

## **Kurt Lewin Award Lecture, 1997**

### **Field Theory as a Basis for Scholarly Consulting**

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About 45 scholars throughout the world met recently to celebrate the 50th anniversary of the Tavistock Institute. The Tavistock Institute was the organization that founded action research in Europe as well as the journal *Human Relations*. The purpose of the conference was to examine the present status of action research throughout the world in order to illuminate its future.

Many, including myself, left the conference with two impressions. There exists a wide variety of views on what is action research. However, when the participants attempted to develop patterns of possible agreement, hopefully to explore next steps, the result was disagreement, conflict, and self-reinforcing defensive arguments. These consequences seem a bit ironic for scholars dedicated to produce the opposite effects in the world that they study.

As I reflected on the conference, I realized that the proponents were explicit about their respective theories; however, each showed little focus on two major features of any theory—causality and the testing of causal claims (Argyris & Schön, 1974; Kaplan, 1964; Pfeffer, 1997).

The participants, for the most part, did not have a metatheory—that is, a theory of how to build a theory. Lewin saw this as an important issue early on in his work. Indeed, he described Field Theory as a metatheory (Lewin, 1935, 1936; Cartwright, 1951, 1959; Gold, 1992; Metraux, 1992).

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I believe that scholars are free to generate any theory about action research that they choose to develop. I also believe that they are not free not to make explicit what they believe are the features of sound theory.

Lewin did say, in effect, that there was nothing as practical as sound theory. He defined the properties of any sound theory. I will make some of these properties explicit and illustrate their implications for scholarly consulting.

### **Scholarly Consulting: Conditions and Consequences**

Scholarly consulting contains the following features:

1. Propositions that are valid *and* actionable, that are generalizable *and* applicable in the individual case.
2. Propositions that can be produced by scholars or practitioners under real time, everyday life conditions.
3. The effective implementation of these propositions leads to results that are consistent with what is intended. There are no inner contradictions. That is, the effective implementation does not necessarily create effective *and* ineffective consequences.
4. The claim of effectiveness is testable by methods that meet the most robust standards of disconfirmation.

### **Scholarly Consulting: Premises**

1. All understanding and explanation are in the service of action. The validity of the understanding or the explanation is ultimately established by the effectiveness of the action.

*Implication:* All propositions about understanding and explanation are normative, because the basis for effectiveness is chosen by those producing the propositions and those implementing them. These choices are not based on some objective truth. The method for testing effectiveness is through the implementation of prescriptions that are derived from the normative descriptions. Action research, at its core, is normative and prescriptive.

2. All scholarly consulting activities are implemented by human beings in a context or situation.

*Implication:* Scholarly propositions are crafted in ways that respect any systematic constraints imposed by the human mind/brain and by systematic features of the context in which the actions are being taken.

I will expand on the definition and the premises as I describe some key features of sound theory.

### Propositions That Are Relevant to the Many and to the One

Sound theory produces propositions that are generalizable *and*, at the same time, applicable to the individual case. There is not one theory for the many and another for the one.

Lewin presented this feature in his description of the Galilean approach to explanation, as compared with the Aristotelian approach (Lewin, 1935, 1936). The premise of the Galilean approach is that a complete description should lead to predictions that do not admit exceptions. If one exception is found, it can serve as the basis for disconfirmation *and* as a basis for further research. One cannot ignore or delay dealing with exceptions by labeling them as residual categories.

The criterion of what is complete is defined by the theory. This makes it especially important that the theory be confronted with robust tests such as those proposed by the Galilean approach.

I should like to illustrate by the gatekeeper concept developed by Lewin. For example, in the group climate and leadership research, Lewin proposed that an autocratic leader created the following conditions. There are four subordinates who report to the leader. The key feature of autocratic leadership is that the leader is in control of the gate through which any subordinate must go if he is to achieve his goals, whatever those goals may be. This places the subordinates in a situation where they will be dependent upon the leader. They have to learn how to deal with her if she is to let them through the gate to enter the region in which their goal exists. The subordinates will also be placed in a situation where they compete with each other in order to obtain the leader's permission to go through the gate.

The claim by Lewin and his colleagues is that these conditions and their consequences are valid for many contexts—be it a business organization, a government bureau, a union organization, a church setting, a school setting, or a setting where parents are trying to get their children to eat healthy food. Moreover, the claim is that these predictions will hold if the subordinates are female or male, young or old, better or less well educated, poor or wealthy, and regardless of race or culture. Finally, the claim is that this will be true from the day it was constructed until forever, as long as the conditions depicted in the diagram are not altered.

Exactly what words the actor uses to execute these actions may vary widely. But, whatever the variance, it is a difference that makes no difference in understanding and explaining. The words, in Lewin's terms, are phenotypes. Meaning and the concepts inferred from knowing it—such as gatekeeper, goals, and needs of the subordinates—are genotypes.

### **Propositions That Can Be Produced by Anyone Under Real Time, Everyday Life Conditions**

Propositions that are actionable must be producible in the universe being studied by the scholars and the practitioners. In order to be producible, the propositions must contain causal claims crafted as statements—"If A, then B." Causality is the basis of effective action in everyday life (Shoham, 1990).

The ideas in good currency about how to produce valid descriptions of causality that should lead to effective action are largely based on the concept of variance. Many social scientists strive to produce causal explanations by using such perspectives as Mill's Method of Difference. This leads to the following consequences:

1. The research is organized around activities for the purpose of understanding. Understanding, at the outset, is in the service of explanation. The ultimate product is a valid description of the universe.
2. The rules for carrying out this activity include:
  - Produce as accurate and as complete descriptions as possible.
  - Take as much time as is necessary to produce these valid descriptions. Research time is different from time in everyday action. It is legitimate to study a 1-hour group interaction for several months.
  - Develop causal analyses that describe the variables and their interrelationships as completely as possible.
  - Test the causal claims under conditions where unrecognized distortions that confound the claims are minimized.

There are several consequence of these rules, when they are implemented *correctly*, that are counterproductive to producing valid knowledge in the service of effective action.

The causal models developed with an eye on completeness may not be actionable because they are not storable in and retrievable by the human mind under everyday life conditions. The human mind is a finite information system (Miller, 1956; Simon, 1969).

For example, Naylor, Pritchard, and Ilgen (1980) developed a model to understand behavior in organizations. The model was described as being as scientifically complete as they could make it. When the authors were finished, they recognized that human beings were unlikely to use the model in everyday life because it was too complex. They argued that human beings will have to develop a more simple method in order to act. They lamented what they called the degrading of the model in order to act, but agreed that it was necessary.

There is another interpretation that can be taken if the requirements for actionability are considered as equal to the requirements of validity. It could be that the more simple model is the correct *scientific* model because this is what the human

mind, being a finite information processor, can deal with. It is their model that degrades reality of science (Argyris, 1980).

A second counterproductive consequence that flows from propositions that are rigorously defined by the current ideas about rigor is that their “correct” implementation requires conditions which, if they exist, will *prima facie* lead to consequences that are counterproductive to the results.

The first example comes from the excellent research by Barker, Dembo, and Lewin (1943) on frustration and regression. Recall that they found that mild frustration led to creativity, but beyond a certain threshold it led to frustration. Frustration, in turn, led to dedifferentiation of the human personality which, in turn, resulted in regression.

Assume for a moment that a practitioner wishes to increase creativity by creating mild frustration. How would she do it? Would she tell her subordinates that she is about to generate mild frustration in them in order for them to act more creatively? Could doing so upset the subordinates and hence speed up the frustration?

How will she discover the threshold point? One answer is to use the instruments that the researchers developed to ascertain the threshold in their study. How could she use those instruments? Would she fill them out on-line? Would she have some observers fill them out? How could the results be analyzed in time to effect the on-line activity?

A second example comes from the research on communication (Aronson, 1972). The following advice flowed from the research findings. If the messenger and the message are to be trusted by an audience that is deemed to be bright, present several different messages. If the audience is composed of individuals who are not so bright, then only one message is effective. Nowhere in the publication is the practitioner told to make this reasoning explicit to the audience. That is not so surprising. Imagine telling the audience that they are getting one message because they have been judged as not being smart. Perhaps telling the bright audience that they are being given several messages could also activate mistrust—i.e., “What are they driving at?”

In both cases, the reasoning used to design the experiments followed the current rules for conducting empirical research. In the name of minimizing the confounding of results, the reasoning is covered up and the cover-up is covered up. If we are to be rigorous in the statements of the empirical results, these conditions should be included explicitly as part of the conditions under which the results hold.

If these “cover-up” conditions were included explicitly, at least two consequences would occur that are consistent with stating the propositions rigorously. The first is that scientists would be clear about the conditions under which their results hold. The second is that the practitioners would be alerted to the conditions that they must create if the propositions are to be actionable.

These conditions are not neutral. They are based on a theory-in-use of unilateral, top-down control.

The counterproductive consequence is that the findings are usually organized around the specification of empirical relationships among variables, such as a curvilinear relationship. The difficulty with such descriptions is that, to my knowledge, the human mind cannot produce them in the form of actual behavior in a specific situation or context. The results are not actionable.

How did the researchers produce this generalization? They followed the rules. They analyzed their data by using “research time.” They crafted the generalization, consistently with the rules, that focuses on variance. Finally, they organized their findings in the form of a proposition about the empirical relationship. This proposition is so information rich that it is difficult to implement.

These examples illustrate, I suggest, that knowledge produced rigorously following the empirical methods based on variance is not user-friendly. The examples also illustrate that embedded in the generalizations are the conditions under which they were generated. In practice, these conditions are kept tacit. The difficulty is that, in keeping them tacit, they can lead to advice that, when carried out effectively, leads to consequences that are counterproductive to the claims made in the propositions.

### **Predictions That Are Tested Under the Most Rigorous Conditions of Disconfirmation**

All tests should be crafted in ways such that they can be carried out effectively under everyday life conditions by scholars or practitioners. Tests that cannot be carried out by practitioners render helpless those practitioners who are concerned about the validity and the actionability of the propositions that they are using.

There are four types of tests that scholarly consultants can use. The first is the least robust. The last is the most robust.

- a. Predict what will and will not occur under conditions that are consistent with the universe as it is—that is, the status quo.
- b. Predict the conditions under which the above conditions will persevere.
- c. Predict that if the solutions are to be implemented, they will require a context that does not exist in the present universe.
- d. Predict the conditions under which (c) will persevere.

These modes of testing have important implications for establishing validity. For example, external validity is a necessary, but not a sufficient, condition for actionability. External validity makes the claim that the results obtained in one setting (e.g., an experiment in the laboratory or the field) are *relevant* to other settings consistent with the conditions under which the research was conducted (Campbell & Stanley, 1963).

High external validity tells us little about actionability. Trust, for example, is a variable that has high external validity. But most of the research that has established the external validity of trust specifies little about how to produce trust in the world of practice.

The most robust tests of validity are those that can be used to predict about universes that do not, as yet, exist. It is such tests that Lewin focused upon when he advised that, if social scientists truly wish to understand certain phenomena, they should try to change them.

Creating, not predicting, is the most robust test of validity-actionability. The criteria identified as (a) and (b) can be met by observing a universe that already exists. A more powerful test is if the scholarly consultants can show that they can recreate the universe that exists. This is an important distinction. Years ago, when the center research for the study innovations was created at Michigan, called CRUSK, its primary focus was to seek out and study innovative examples of organizing and managing human effort. CRUSK limited itself to studying what already existed. But how about innovations that did not exist, yet were derivable from social science research? For example, how does one create organizations that manage with the use of trust and learning and not with unilateral control? If the social scientists had focused on such questions, it is likely that they would not have found such examples in the empirical world. In order to answer that question, they would have had to create new forms of managerial procedures and organizational structure. If they had done so, they would have had to create rare events. Let us examine this claim in more detail.

Even more powerful is to fulfill conditions (c) and (d) when the conditions specified do not exist. To do so is to create rare events. Let us examine this claim in more detail.

### **Knowledge That Can Be Used to Create Rare Events Is What Drives Scholarly Consulting**

All universes that we study are created by our subjects or clients. We may describe a universe, but we are always describing normative processes. Scholarly consultants keep reminding themselves that, rigorously speaking, description cannot be separated from normative activities and get at the causality that creates the universe being described.

The research on group climates and leadership contains many predictions about the impact of autocratic, democratic, and laissez-faire leadership on such variables as group productivity and cohesiveness. Lewin and his colleagues went beyond prediction when they attempted to reeducate the autocratic leaders to act more democratically. This is an act of creation. Such acts require much richer explanatory sequences than are required by straightforward prediction.

For example, autocratic and democratic leaders act on the basis of their respective theories-in-use about effective action. Lewin and his colleagues defined these theories-in-use on the basis of their theoretical perspective. They taught these theories-in-use to the various leaders until they were skillful at producing the leadership actions that they were assigned. Based on their theories, the researchers could predict the impact on specified outcomes.

In order to retrain the leaders to be more democratic, Lewin and his colleagues needed more information. How do they unfreeze the autocratic style with its accompanying skillful defenses and introduce a more democratic style? How is this done so that it not only perseveres but so that it can be monitored effectively in order to detect and correct any error or mismatches?

Experiments to change autocratic styles to democratic styles represented rare events in the leadership domain and in the organizational domain. Rare events are not likely to occur nor to persevere if the organizational context in which they occur is not also changed. How does one produce changes simultaneously in the leadership styles, group and intergroup dynamics, as well as the organizational context?

The hesitation by social scientists to become concerned about producing change creates a consequence that limits the power of our causal explanations because we do not include in the descriptions the causal reasoning that produced the patterns in the first place. For example, social scientists may study organizations and develop patterns or generalizations that organize their observations in the form of patterns and generalizations. They may observe actions that they categorize as “low trust,” or “coalition group conflict,” or “quasi-resolution of conflict.” They may then claim, as do the behavioral theorists of the firm, that such phenomena are at the core of relationships in organizations (Cyert & March, 1963). A parallel program of inquiry called “action science” arrives at similar descriptions (Argyris & Schön, 1974, 1996).

So far, these descriptions describe what the scholars claim to have observed. They do not, however, provide causal explanations of how the actors created or produced the behavior that the social scientists organized around such concepts as quasi-resolution of conflict, etc. What is lacking are causal explanations of what was in the heads of the actors that caused them to create the actions that were observed by the researchers. It is these types of causal explanations that are required if social science is to be actionable—that is, if it is to be used to create the universe that social scientists describe. This cannot be accomplished without focusing on the question, “How do the subjects produce and create the behavior that we observe and that we use to define our universe?”

There is another counterproductive consequence of research that describes reality and ignores how the reality was created in the first place. The research activity becomes the servant of the status quo.

For example, scholars associated with the behavioral theory of the firm create concepts that describe what they observe, but there is little attention paid to



producing knowledge about how to change them. For example, several “relational concepts” are defined. These include limited learning, quasi-resolution of conflict, and intergroup rivalries (Cyert & March, 1963). To my knowledge, the founders of this theory pay little attention to conducting research on how to reduce the dysfunctional features of limited learning, quasi-resolution of conflict, and intergroup rivalries (Argyris, 1996). They appear uninterested in changing the status quo, especially in creating rare events.

### **A Theory of Action Perspective**

An example of an approach that is concerned about changing the status quo is the one developed by Donald Schön and myself. The approach has been described in several publications and, hence, I will not repeat the description at length (Argyris, 1982, 1985, 1990a, 1993; Argyris, Putnam, & Smith, 1985; Argyris & Schön, 1974, 1996).

The approach begins with the observation that individuals hold theories of action. The theories of action can be subdivided into the theories that they espouse and the theories that they use when they take action. Espoused theories are produced in the mind/brain in the activity described as “understanding in order to explain.” The theories-in-use are produced in the mind/brain in the activity described as “understanding to take action.”

So far, all the individuals that we have studied hold a theory-in-use that we call Model I. Gender, race, education, age, wealth, position in the organization, and country culture appear to make no difference. The espoused theories vary widely, but they do not cause the actions the mind/brain produces. Also, the actual words used may vary. Thus, a Briton may speak of being civilized, an American of easing-in, a Japanese of being respectful. The theory-in-use is the same. It is, in effect, “If you sense embarrassment or threat, in self or others, bypass it and cover up the bypass.” Indeed, primates may exhibit Model I (de Waal, 1982, 1989).

The causal reasoning of all Model I designs is primarily defensive. Defensive reasoning is to use premises that are subjective and tacit, and to infer from the premises in ways that are also subjective and tacit, and to test the conclusions reached by using logic that is the same as the logic used to reach the conclusion. The test therefore does not use independent logic. It is self-referential logic. “Trust me, I know what is really going on,” is a frequently used example.

Productive reasoning focuses on making the premises explicit, making the inferences from the premises explicit also, and having the conclusions tested by logic that is independent of the logic used to create the conclusions in the first place. This requirement has embedded in it a position about rigor. Rigorous, or what I call “productive” reasoning, is enhanced to the extent that causal relationships are specifiable at the conceptual level (e.g., the relationship between concepts of topology

and methodology). Rigor also includes specifying the operational definitions and showing how they are connected to the conceptual domain.

Lewin spent many hours striving to be as rigorous as possible in his thinking, including where he thought gaps existed that someday had to be closed (Lewin, 1938). It is such hard work at theory building and connecting it to empirical reality that led to the asking of clear questions, because “nature cannot give clear and definite answers to vague questions” (Lippitt, 1940, p. 9).

Lewin’s conception of theory building illustrates a theory that is based upon productive reasoning. For example, his theory is composed of concepts and their logical interrelationships defined so precisely that hypotheses can be formulated about what is excluded and included. Lewin provides many illustrations in his early work (Cartwright, 1951; Lewin, 1935, 1936, 1938; Marrow, 1969) of how topological and vector concepts can be integrated to provide a valid psychological science that permits the rigorous testing of hypotheses. Lewin’s use of the concept’s mathematical features is still open to question (Back, 1966), but this is not the point being made. The point is that his writings show that he was continually concerned with the use of rigorous productive reasoning.

For example, in his early writings (Lewin, 1935, 1936, 1938), Lewin published detailed discussions of what is required to translate topological vector concepts to psychological meaning. There are detailed discussions of the criteria used to coordinate a type of action to a place in the life space; the psychological meaning attached to the boundaries of regions and their degree of permeability; the psychological meaning connected to units of life space that are peripheral or central; and the path of locomotion through the life space.

I can recall providing, as concretely as possible, examples of what I had experienced in organizations to Roger Barker, Fritz Heider, and Herbert Wright. Their first step was to try to assess the richness of the examples. Consistent with Lewin’s admonitions, they pressed their inquiry in order to see if my examples described the total dynamic structure. Instead of trying to abstract averages of as many historically given cases as possible, they sought to develop the full comprehensives of the situation. “[Comprehending the] actual situation as fully and as concretely as possible, even in its individual peculiarities, make the most precise possible qualitative and quantitative determination necessary and profitable. But it must not be forgotten that only this task, and not numerical precision for its own sake, gives any point or meaning to exactness” (Lewin, 1935, p. 35).

Not surprisingly, Barker, Heider, and Wright would spend hours coordinating the features of the examples to various topological and vector concepts. In order to produce valid coordination, they had to be as explicit as they could be as to what was occurring in the situation. They strove to be as comprehensive as possible and to minimize unexplained meaning (residual categories), because one empirical exception could disconfirm our hypothesis and features of our theory. Recall Lewin’s

claim that “[e]very psychological law must hold without exception” (Lewin, 1935, p. 23). Any exception found is a trigger for research and inquiry to modify the theory.

I noticed that another consequence of these discussions was that it prepared me for my encounters with clients. The clients rarely raised questions that were beyond those that we had raised. Or, if they did, it was psychologically easier to admit that I had not thought of the question and to present my reasoning behind this unrecognized gap.

It was also easier to engage in a dialogue with the client on how to close the gap. Not infrequently, I found myself questioning the client’s reasoning. In most cases, they found the inquiry challenging and productive of important learning. Some realized early on that it would be useful for them to become skilled at productive reasoning.

As I have reflected on my experiences, I began to realize that productive reasoning was, at the heart of individuals and organizations, becoming more effective. I realized that one of the best ways to assure clients and myself that I was not unrealistically leading them toward error was the vigilant use of productive reasoning. I learned that my confidence in myself and the clients’ confidence in my competence was deepened and strengthened by becoming skillful at productive reasoning. This was not true in some cases. The client sought answers that did not require examining their reasoning process. I would do my best to show them why their quick fixes were not likely to work and, more certainly, not persevere. In those cases, I did not succeed and recommended that they look elsewhere.

I was bewildered to find that many organizational development professionals were not competent in productive reasoning. Some were not interested in it, and some thought it was counterproductive. Many described the emphasis as too rational, too logical, and too scientific. I recall that I would push back for them to define “too rational” or “too logical” and to illustrate through the use of their behavior with me and others. They often became defensive and expressed emotions that they had just claimed would not be generated by this “too rational” approach. When I would focus on their *and* my here-and-now actions, they became more upset and closed off learning (Argyris, 1987, 1990b).

There is another feature of theory-in-use that is important. All theories-in-use are implemented through skillful actions. Such actions are rigorously programmed in the mind/brain. The features of skillful action include

- It works.
- It appears effortless.
- It is automatic, spontaneous, and tacit.

Once actions become tacit they are no longer at the forefront of the mind. It is precisely because the actions are skillful that there is little need for paying conscious attention to producing them. The ability to act skillfully depends on

“designed ignorance.” This designed ignorance leads human beings to be unaware of gaps and inconsistencies between their espoused theories and their theories-in-use. This unawareness is skillful. It is not based on incompetence or a hole in one’s head.

The window into the mind/brain, therefore, is to begin with the actions human beings produce (e.g., conversation) and not espoused theories that are the basis of most paper-and-pencil instruments. Lewin and his colleagues were among the first to point out that observational data and transcripts of conversation were richer for producing and predicting than the many paper-and-pencil instruments that they used (Lippitt, 1940).

If we combine the existence of defensive reasoning and skilled unawareness, it is possible to explain why human beings often act in ways that can be described as skillfully incompetent. The actions are incompetent because they produce consequences that the actors do not intend. The actions are skillful because they are consistent with the actors’ theories-in-use. This has important implications for the kind of behavior that is created in organizations.

For example, in our theory of action perspective, we causally connect Model I theory-in-use to producing contexts (in groups, intergroups, and organizations) that inhibit productive reasoning and double-loop learning (Argyris & Schön, 1996). A key concept is defensive routines. Defensive routines are any actions that are produced to prevent embarrassment or threat to the participants *and* simultaneously prevent getting at the causes of the embarrassment or threat.

Defensive routines are designed and implemented through the use of defensive reasoning. An example is mixed messages: “Joe, you run the department, but check with Charley”; “Mary, be creative, but be careful.” The reasoning embedded in the theory-in-use of mixed messages is:

- State a message that is inconsistent.
- Act as if it is not inconsistent.
- Make the above undiscussable.
- Make the undiscussability undiscussable.

Under these conditions the effectiveness of learning is likely to be low. This feeds back to reinforce Model I theories-in-use, which, in turn, feeds forward to reinforce the organizational defensive routines. The result is an ultrastable state that minimizes effectiveness.

Organizational defenses and theories-in-use are intimately related. Interrupting and changing the ultrastable state just described should not be possible by making organizational changes that bypass the Model I theories-in-use and the defensive reasoning used by most human beings.

### **The Preciousness of Human Beings and Their Institutions**

Human beings are not likely to cooperate and seek help if they do not feel respected by those aspiring to help them. The first source of respect is for the scholarly consultants to realize that, whatever problems the clients produced, they did so because they believed it was the most effective way for them to act.

It follows from the above that whenever individuals act on the basis of their designs, the consequences cannot be errors if they produce what they intend. Such an outcome is a match, not a mismatch.

The second source of respect is the belief that individuals are capable of learning in order to detect error in their actions (single-loop learning) or in their underlying values (double-loop learning). I believe that this is the basis for the respect scholars such as Lewin and Barker exhibited toward human beings regardless of race, gender, wealth, education, and culture. Human beings were to be treasured, otherwise neither they nor the scholars would be able to learn, especially in the case of double-loop learning.

The scholarly consultant is genuinely committed to help the clients build a better life. "Better" means having more effectiveness, more competence, and greater confidence in themselves and their institutions to provide enhanced learning that perseveres. Lewin, according to Deutsch (1992), exhibited these features in dealing with clients and with his colleagues. Caring for human beings is not unconditional. Indeed, unconditional caring may result in human beings who are overprotected and brittle (Argyris, 1985). Perhaps this is one reason why Lewin was caring but tough (Ash, 1992; Deutsch, 1992). The scholarly consultant is caring to the extent that the clients are genuinely interested in learning, in producing valid information, in encouraging informed choice, and in accepting personal responsibility for these actions.

This sense of the preciousness of human beings inveighs against the notion that scholars, in the interest of being objective, must distance themselves from those they study. By distancing, I mean acting in ways that separate or make the researcher appear remote, cold, and reserved.

There is another consequence of distancing that concerns me. The distancing leads to separating basic from applied research.

For example, Cronbach and Suppes (1969) differentiate between "conclusion-oriented research" (basic) and "decision-oriented research" (applied). This distinction may make sense if the focus of research is upon diagnosis and invention (such as inventing policies). However, the distinction becomes problematic if one includes production and evaluation of human action as well as diagnosis and invention. Actors are forever reaching conclusions and making decisions about actions.

Moreover, Coleman's (1972) advice that self-correcting activities in present policy research are best done by independent studies, if carried out, undermines the personal responsibility of those who did the diagnosing and inventing of policies for

implementation and monitoring their actions. True, this integrative approach is not easy for individuals or for organizations to implement. But this does not seem to be a valid reason to bypass the problem. It is a reason for conducting that research that leads to individuals or organizations that are capable of integrating diagnosis, invention, production, and evaluation.

Years ago, when the Ford Foundation began, it was genuinely concerned with supporting social science. I was a member of a small committee of scholars invited to brainstorm about methodologies for the financial support of research. My model was one of a high degree of decentralization, with as much control of the funding at the local level (university, individual scholar) as possible. The idea was dismissed quickly by the senior officials of the foundation (most of whom were former academics) and the committee (all of whom were academics). The reason, I was told, was that academics could not be trusted to be concerned about anything except their personal agendas. The foundation officials expressed grave doubts that social scientists, left to their own devices, would care about producing basic and actionable knowledge that would help human beings and their institutions. When I pushed back, there was near unanimity that the reason that I thought academics might be trusted to change was that I was young and with little experience. As many of you know, the Ford Foundation executives eventually concluded that a radical change was necessary. They funded their own action projects.

This trend of distancing and mutual mistrust spread to the governmental agencies. They too began to define the research to be funded. Scholars who appeared before congressional committees and who promised that researchers were centrally interested in actionable knowledge began to lose their credibility. This distancing continues. Congressman George E. Brown, Jr., known for being a supporter of social science research, recently publicly raised questions about researchers' credibility (Johnson, 1993).

In summary, I am asking the audience to consider the following causal scenario. In the interest of being objective, social scientists often distance themselves from their subjects (be they individuals, groups, intergroups, or organizations). This distancing is often legitimized as necessary for basic research. This, in turn, leads social scientists to take for granted that in the name of getting funding, they can promise results that are actionable when they know ahead of time that this is not going to be the case, or if it is, the case will be much weaker than they say.

### **Conclusion**

There are two key issues in studying organizations. They are establishing causality and the testing of the validity of causal claims (Pfeffer, 1997). Indeed, these two issues are key for social science inquiry in general (Argyris & Schön, 1974; Kaplan, 1964; Popper, 1959).

In examining the literature that contains reviews on action research (Bargal, Gold, & Lewin, 1992; Deutsch, 1968; Eden & Huxham, 1996; Elden & Chisholm, 1993; Gold, 1992; Susman & Evered, 1978), it is clear that there exists agreement that action research is intended to explain problems and, in many cases, attempt to solve them, through the use of collaboration and participation. It is also clear that there exists a wide range of perspectives on how to accomplish these purposes and what methodologies to use.

As best I can determine, the majority of the contributors do not focus explicitly on causality and how it is to be established. Indeed, there is a significant proportion who claim that it is not possible or necessary to make causal claims. Many of those typically decry positivism as being out of date and irrelevant. The same group substitute a more humanistic approach, subjective, postmodern perspective.

There are several problems with these claims. The researchers using the subjective and humanistic approaches can also distance themselves from the "subjects." They appeared unaware that they were creating the distancing that they decried positivism for creating (Argyris, 1995). Moreover, taking the position that causality is not relevant or testable is itself a causal claim. How can those who decry causality make arguments about more relevant ways to explain phenomena that are, themselves, based on causal reasoning?

Finally, there is increasing evidence from studies of the mind that much thought and all of action is based on causality (Shoham, 1990; Simon, 1969). Those who wish to eliminate causality are responsible for making it clear how they believe the mind works without causality.

Action researchers may define their respective perspectives as they wish. But they cannot escape from defining what they mean by rigor (Heller, 1993, Peters & Robinson, 1984). Lewin's work and that of Schön and myself provide two metatheoretical perspectives that are relevant to dealing with this issue.

## References

- Argyris, C. (1980). *Inner contradictions of rigorous research.*, San Diego, CA: Academic Press.
- Argyris, C. (1982). *Reasoning, learning, action.* San Francisco: Jossey-Bass.
- Argyris, C. (1985). *Strategy, change and defensive routines.* Boston: Pitman.
- Argyris, C. (1987). Reasoning, action strategies and defensive routines: The case of OD practitioners. In *Research in organizational change and development* (vol. 1) (pp. 89–128). Greenwich, CT: JAI Press.
- Argyris, C. (1990a). *Overcoming organizational defenses.* Needham, MA: Allyn and Bacon.
- Argyris, C. (1990b). Inappropriate defenses against the monitoring of organizational development practice. *Journal of Applied Behavioral Science*, 26(3), 299–312.
- Argyris, C. (1993). *Knowledge for action.* San Francisco: Jossey-Bass.
- Argyris, C. (1995). Knowledge when used in practice tests theory: The case of applied communication research. In K. N. Cissna (Ed.), *Applied communication in the 20th century* (pp. 1–19). Mahwah, NJ: Lawrence Erlbaum.
- Argyris, C. (1996). Unrecognized defenses of scholars: impact on theory and research. *Organization Science*, 7(1), 79–87.
- Argyris, C., Putnam, R., & Smith, D. (1985). *Action science.* San Francisco: Jossey-Bass.

- Argyris, C. & Schön, D. (1974). *Theory in practice*. San Francisco: Jossey-Bass.
- Argyris, C. & Schön, D. (1996). *Organizational learning II*. Reading, MA: Addison Wesley.
- Aronson, E. (1972). *The social animal*. San Francisco: Freeman.
- Ash, M. G. (1992). Cultural contexts and scientific change in psychology: Kurt Lewin in Iowa. *American Psychologist*, 47(2), 198–207.
- Back K. (1966). This business of topology. *Journal of Social Issues*, 22(2), 51–66.
- Barker, R. G., Dembo, T., & Lewin, K., (1943). Frustration and regression. In R. G. Barker, J. Kounin, & H. Wright (Eds.), *Child behavior and development*. New York: McGraw-Hill.
- Bargal, D., Gold, M., & Lewin, M. (1992). Introduction: The Heritage of Kurt Lewin. *Journal of Social Issues*, 48(2), 3–13.
- Campbell, D. T., & Stanley, J. C. (1963). *Experimental and quasi-experimental design for research*. Skokie, IL: Rand McNally.
- Cartwright, D. (Ed.). (1951). *Field theory in social science*. New York: Harper.
- Cartwright, D. (1959). Lewinian theory as a contemporary systematic framework. In S. Koch (Ed.), *Psychology: A study of a science* (pp. 7–91). New York: McGraw-Hill.
- Coleman, J. S. (1972). *Policy research in the social sciences*. Morristown, NJ: General Learning Press.
- Cronbach, L. J., & Suppes, P. (Eds.). (1969). *Research for tomorrow's schools*. London: MacMillan.
- Cyert, R. M., & March, J. G. (1963). *A behavioral theory of the firm*. Englewood Cliffs, NJ: Prentice Hall.
- Deutsch, M. (1968). Field theory in social psychology. In G. Lindzey & E. Aronson (Eds.), *Handbook of social psychology* (pp. 412–487). Reading, MA: Addison-Wesley.
- Deutsch, M. (1992). Kurt Lewin: The tough-minded and tender-hearted scientist. *Journal of Social Issues*, 48(2), 31–43.
- de Waal, F. (1982). *Chimpanzee politics: Power and sex among apes*. New York: Harper and Row.
- de Waal, F. (1989). *Peacemaking among primates*. Cambridge, MA: Harvard University Press.
- Eden, C., & Huxham, C. (1996). Action research for the study of organizations. In S. R. Clegg, C. Hardy, & W. R. Nord (Eds.), *Handbook of organization studies* (pp. 526–542). London: Sage.
- Elden, M., & Chisholm, R. (Eds.). (1993). Emerging varieties of action research: Introduction to a special issue. *Human Relations*, 46(2) 121–142.
- Gold, M. (1992). Metatheory and field theory in social psychology: Relevance or elegance? *Journal of Social Issues*, 48(2), 67–78.
- Heller, F. (1993). Another look at action research. *Human Relations*, 46(10), 1235–1242.
- Johnson, D. (1993). Psychology in Washington. Measurement to improve scientific productivity: A reflection on the Brown report. *Psychological Science*, 4(2), 67–69.
- Kaplan, A. (1964). *The conduct of inquiry*. San Francisco: Chandler.
- Lewin, K. (1935). *A dynamic theory of personality*. New York: McGraw-Hill.
- Lewin, K. (1936). *Principles of topological psychology*. New York: McGraw-Hill.
- Lewin, K. (1938). *The conceptual representation and the measurement of psychological forces*. Durham, NC: Duke University Press.
- Lippitt, R. (1940). An experimental study of the effect of democratic and authoritarian group atmospheres. In K. Lewin, R. Lippitt, & S. K. Escalona (Eds.), *Studies in topological vector psychology I* (pp. 45–195). Iowa City: University of Iowa Press.
- Marrow, A. (1969). *The Practical theorist*. New York: Basic Books.
- Metraux, A. (1992). Kurt Lewin: Philosopher-Psychologist. *Science in Context*, 5(2), 373–384.
- Miller, G. (1956). The magical number seven, plus or minus two: Some limits on our capacity for processing information. *Psychological Review*, 63, 81–96.
- Naylor, J. C., Pritchard, R. J., & Ilgen, D. R. (1980). *A theory of behavior in organizations*. New York: Academic Press.
- Peters, M., & Robinson, V. (1984). The origins and status of action research. *Journal of Applied Behavioral Science*, 20(2), 113–124.
- Pfeffer, J. (1997). *New directions for organizational theory*. New York: Oxford University Press.
- Popper, K. (1959). *The logic of scientific discovery*. New York: Basic Books.
- Shoham, Y. (1990). Nonmonotonic reasoning and causation. *Cognitive Science*, 14, 213–302.
- Simon, H. A. (1969). *The science of the artificial*. Cambridge, MA: MIT Press.
- Susman, G. S., & Evered, R. D. (1978). An assessment of the scientific merits of action research. *Administrative Science Quarterly*, 23, 582–603.



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