Action Research and Scientific Method: Presumed Discrepancies and Actual Similarities

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Recent comparisons between action research (AR) and scientific method (SM) have typically focused on their differences and generally concluded that they are two distinct and incompatible methods for cumulating knowledge. The present article attempts to bridge the two approaches by reviewing their common roots and by analyzing the assumptions underlying three frequently noted discrepancies between these two forms of inquiry: (a) the treatment of (multivariate) causality, (b) the setting of the experiment or intervention—field versus laboratory—and the use of a control group, and (c) the use of qualitative versus quantitative data. The evidence presented suggests that despite the differences between AR and SM, these three presumed discrepancies are, in actuality, similarities. In addition, AR and SM seem to be less distant philosophically than some recent comparisons have recognized.

The scientific method (SM) is a form of inquiry used to cumulate knowledge within the scientific community. SM is not the only method to legitimize knowledge, and in other contexts, alternative approaches, such as authority (e.g., "If it is in the Bible, it is true"), or common sense (e.g., "this is true because it is self-evident") are used (Cohen & Nagel, 1934). Epistemology is the discipline that studies *scientific* knowledge and how it is generated, and it is distinguished from gnoseology, which is concerned with knowledge in a more general sense. The dominant paradigm (Kuhn,

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1962) in the field of epistemology is the so-called hypothethico-deductive method, which is the core of the scientific method. Some disciplines do not use the hypothethico-deductive method to cumulate knowledge because some of its assumptions may not be tenable in all circumstances. Two examples of such disciplines are philosophy and art. SM does not include aesthetic features or moral values, and this characteristic impedes its use with any discipline that involves moral or aesthetic judgments. SM is the result of almost 400 years of debate regarding different ways of describing, understanding, explaining, and predicting reality.

Organization development (OD) is a long-term, planned effort (French, Bell, & Zawacki, 1989) that uses the theories and knowledge of the behavioral sciences (Miles & Schmuck, 1989) to produce changes in organizations (French & Bell, 1990). OD brings a unique perspective to the analysis of organizations. It adopts an open systems (Von Bertalanffy, 1950, 1968) approach. Some authors (e.g., Argyris, 1980, 1983; Argyris, Putnam, & Smith, 1985; Argyris & Schön, 1989; Peters & Robinson, 1984; Schön, 1983; Shepard, 1960; Susman & Evered, 1978) have argued that the assumptions underlying the systems approach to organizational change are not entirely compatible with the scientific method, and therefore OD and the study of organizational change in general require a new method for cumulating knowledge, a new form of inquiry. Thus a variety of methods based not only on understanding and learning about organizations but also on changing them have been proposed (action research, e.g., Brown, 1972; participatory action research, e.g., Whyte, Greenwood, & Lazes, 1991; action science, e.g., Argyris, 1983; experimental action research, Chein, Cook, & Harding, 1948). These action-oriented methods are the product of methodologies and assumptions existing in a number of social sciences, such as anthropology, economics, ethnography, organizational behavior, psychology, and sociology.

Recent comparisons between the varieties of action-oriented methods and the scientific method have concluded that these two forms of inquiry are dissimilar and different in nature (e.g., Argyris & Schön, 1989; Brief & Dukerich, 1991; Stone, 1981). More specifically, the discrepancies between AR and SM are frequently highlighted (e.g., Susman & Evered, 1978), whereas the parallels between the two are not typically emphasized. The present article *does not attempt* to evaluate the relative worth and usefulness of SM and AR for organizational research. Moreover, it does not attempt to describe the situations under which action-oriented methods or SM could be used. Both of these topics have generated much controversy in recent years (Argyris, 1980, 1982; Argyris & Schön, 1991; Brief & Dukerich, 1991; Lawler, 1985; Stone, 1980, 1981, 1982, 1987; Susman & Evered, 1978). The goals of the present article are to show (a) that despite the differences between AR and SM, there are definite similarities that are being overlooked; (b) that frequently outlined differences between them are not such in some cases, and in other instances they can be easily bridged; and (c) that AR and SM are two forms of inquiry that may be less distant philosophically than some recent comparisons have recognized.

First, the distinguishing characteristics of the two methods are briefly outlined. Second, some previously highlighted differences between the two methods are reviewed. Third, evidence is provided indicating that three presumed incompatibilities between AR and SM are, in actuality, continuities. Finally, reasons are provided for

why AR and SM may be seen as more distant than they actually are. Because of the extensive literature pertaining to each of the two methods, the discussion is limited to representative work. The assumptions, procedures, and data-gathering and analytic techniques of SM are reviewed following the mainstream work by Bachelard (1983), Bunge (1985), Campbell and Stanley (1963), Cohen and Nagel (1934), Cook and Campbell (1979), Kerlinger (1986), Kuhn (1962), and Popper (1959). AR is conceptualized following Alderfer (1977), Alderfer and Brown (1975), Argyris and Schön (1989), Brown (1972), Chisholm and Elden (1993), Coch and French (1948), Elden and Chisholm (1993), Lewin (1946), McGill and Horton (1973), Peters and Robinson (1984), Rapoport (1970), Shepard (1960), and Susman and Evered (1978).

THE SCIENTIFIC METHOD: BRIEF OUTLINE

In the scientific approach, the process of cumulating (or generating) knowledge involves a sequence of stages. First, there is a problem, or a difficulty, with which the researcher is concerned. Second, there is a clear statement of the problem. Without an explicit recognition and definition of the problem the scientist cannot advance to the next step. The researcher observes some phenomenon repeatedly and inductively concludes that there is a pattern or repetition of some relation and then generates a hypothesis (Stone, 1978). A hypothesis is a conjectural statement about relations between/among variables (Kerlinger, 1986). Examples of hypotheses are "Frustration leads to aggression," "Attitude similarity leads to attraction," and "Team-building interventions increase the productivity and the satisfaction of the members of the organization." A good hypothesis needs to be stated in declarative form and should not be too specific or too general (Kerlinger, 1986). Also, a good hypothesis should allow for its empirical falsification. The next step is a deductive reasoning process, through which the hypothesis leads to a prediction about the "real world." A hypothesis connects abstract constructs (frustration, aggression, satisfaction), and a deductive process allows for the empirical realization (Carlsmith, Ellsworth, & Aronson, 1976) of the conceptual proposition. Finally, the process of falsification matches the hypothesis with the data empirically collected, and the degree of correspondence will indicate the appropriateness of the hypothesis. If there is limited or no correspondence, the hypothesis will have to be appropriately modified and the process be repeated in a cyclic fashion.

ACTION RESEARCH: BRIEF OUTLINE[1]

Kurt Lewin is often considered one of the principal founders of AR (Marrow, 1969). In his seminal 1946 article, he proposed the action research model. Lewin was concerned with intergroup and majority-minority relations, and societal change and improvement. Because of these social concerns, he suggested that social research should lead to social action, and proposed a model that includes both science *and*

action, because "research that produces nothing but books will not suffice" (p. 35). Lewin proposed to use the principles of normal science to attempt to solve specific social problems, such as anti-Semitism. Almost concurrently, Collier (1945) adopted a similar action-oriented approach to understand and improve Native American affairs. In addition, on the other side of the Atlantic, researchers at the Tavistock Institute of Human Relations in London were blending science and action in attempting to solve social problems, such as the repatriation of British soldiers after the war (e.g., Bion, 1948). The AR model was then quickly extended to industrial and organizational settings (e.g., Coch & French, 1948; Trist & Bamforth, 1951; see Elden & Chisholm, 1993, and Rapoport, 1970, for historical accounts).

Thus Lewin's (1946) model combines research and action to increase understanding and generate change. Lewin emphasized that the introduction of action into the scientific model "by no means implies that the research needed is in any respect less scientific or 'lower' than would be required for pure science" (p. 35). Moreover, attempting to change a system may lead to a better understanding of it (Brown, 1972).

AR is the process of systematically collecting data about a system (e.g., organization) relative to some goal or need of the system. The data collection is not arbitrary; on the contrary, the action researcher is guided by hypotheses and assumptions about the nature of the organization and its subsystems. The data are fed back into the system, and action is taken as a consequence. Part of this action involves modifying the hypothesis if the empirical data so indicate. AR is a systematic and ongoing process of stating hypotheses, collecting data, and feeding the data back into the system. AR is a process of hypothesis testing and problem solving. In fact, AR can be seen as the application of the scientific method to problem solving and fact finding in organizations, with the difference that there is an active participation by not only the researcher but also the members of the organization. This model can be seen as a cyclical process involving a number of phases: analysis, fact finding, conceptualization, planning, execution, evaluation, and specifying learning.

ACTION RESEARCH AND SCIENTIFIC METHOD COMPARED: HIGHLIGHTED DIFFERENCES

Recent work comparing AR and other action-oriented methods with the scientific method have highlighted the differences and incompatibilities between the two approaches rather than their similarities. Gergen (1982), for example, as he explored alternative orientations toward science, identified environmental-centered and person-centered theories of knowledge. Gergen described these two epistemologies as holding distinct assumptions. Environmental-centered, or *exogenic*, theories assume that knowledge is a reflection of the real world or "a map of nature's contour" (p. 175). Alternatively, person-centered, or *endogenic* theories assume that the human mind is the originator of knowledge, a source of construction of reality. It can be argued that normal science shares some of the assumptions of the exogenic worldview because it assumes that the environment drives the senses in predictable ways (e.g., Stone, 1992).

Conversely, the action-oriented methods (e.g., action science) share the endogenic view because knowledge is primarily a product of the processing agent and understanding meanings is more important than describing "objective" reality (Argyris et al., 1985).

Susman and Evered (1978) also focused on the differences between AR and SM and evaluated the scientific merits of AR. They concluded that because the two methods are intrinsically different, "action research is not compatible with the criteria for scientific explanation as established by positivist science" (p. 601). Moreover, they argued that action research "constitutes a kind of science with a different epistemology that produces a different kind of knowledge" (p. 601). AR and SM were described as being different on numerous dimensions. For example, positivist science is described as attempting to be value free, whereas action research is viewed as aimed at developing social systems and releasing human potential. Second, the positivist approach is described as being present oriented, whereas action research is more concerned with the study of the present only because its analysis will be instrumental in conceptualizing more desirable futures. Third, the positivist tradition uses samples only as representative of populations, whereas action research treats cases as sufficient sources of knowledge. Fourth, the units (e.g., organizational members) are objects to study from the positivist tradition, whereas action research treats the client system members as self-reflective subjects with whom to collaborate.[2]

Whyte et al. (1991) also pointed to some of the differences between normal science and participatory action research (PAR), a "form of action research" (Greenwood, Whyte, & Harkavy, 1993, p. 177). For example, they argued that in the standard model of science the researcher is active at the beginning (i.e., designing the experiment or intervention) and at the end (i.e., data analysis and writing) of the project. In contrast, in PAR the researcher is constantly challenged by ideas and arguments posed by the project participants (e.g., Aguinis, 1990).

Another highlighted difference between AR and SM relates to the interdisciplinary nature of the former and the high degree of specialization of the latter. Sanford (1970), for example, noted that action research (which he prefers to label "research-action," p. 11) is properly multidisciplinary because a change project requires the expertise and theoretical background of several academic disciplines.

Friedlander and Brown (1974) observed that action researchers suffer a tension between "action" (change implementation) and "science" (knowledge generation). Their comments suggest that ARers need to alternate the *change agent* and the *researcher* roles, whereas researchers adopting SM only do not feel this pressure.

Differences in procedural steps involved in knowledge generation were noted by Alderfer (1977), who indicated that in OD interventions the hypotheses are originated inductively[3] and that interventions are not typically deductively derived from theoretical propositions, as would occur with an experiment within the SM framework. Also, Alderfer pointed out that the goal of most OD interventions seems to be to change the system rather than to evaluate the intervention or to generate or test a general theory, and Alderfer and Brown (1975) wrote that "the practice of organizational development has outstripped research and theory about planned change in organizations" (p. 11).

However, despite the aforementioned differences, and in agreement with the arguments of the present article, William F. Whyte (one of the intellectual founders of AR) et al. (1991) concluded that AR and SM are *not* in conflict. In their own words, "PAR is not an alternative to existing [traditional] social science but a way of dramatically enhancing our achievement of the goals of theoretical understanding and social betterment by widening the range of strategies at our disposal" (p. 54). Friedlander and Brown (1974) adopted a similar position and suggested that the separation of the roles of consultant and researcher is not only avoidable but also undesirable. Moreover, they stated that "the integration of the two perspectives within a single theorist-researcher-practitioner person is necessary" (p. 319). Rapoport (1970) noted that the integration of these roles is difficult but not impossible and provided specific suggestions so that a scientific pursuit can be accomplished even in situations ordinarily funded by clients with practical goals only. The research by Alderfer and Brown (1975) serves as a good example for Rapoport's predicament because of the successful integration of research, practice, and theory. In addition, Sanford (1970) also commented on the "rigid separation of research and action" (p. 3) and noted that this separation has had "some serious negative consequences for social science" (p. 8). The reason is that this split was the prelude to further splits and detrimental fragmentation signaled by a general tendency toward specialization. Sanford (1970) argued that as a consequence of this fragmentation there is a "fantastic proliferation of bitsy and disconnected and essentially unusable researches" (p. 10). Alderfer (1977), also commenting on the relationship between action and science, stated that even though only a small proportion of OD interventions are evaluated systematically, the scientific values are still present. Moreover, he described a number of successful organizational development interventions that improved research design, the quality of the measures used, and the theories generated.

The aforementioned arguments supporting a nonconflictive relationship between AR and SM follow the seminal article by Lewin (1946), who argued that since its conception the goal of AR has been not to confront normal science but to expand it. Consequently, there can be a concurrent investigation of "general laws of group life and the diagnosis of a specific situation" (p. 36).

If, indeed, and as many authors have suggested, AR may complement normal science, then the continuities as well as the differences between the two methods need to be uncovered. Three issues that have been presented as unbridgeable discrepancies between AR and SM are described. Next, their underlying assumptions are analyzed, and evidence is presented indicating that these discrepancies are not such. The three presumed dissimilarities between the two methods are described, as posited by Beer and Walton's (1987) article on OD in the *Annual Review of Psychology*. Their review posits that SM cannot be used to evaluate organizational change interventions. The presumed discrepancies posited by Beer and Walton also echo other proponents of an unbridgeable differentiation between action-oriented methods and normal science, some of whom favor the former (e.g., Argyris, 1980), whereas others adhere to the latter (e.g., Stone, 1982).

ASSUMED DISCREPANCY 1:
THE ISSUE OF (MULTIVARIATE) CAUSALITY

It has been argued that research following the guidelines of SM cannot be used to evaluate OD interventions. The reason provided for this argument is that SM aims at isolating causation and thus overlooks the systemic nature of organizations; that is, it "tries to identify the results of a single intervention" (Beer & Walton, 1987, p. 343).

The implicit underlying assumption is that the scientific method can solely operate on the basis of univariate isolation. It is implied that the scientific method is not concerned with multivariate causes and multivariate effects, and only a single intervention (i.e., single exogenous variable) is examined as it has an impact on a single effect (e.g., endogenous variable, such as organizational productivity or change in individual attitudes). These assumptions are not tenable if we examine the *essentialist epistemology* contribution to normal science.

The Essentialist Notion of Causality

The concept of causality plays a central role in the scientific method. Discussions about the philosophical roots of the term suggest that "causality" has different meanings for different epistemologic and philosophical schools. Brand (1976), Bunge (1959), and Wallace (1974) offer extensive reviews of the concept. The contribution of the essentialist school of epistemology to SM is referred to as being significant by all three reviews.

Essentialists use the term causality for a relation in which a set of variables is a necessary and sufficient condition for an effect to occur (Bhaskar, 1975; Feigl & Brodbeck, 1953). A set of variables necessarily, infallibly, and inevitably result in the predicted effect. This position is frequently associated with reductionism (a shift in the level at which causal constructs are postulated) because of its mechanistic tone (Suppes, 1970). Essentialists search for the ultimate causes, and they do not require that a presumed cause precedes a presumed effect. Essentialists do not contemplate contingent relations (A, B, C → Y / E; that is, A, B, and C *together* cause Y, given E) but only direct relations (A, B, C → Y). The essentialist theory of causation has been criticized because of its mechanistic tone. However, the main contribution of the essentialist school to SM is the proposition that there are multiple causal determinants for a particular effect.

A direct impact of this philosophical position on normal science is evident by examining the preponderance of multivariate data-analytic techniques in the most recent literature on organizational phenomena (e.g., canonical correlation, cluster analysis, discriminant analysis, factor analysis, and MANOVA). Furthermore, structural equation modeling (SEM; e.g., Anderson & Gerbing, 1988; Bentler, 1990; Goldberger & Duncan, 1973; Jöreskog & Sörbom, 1989) is becoming a frequent data-analytic tool in mainstream scientific journals in management, industrial and organizational psychology, and organizational research, such as the *Academy of Management Journal*, the *Journal of Applied Psychology*, and *Personnel Psychology*. This literature indicates that complex organizational phenomena do not preclude the

application of SM. A systems approach, for example, suggests decomposing complex systems into simpler subsystems. Writings on systems theory (e.g., Von Bertalanffy, 1968) indicate that there is no inherent incompatibility between SM and the multivariate complexity of the system under investigation. Also, the dynamism of organizational phenomena may be captured by longitudinal studies (Pettigrew, 1985), and SEM is a suitable technique to analyze longitudinal data. Researchers often choose to analyze data by examining correlation coefficients and means (e.g., Beer, Eisenstat, & Spector, 1990). These techniques may be criticized because they do not seem to capture the dynamism of cyclical multivariate/multilayered causal phenomena (Schein, 1990). Alternatively, SEM may be more apt to capture the dynamism of phenomena, such as corporate revitalization (Beer et al., 1990), and the implementation of information technology systems in organizations (Walton, 1989). Finally, a recent examination of issues associated with the use of difference scores encourages the use of multivariate linear regression models to measure change in organizations (Smith, Tisak, & Ryan, 1993).

In sum, the notion of multivariate causality is important for both AR and SM. It is easy to recognize that even though univariate relations are of interest, the ultimate goal of *both* AR and SM is to understand relations in which several variables are involved.

ASSUMED DISCREPANCY 2: FIELD VERSUS LABORATORY SETTING AND THE USE OF A CONTROL GROUP

A second assumed discrepancy between SM and AR is related to the use of a control group. It has been argued that OD interventions cannot be evaluated by relying on normal science because "normal science methodology can damage the experiment itself" (Beer & Walton, 1987, p. 343). For example, Blumberg and Pringle (1983) described how a group of unionized miners were so resented because they did not receive the "treatment" that they voted to prevent further changes in the workplace. The control group presumably learned about the treatment group because the intervention was taking place in a field setting. Thus Beer and Walton (1987) conclude that "the concept of control groups or uninformed subjects may diminish the value of the information or halt the experiment altogether (via control-group hostility)" (p. 343).

The assumption underlying this presumed difference is that it is not possible, or that it is at least very difficult, to have control groups in a field setting. Thus, presumably, there is an unbridgeable difference between SM and AR because scientific research usually allows for the use of a control group (because experiments are typically conducted in laboratory settings), and OD interventions cannot include control groups (because they are conducted in field settings).

A large body of literature and recommendations about the planning of *successful experimental designs in field settings* can be examined to bridge this assumed discrepancy between AR and SM. The inclusion of a control group (defined below) can

take place within either an AR or SM framework. AR can also operate including a control group in a field setting as part of the intervention.

The Use of a Control Group

Proponents of a seemingly unbridgeable differentiation between AR and SM assume that a comparison between groups can only be achieved through the administration of the treatment (i.e., intervention) to one group and not to a second comparison group (e.g., Beer & Walton, 1987). Widely recognized work on the use of experimental research in naturally occurring or field settings is that by Campbell and Stanley (1963), Cook and Campbell (1979), and Festinger and Katz (1953, chaps. 2-4). This body of literature indicates that to make inferences regarding causality, the physical setting of the experiment (e.g., laboratory, organization, computer simulations) is not as important as other factors, such as random assignment of individuals to groups or conditions, the existence of at least two groups who receive the treatment at different degrees of intensity, and the manipulation of the treatment. It is explicitly asserted that the so-called control group that *does not receive the treatment* is not a crucial part of the design. The relevant issue is that there should be at least two groups that can be compared to each other and not that there is one group that does not receive the treatment at all.

Cook and Campbell (1979) described a phenomenon they labeled "resentful demoralization of respondents receiving less desirable treatments" (p. 55). Resentful demoralization is a general problem that arises when valuable resources are distributed in a fashion that is perceived as unfair by the participants in the experiment. There are a number of solutions to prevent resentful demoralization. One of them, which has been shown to be effective in practice (e.g., Bishop & Hill, 1971), consists of giving the comparison group some other desirable resource that is not expected to influence the measured dependent variable(s). The control group would receive a treatment that is not conceptually related to the possible outcomes of the OD intervention in the other group. For example, if the OD intervention consists of a job enlargement, then the control group can have their jobs changed but not enlarged by the addition of new tasks, and this was precisely the strategy adopted by Bishop and Hill (1971). In this type of design, the control group is named "placebo control group."

In sum, the prescriptions of normal science about experimentation in settings other than the laboratory suggest that it is possible to plan interventions that include a comparison group in adopting either an AR or SM approach in field settings.

ASSUMED DISCREPANCY 3:
QUALITATIVE VERSUS QUANTITATIVE DATA

It is argued that a "quantitative description may not be the best method for understanding a multi-causal phenomenon" (Beer & Walton, 1987, p. 344). The underlying assumption is that the exclusive use of quantitative methods may not permit the accumulation of knowledge about complex, multicausal organizational phenomena.

Also, it has been stated that research following the guidelines of the scientific method "attempt[s] to find the perfect quantitative methodology and 'scientifically' prove its value . . . [but] . . . OD should attempt to build a different model of knowledge" (Beer & Walton, 1987, p. 344). Thus the assumption is that action-oriented research (not only quantitative but *also* qualitative; e.g., Argyris et al., 1985) is more suitable for organizational research than SM, which is supposedly *exclusively* quantitative.

The main purpose of the scientific method is to explain, understand, and predict phenomena (Kerlinger, 1986). It should be apparent that these goals may not be achieved by only examining univariate relations. There is also enough evidence in the literature suggesting that using only a single data collection and analytic technique (e.g., quantitative) may also lead to the failure of achieving these goals (e.g., Campion & McClelland, 1991; Edwards & Baglioni, 1991; Frone, Russell, & Cooper, 1992). The scientific method does not prescribe the use of a specific type of data (e.g., quantitative only, quantitative and collected on ratio-level scales, or quantitative and collected on interval-level scales). On the contrary, a variety of data types should be collected to capture the complexity of reality. Furthermore, the exclusive use of quantitative data may be signaled as a weakness and possible bias of a research design and thus something to avoid (Cook & Campbell, 1979). Monomethod bias refers to the manipulation or measurement of constructs in such a way that only a single method is used and to the inability to generalize the findings beyond the method used. For example, if "satisfaction with the job" is measured with a paper-and-pencil self-report questionnaire, the researcher does not know whether the responses will be consistent with the responses in a face-to-face interview in which the interviewee is asked "How satisfied are you with your job?" The rapid progress and complexity of the processes of collection and analyses of qualitative data within the scientific method approach are widely documented by Miles and Huberman (1991).

Similarly, action-oriented research is not *only* concerned with qualitative data but *also* quantitative data (e.g., Argyris et al., 1985). Indeed, research by Guzzo, Jette, and Katzell (1985), Neuman, Edwards, and Raju (1989), Nicholas (1982), Roberts and Porras (1982), and Vicars and Hartke (1984) investigated the effectiveness of typical OD interventions using hard-core quantitative data-analytic methods such as meta-analysis. Also, the proper utilization of data is an important factor in the success of any OD intervention. Data are a basic tool for organizational change (Nadler, 1977). The open systems approach assumes that the study of organizations is not a simple task and that there is a large number of interdependent-interacting variables that affect each other in a continuous, ongoing process. This complexity can be measured and captured only through a combination of qualitative and quantitative data collection and analysis methods. For example, Alderfer and Brown (1975) used a combination of unstructured observations, tape-recorded proceedings, semistructured interviews, a structural questionnaire, and a structured time series questionnaire. Additionally, the most well-known diagnosis approaches to organization development use a combination of quantitative and qualitative methods (e.g., Faerman, Quinn, Thompson, & McGrath, 1990; Harrison, 1989; Nadler, 1977; Weisbord, 1976). Obviously, the effectiveness of an OD intervention cannot be evaluated solely by analyzing data

collected with a paper-and-pencil self-report questionnaire, but the exclusive use of an individual interview may not yield all of the necessary information either.

CONCLUSIONS

Recent work comparing AR and SM have typically concluded that they are different and incompatible forms of inquiry (e.g., Brief & Dukerich, 1991; Susman & Evered, 1978). Some argue that SM is superior because it is more rigorous (e.g., Stone, 1982), whereas others argue that AR provides information that is more relevant and usable (e.g., Argyris & Schön, 1991). Previous reviews have examined the relative merits of AR and SM (e.g., Susman & Evered, 1978) and the differences between them (e.g., Argyris & Schön, 1989). The present article adopted a different approach and focused not only on the differences between AR and SM but also on their commonalities.

AR and SM seem to have been more closely aligned when AR was first proposed in the 1940s. However, the past three decades have shown an increasing separation rather than an integration of science and practice. Given the existence not only of differences between AR and SM (e.g., Susman & Evered, 1978) but also of similarities between the two (e.g., Lewin, 1946), why have their differences been more empha-sized than their continuities? In his closing remark, Alderfer (1977) noted that the tension between practice and science (an issue already raised by Sanford, 1970) was a debate that threatened to influence not only social action but also the social sciences in general. The realization of Alderfer's comment may have been one of the reasons for the apparent separation between AR and SM. It would seem that the dual agenda of AR (social action *and* social science) was particularly vulnerable to the more general debate in the social sciences about practice versus research and the position of the "psychology establishment" (Sanford, 1970, p. 7) that these two could not be inte-grated (e.g., Miller, 1969). Thus, apparently, action-oriented research may have been an involuntary battlefield for the more general debate of practice versus research (e.g., see the debate between Argyris, 1980, 1982, and Stone, 1981, 1982).

Some (e.g., Alderfer, 1977; Friedlander & Brown, 1974; Rapoport, 1970) argue that the apparent separation of AR and SM may also have been the consequence of an imbalance within AR that favored action goals over research goals (mainly because of the pressure to serve clients and sell specific intervention techniques) and the large proliferation of unevaluated varieties of interventions. This change from AR's original formulation to its implementation in the 1960s and 1970s may have also been a reason why AR and SM seem to be more distant than they originally were. In addition, Sanford (1970) suggested that the precursors of AR have not reproduced themselves, and thus a critical problem in AR is the lack of researcher/practitioner role models, which would also explain, at least in part, the apparent separation between action and science.

However, given the arguments presented in this article, AR and SM seem to be less distant philosophically than some reviews recognize. Furthermore, AR may be conceptualized as an application of the scientific method to a specific setting— organizations—and to a specific purpose—organizational change. Two unique fea-tures of AR are that the hypotheses are not generated only by the researcher but also

by the organizational members, who take an active role in defining the problem and the goals of the intervention, and the purpose of AR is to generate not only general knowledge but also knowledge specific to the situation so that the conditions can be improved. Thus AR may be thought of as a complement and extension of SM (Brown, 1972; Whyte et al., 1991). AR is not necessarily an antagonist of the current general scientific paradigm of the so-called normal science. Interesting evidence supporting this conclusion is that both Kerlinger (1986) and French and Bell (1990) cite the *same* source—John Dewey's (1910) *How We Think*, to describe the procedural stages of SM and AR, respectively.

Additionally, OD may benefit from considering AR as part of the scientific paradigm. Kuhn (1962) analyzed the shift in paradigms and concluded that old paradigms are not easily replaced by new ones. From Kuhn's analysis, it can be concluded that there is a slow process that leads to a "revolutionary" stage. Thus it seems that it may be detrimental for the field of OD to be excluded from the scientific community as a nonscientific field because power struggles within the scientific community may lead to the stigmatization and consequent isolation and segregation of the discipline.

Finally, there are five types of evidence suggesting that AR is closer to SM than some previous comparisons recognized. First, OD researchers and practitioners adopting an action-oriented approach use both qualitative and quantitative methods of analysis (e.g., Argyris et al., 1985; Guzzo et al., 1985; Nadler, 1977; Neuman et al., 1989; Nicholas, 1982; Roberts & Porras, 1982; Vicars & Hartke, 1984). Second, there is a periodic presence of OD in mainstream scientific publications, such as the *Annual Review of Psychology* (see Alderfer, 1977; Beer & Walton, 1987; Faucheaux, Amado, & Laurent, 1982; Friedlander & Brown, 1974). Third, novel data-analytic techniques, such as SEM (e.g., LISREL, EQS), are powerful and increasingly pervasive tools that permit the analysis of longitudinal designs, which seems to be an appropriate method for capturing the complexity of reality in general and organizational phenomena in particular. Fourth, newly proposed scientific theoretical frameworks adopting an open systems approach to organizations posit that only a multidisciplinary approach will lead to a real understanding of organizational phenomena (Polley, 1993). Last, there seem to be specific efforts to weaken the apparent separation between practice and science. For example, *Personnel Psychology*, which is considered a hard-core scientific publication, has recently announced the inauguration of a new section especially devoted to practice/research issues, entitled *Innovations in Research-Based Practice* (Campbell, 1993; Hakel, 1993).

NOTES

1. The description of AR corresponds to what Elden and Chisholm (1993) recently labeled the "classical model of action research." They used this label to distinguish the model from the "emergent varieties of action research," which Elden and Chisholm (1993) and Chisholm and Elden (1993) describe as presenting "clear and substantial contrasts to the classical model of action research" (Elden & Chisholm, 1993, p. 136) and to SM. These emerging varieties (Brown, 1993; Engelstad & Gustavsen, 1993; Greenwood, Whyte, & Harkavy, 1993; Ledford & Mohrman, 1993; Levin, 1993) seem to be epistemologically separating from the so-called classical approach to AR and from SM. The emerging varieties of action research—which should

not be considered to fit in one single model—vary along five dimensions: system level of the change target, degree of organization of the research setting, openness of the AR process, intended outcomes of AR, and the role of the researcher. The newly proposed varieties of AR emphasize such features as (a) full partnership with the organizational members, who now become co-researchers (Greenwood et al., 1993), (b) the study of cases beyond the level of a single production unit or plant (Engelstad & Gustavsen, 1993; Ledford & Mohrman, 1993), (c) a constructivist approach to reality where participants envisage a future and attempt to achieve it (Levin, 1993), (d) the emergent character of the action research project because the research design evolves as the project progresses (Levin, 1993), and (e) the goal of politically empowering and liberating relatively powerless and oppressed groups (Brown, 1993). Thus, because of these important differences with "classical AR" and SM, Elden and Chisholm (1993) suggest that future work in the area of AR should attempt to answer the question of whether the goals and characteristics of the emerging varieties of AR can be realized within the normal science paradigm.

2. Susman and Evered (1978) also viewed AR and SM as differing on other dimensions, such as (a) the language used for describing units (connotative and metaphorical versus denotative and observational), (b) basis for assuming existence of units (human artifacts for human purposes versus independent existence of human beings), (c) epistemological aims (development of guides for taking action that produce desired outcomes versus prediction of events from propositions arranged hierarchically), (d) strategy for growth and knowledge (conjecturing and creating settings for learning and modeling of behavior versus induction and deduction), (e) criteria for confirmation (evaluating whether actions produce intended consequences vs. logical consistency, prediction, and control), and (f) basis for generalization (narrow, situational, and bound by context vs. broad, universal, and free of context).

3. It should be noted that SM also includes an inductive stage (e.g., Glaser & Strauss, 1968; Stone, 1978) that may be useful for initial hypotheses generation (e.g., Alderfer & Brown, 1975). However, Alderfer's (1977) description referred to the preponderance of inductive over deductive processes in OD.

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