasingly employ action research in the future.

A second issue concerns whether the cases generate action, and if so, what type of action. Bartunek and Heller both note that the practice of action research has been devoted more to

action than research, and both make favorable comments about the scope of the action that is depicted in these case studies. Reason and Mangham, arguing from a very different set of perspectives, are far more critical.

Comments about our own paper depict the gap in perspectives. Bartunek finds in our paper "a well-articulated conceptual model." She concludes that, "Overall, this paper makes a careful conceptual contribution. It ties proposed and actual changes to theoretical material and describes learnings that increase conceptual understanding." Reason indicates that the same paper "is written in a very unspecific and generalized manner . . .", and "scarcely once do we learn about what anybody actually does, and why we should see this as an action-research project rather than a good piece of business." In the same vein, Mangham reports that "I learn little about the nature of dialogue in the paper by Ledford and Mohrman . . ." Only a stranger to academic life could be surprised by such disparate assessments of the same work. A closer look, however, explains the divergent conclusions.

Bartunek asks whether the topics investigated by the authors receive adequate theoretical framing, whether the cases show evidence of the types of changes that are relevant to the conceptual framework, and whether the cases link any changes to the processes depicted in the framework. This implicitly accepts that the topics identified by the authors are worthy of study.

It appears to us that Reason and Mangham are critical of the research agendas of the authors. Reason wanted to hear more about epistemology and communities of inquiry, and was disturbed that he did not. He wants the details of interactions in order to find evidence of his type of "new paradigm" resulting from the project. He appears to find a shortcoming in our finding of change in a large corporation that has stopped short of rejecting "business as usual within the Western corporate paradigm." We see no need for action researchers to embrace any particular ideology. We also see no way for any action research case to cover every issue of interest to all readers, and believe that the special issue makes a contribution whether or not it covers all the epistemological issues and other action agendas that interest some readers.

Mangham invites special attention because he goes to considerable lengths to inform us about what is missing in the reports. He structures his commentary around Sederberg's distinction between procedural ideology (how research is done) and substantive ideology (what is studied). This distinction is straightforward and is not novel. He goes on to argue in great detail that the cases focus too heavily on procedural, "how" matters and essentially ignore substantive, "what" issues. For example, he says that "It is this procedural ideology that some of the action research included in this issue seems keen to emulate. Phrases such as 'rigorous social science' and 'hard data' are scattered throughout the papers." Further, "The richness of the *what* all too often is lost as it is strained through the sieve of scientific rigour."

However one may characterize the papers in the special issue, we are astounded by the claim that they show an obsession with methodological rigor. We think it is likely that the overwhelming majority of social scientists would come to the opposite conclusion. Mangham cites our paper as an example of what he means because we reported survey data that bore on the issues we were investigating, rather than "on how ideas are negotiated or how discussion breaks down." In fact, the key issue is one of focus and balance. The term action research calls attention to both substance and method, and the particular balance represented in a write-up should reflect the purposes of the paper. In our case, we dealt not only with survey results but also with their role in the development of the organizational learning system, and the conduct and negotiation of learning and discourse between actors in a multilevel system.

How can Mangham conclude that the papers are concerned with method to the exclusion of substance? The answer is provided by his statement that the papers neglect "the central issue of Action Research--the defining and negotiating of reality that occurs between participants." He represents all else either as "procedural" or a mere smoke screen. This assertion aside, few action researchers believe that the social construction of reality between researchers and practitioners is the only issue of substantive import. Quite to the contrary, the substance of action research is *action* -- in particular, planned change in social organizations. Social construction is an important process that occurs before, during, and after change, and it is worthy of investigation. It is

important to most action researchers, however, only insofar as it helps us understand the overall process of change and the effects of the change. Researcher-participant social constructions are no more important than many other issues that arise in every action research project, such as the appropriateness and efficacy of organizational interventions, political dynamics within the organization undergoing change, and organization design issues.

Social constructionism offers one lens with which to examine organizational change, but at this point is not well linked to the true substance of action research. For social constructionism to achieve primacy over all other issues of interest to action researchers, it would be necessary to show that a superior intervention approach can be based on it. Mangham offers no evidence of this.

RESEARCH IN ACTION RESEARCH

Here we first consider the status of action research as social science. Next, we will review a set of related issues that are concerned with epistemology and methodology.

Three of the reviewers commented on what they saw as excessive concerns of the action researchers with their scientific standing. As we have indicated, Mangham makes this issue a major critique. He concludes his commentary with this following statement: "As someone interested in the negotiation of reality I wish the writers had shared more of this activity with me and had taken less time aspiring to be natural scientists." Heller also states that the authors seemed excessively preoccupied with their scientific status. Reason would prefer that the authors abandon their allegiance to conventional science.

We admit to being puzzled about why this issue received so much play in some of the commentaries. We do not see why it is noteworthy that a group of authors who identify themselves professionally as social scientists, and who are publishing papers in a prominent scientific journal, make references to science, scientific knowledge, and scientific methods in their papers.

A more important issue concerns the epistemology underlying social science. How we know, and how we know that we know, will ever be central concerns of researchers. These are

of particular importance to researchers whose assumptions vary from the mainstream. Both Reason and Heller note that the standards for evaluation of action research papers are ambiguous partly because these issues are not resolved. Reason offers a useful synthesis of themes in the papers that contradict the assumptions of normal science, and points out that the challenge to normal science is implicit rather than explicit. We agree with most of this commentary. We cannot be certain of the extent to which we agree with Reason's alternative philosophy of science, since its details are not spelled out in the commentary. Nevertheless, he offers a helpful service in identifying the issue. We endorse his call for a special issue of Human Relations devoted strictly to this issue.

Reason points out that action research abolishes the separation of the knower and the known that is central to the practice of conventional science. Subjects become co-researchers who not only know the purpose of the research, but help shape it. Reason argues that since conventional objectivity is lost, the old quality standards are lost as well. He suggests abandoning conformance to method in favor of relying on "an epistemology based on personal and communal self-reflection, and in the end on a more integrated form of consciousness." This series of statements does not follow, in our view. Because the methods of social science are not truly objective does not mean that method is now a meaningless standard. By analogy we point to the physics of subatomic particles, where the "subjects" (particles) react to attempts to measure them. This is the source of the famous Heisenberg uncertainty principle. Yet, physicists continue to make progress without abandoning methodological standards and without adopting a new epistemology. The "more integrated form of consciousness" that interests Reason may be an outcome of some action research projects, but one need not pursue this end to embrace action research. Moreover, one is still left with the problem of knowing how we know that a higher form of consciousness has been attained, which returns us to the problem of method.

Reason further argues that since action research is an inevitably political process, researchers need to "explicitly explore their own political allegiances, their perspectives based on class, gender, race, country of origin, and so on." He specifically criticizes our paper for failing to

do this in our paper. The implication is that the values that will prevail in the organizational change are at stake, and that these values are in some way aligned with demographic variables. While we may agree with this proposition, it is not necessarily true that an action research account is incomplete unless researcher demographics and personal values are explicit. The four commentaries on the special issue of Human Relations are likewise value-laden, and yet the commentators, Reason included, did not feel the need to self-disclose along these lines. The political nature of action research means that personal sharing of the type Reason calls for should be done with the client, and indeed the action researcher's appearance, dress, and manner of speech immediately convey much of this information. This does not necessarily mean that personal sharing is necessary in research reports. At some point, the ideas expressed in a research paper need to stand on their own.

An epistemological issue that lies just below the surface in three of the commentaries is the debate over quantitative versus qualitative methods. Action research inevitably uses qualitative methods to some degree. It is a literature of cases and stories by its nature. A difficult problem is compressing the account enough to generate a publishable paper, while preserving enough of detail to tell a meaningful story. Several reviewers complain that they were dissatisfied by the lack of qualitative detail, by issues not addressed, and by the difficulty of knowing what standards to use in evaluating the qualitative evidence that is presented in the cases. These are difficult problems with using qualitative methods in the social sciences generally. Action researchers face a greater problem than many other social scientists, however, in the use of qualitative methods. Consider the difficulty of telling a 25-page story about a five-year change effort in a large, multi-site system or set of systems with thousands of employees. That is representative of the cases presented here. This is more difficult than organizing targeted qualitative data about a narrowly defined research issue. We believe there is considerable work to be done in defining standards for the reporting of action research cases.

Finally, we wish to recall an issue that we raised in our original paper, but none of the reviewers addressed directly. That is the need to rely on quantitative methods in studying change

in large systems. No individual researcher or research team can experience more than a fraction of the telling interactions and events in the large-scale change efforts reported in the special issue. The researchers cannot be everywhere, know everyone, and observe everything. How will they know whether change has occurred, and what has caused the change, in a large system? We do not doubt that they will be able to reach conclusions in the absence of adequate valid data; managers and other organizational members do so continually. The issue is whether their conclusions are to be trusted without quantitative as well as qualitative data.

This is a controversial issue among action researchers. Some, including some authors of papers in the special issue, view quantitative methods as invalid and even morally unacceptable. They view quantitative methods as the handmaiden of orthodox social science, and therefore unacceptable. Our position is that the use of quantitative methods can be an integral part of the action research process. For example, the design of measures and the use of the data can be collaborative. An internal learning community can employ measures as part of their learning process. Ultimately, we find no incompatibility of action research and quantitative methods, and indeed believe they are essential to assessing the phenomena that interest us.

CONCLUSION

We wish to thank the commentators for shedding light on some important topics raised by the special issue, and for continuing a scholarly dialogue about action research. We would like to close by returning to an earlier theme. The special issue demonstrates that the action research approach can advance theory as well as practice, and that the fruits of this labor are publishable in the key journals. We encourage all those who are considering learning this approach; there is plenty of room for additional contributions to the action research tradition. We would like in particular to invite the special issue commentators to add to the richness of the action research literature by contributing new exemplars of their own.

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