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The Social Scientist as Practitioner: Barriers to Translating Scientific Knowledge into Practical Knowledge

The social scientists in the previous three chapters all might be regarded as practitioners at work within their own distinctive communities of inquiry. Some are basic researchers, others applied; some work within the empirical-analytic tradition, others within the hermeneutic-historical tradition. But as members of a community they all follow a set of rules that tell them which problems to go after and which ones to leave alone; what kinds of solutions to seek and when to consider a problem solved; what they should do as they go about solving their problems and what they should avoid doing. As Kuhn (1970b) describes them, these rules are a part of a practitioner's stock of knowledge. Acquired during his or her apprenticeship within a particular community, they are often simply taken for granted.

But periodically practitioners take stock of such rules, and the past two decades have been such a time in the social sciences. Critics both in and outside the field have debated whether the social sciences have been studying the right problems in the right way or whether these sciences have become part of the

problem rather than the solution (Mills, 1959; Zuniga, 1975; Caplan and Nelson, 1973; Ryan, 1976; Friedrichs, 1970). Out of this debate a gradual shift has taken place in the kinds of problems and methods considered legitimate for study. In the empirical-analytic tradition this has led to a move from a pure science orientation with an emphasis on laboratory methods to an approach more concerned with socially relevant problems and with developing methods that could travel beyond the confines of the laboratory (Reich, 1981; Campbell and Stanley, 1963). At the same time, those at work in the hermeneutic tradition have made a parallel move, no longer defining themselves strictly as dispassionate observers but gradually recasting their roles as helpers or advocates of those they study (Cassell, 1982; Spindler, 1982; Spradley, 1980).

Yet despite these new aspirations, researchers continue to solve their problems without taking into account what practitioners require to solve theirs. For a basic researcher, the problem is to describe and to account for some phenomenon, and for the applied researcher it is to figure out what can be done about it. The difficulty is that both consider their problems solved and their tasks complete long before considering the practitioner's problem of how to understand and act in real-life contexts amidst all the complexity and multiple dilemmas of value they pose. That problem lies beyond the borders of the communities of inquiry of the basic researcher and the applied scientist.

In this chapter we will consider an obvious point with some nonobvious implications: What you look for is what you get. Depending on the community in which he works, each researcher looks for different facts and solutions in accord with his own community's norms for inquiry. For our purposes we can distinguish four kinds of communities, representing basic and applied research within the two traditions in the social sciences (see Table 3).

What follows is a consideration of how these different community norms govern the practice of research and determine the kind of knowledge that is produced. To anticipate, we

Table 3. Communities of Inquiry.

Form of Research	*Tradition*	
	Empirical-Analytic	*Hermeneutic-Historical*
Basic	Milgram (experimental social psychologist)	Philips, Erickson, McDermott and Gospodinoff (descriptive ethnographers)
Applied	Hackman and Oldham, Lawler (assessment research/ organizational behavior)	Jordan, Heath (applied ethnographers)

Note: Many communities of inquiry exist within each of these four cells. This table focuses on generic differences among kinds of communities of inquiry.

found a paradox. By following the rules of their practice, the researchers in our case studies ended up with solutions that fell short of their own and practitioners' standards.

Figuring Out How It Happens to Be

In their practice as scientists, the basic researchers whose work we have discussed faced the theoretical problem of figuring out how some phenomenon happens to be. At the same time, the problems these researchers took on were not simply theoretical ones. Without exception they each studied a critical social problem: obedience to authority and failure to learn at school. Thus far we have a happy match between social relevance and potential usefulness, on the one hand, and theoretical requirements and interests, on the other. But now these researchers must construct some line of inquiry into these problems in order to solve their own problems, and not all lines of inquiry are equal in yielding socially useful results—even if they do lead into the most important and relevant of social problems.

First and foremost from the researchers' point of view is that an inquiry conform to the rules of their practice and then that it move along into domains most likely to yield a solution acceptable to its norms. We thus saw that Milgram constructed

his inquiry to follow the rules of the laboratory context by asking: "If an experimenter tells a subject to act with increasing severity against another person, under what conditions will the subject comply and under what conditions will he disobey?" (1974, p. xii). And his search for a solution went in a direction guided by his particular community. He looked at what subjects did under conditions relevant to a social psychologist, varying situational factors such as distance and roles in order to study their psychological impact.

The descriptive ethnographers took a different but equally systematic tack in framing their inquiry. Unlike Milgram, they were not bound by the constraints of the laboratory and were not required to tailor their questions to suit such constraints. Philips (1983) could therefore ask the less precise question of whether Indian children acquired distinctive communicative codes and, if so, whether this might account for school failure, while McDermott and Gospodinoff (1981) could ask whether there might be something functional about this dysfunctionality. Once framed, their questions could also give way to a less circumscribed search. The facts these ethnographers sought were defined neither a priori nor with precision, and they were free to pursue unanticipated hunches as they arose. Nevertheless these ethnographers were as bound to the constraints of their community as Milgram was to his. They each looked for facts with an ethnographer's eye, searching for similarities and differences in communicative codes and rules of interaction and looking for what elements in their subjects' ethnic identities and early socialization processes might account for these. These are ethnographic "facts," and the ethnographic situation is thus defined, as Scholte (1974) also argues, as much by the "ethnological tradition in the head" of the ethnographer as by the nature of the culture or problem before him.

As this suggests, the questions asked and the facts sought by the experimentalist and the ethnographer are guided by different rules, and their inquiries move down different paths. Yet all the researchers were trying to solve some descriptive puzzle and to do so within the parameters of their own tradition. Milgram created situations that *limited* or *produced* obedience in

order to describe and account for it, whereas the ethnographers delved into cultural factors that *led* to school failure in order to account for it.

So now the question arises: What do we get from these lines of inquiry? First of all, the "facts" they generate do not speak for themselves but must be organized into theories that can answer certain questions before they constitute a solution. Just as these researchers drew on existing conceptual tools to guide their search, they now can hang the facts they find on the conceptual structures available within their particular disciplines and traditions. Milgram therefore took his findings and formulated a solution in two steps. He first explained what led his subjects to obey by describing how certain situations resulted in psychological states that produced or limited obedient responses. Then as a basic researcher, he took a second step, asking the question "why obedience?" What does this tell us about human beings and the human condition? To answer this question, Milgram drew on what he called an evolutionary cybernetic model, "convinced" that these cybernetic principles were "very much at the root of the behavior in question" (1974, p. 125). Such models, Milgram explained, alert us to what "*must* occur" when an individual is brought into a hierarchical structure in which he no longer functions on his own but as a component of the system. And what does this model alert us to? Recall that Milgram's answer to "why obedience?" was that it was necessary for social coherence. For social organization to survive, those at the local level must cede control to those higher up. Hence over time human beings have acquired—actually have born into them—the potential for obedience. Without it social organization would be in jeopardy.

It is at this point that Milgram's problem of "why obedience?" was solved and his job was done. But notice that it is also at this point that his solution becomes our dilemma. If we foster disobedience, we may jeopardize the survival of social organization. Yet if we encourage obedience, we may jeopardize our responsibility toward other human beings.

The solutions of the ethnographers also present us with a dilemma. Once Philips (1983) and Erickson (1975) found their

facts, they too went on to put them within their tradition's assumptive and theoretical frameworks. Philips explained that teachers' inability to bridge or better handle differences was due to conflicting communicative styles that are so highly learned and skillful, some perhaps even neurologically based, that they lie outside of human awareness and control. Similarly, Erickson argued that regardless of our attempts to be fair by using universal criteria such as test scores in evaluating students, we automatically size people up by using particularistic and potentially unfair criteria such as race, ethnicity, and so on. Moreover, he added that these processes are so complex and highly learned that they cannot be performed reflectively or stopped at will.

It is at this point that the ethnographers' problem of "why failure?" was solved. But once again their solution becomes our dilemma, because what is thought necessary for competence as a teacher or counselor will necessarily lead to unfairness and failure. Framed this way, there is little we can do short of matching teacher and student according to race or ethnicity, a cure that may make the illness worse.

Recognizing this, McDermott and Gospodinoff attempted to turn this solution on its head by going after an alternative explanation for what looks like interethnic miscommunication and for what results in school failure. First, they called into question the assumption that ethnographers like Philips and Erickson take for granted. They pointed out that differences in communicative codes and rules are neither "natural" nor "in the long run irremedial," since studies such as Efron's (1941) have shown that ethnic groups can and do bridge differences even in their kinesic behavior. Next they cited studies and generated their own data to suggest an alternative explanation for the existence of seemingly dysfunctional differences. They argued that it is not that such dysfunctionality is necessary but that, paradoxically, it is functional within certain social arrangements.

Yet in the end even McDermott and Gospodinoff solved their problem in a way that put teachers and students in a box. "Our problem," they explained, "is that our school systems are set up to have conscientious teachers function as racists and

bright little children function as dopes even when they are trying to do otherwise" (1981, p. 226). In other words, faced with the present realities of school systems, teachers and students have little choice and are destined to fail. This solution thus gets them out of one dilemma only to plunk them down in a new one.

But what are the implications of such solutions for the problems these researchers study? The answer to that depends partly on whose problem we are considering. We know already that researchers will construct one kind of problem to suit their purposes and practitioners another to suit theirs. How good we consider these solutions to be depends on whose problem we would like to solve: the researcher's problem of how to describe causality within the requirements of their community of practice, or the practitioner's problem of how to transform causality in light of normative concerns. In what follows we will consider how adequately the researchers solved each problem.

Researcher's Theoretical Problem. In Part One we saw that there is a divergence in what counts as a solution in the empirical-analytic and the hermeneutic traditions. On the one hand, accounts in the empirical-analytic tradition speak of analyzing the relationships among events and of devising causal explanations that are abstracted from concrete situations and thus become generalizable. These accounts are as complete and precise as possible in order to be falsifiable. On the other hand, accounts of the hermeneutic tradition reject the notion of causal explanation and speak instead of understanding social action in the sense of grasping the logic of action—or more precisely, the meanings and intentions of actors embedded in the particulars of a concrete situation. As we already saw, at stake in what constitutes an adequate solution are different assumptions about the nature of action and human agency and how accounts of social action can best grasp them (Chapter Two).

But in practice researchers from both traditions have worked out an artful compromise in these domains. Most experimental psychologists now assume human agency, and with varying success try to take into account the rules, meanings, and intentions of their subjects, both in their methods and in

their formulations. In the same vein ethnographers often speak the language of the empirical-analytic tradition, putting the rules of actors in a causal context, considering the relationship among events, and working to rule out alternative explanations. Thus the borders distinguishing the two communities are not quite as distinct in practice as they are often thought to be. But at the same time, practitioners at work in these two communities cannot ignore their own tradition's criteria for what does and does not count as a good solution. The experimentalist must construct falsifiable explanations that have both scope and elegance, while the ethnographer must strive to accurately grasp the meanings and intentions of those she studies within the contexts in which they act. And while they can each get away with importing an occasional rule from the other's traditions, neither is permitted to move beyond the realm of explanation or understanding and into the realm of normative concerns. Researchers in the empirical-analytic tradition still aspire to keep values and facts separate and to stick to the world of facts in their solutions, whereas those in the hermeneutic tradition still try to take a disinterested stance toward their participants and to avoid imposing their own values on them.

In both traditions these norms simultaneously help and hinder the researchers' task. On the one hand, they serve to give their practice shape and meaning; while on the other hand, they put them in a double bind, because to meet one norm requires them to violate another. To illustrate what we mean, let us first consider Milgram's solution. In many respects it meets the criteria of the empirical-analytic tradition in which he worked. It disconfirmed the prevailing and erroneous view that obedience to an unjust authority is pathological or dispositional. It has a certain elegance in that it accounts for a wide range of facts with relatively few concepts. And it offers an explanation that speaks to the nature of human beings and their social institutions.

But herein lies the problem, because this explanation is not falsifiable within the norms of the empirical-analytic tradition. Recall that Milgram's solution put obedience in a hierarchical context, explaining that it is necessary for social coher-

ence (those at the lower levels *must* cede control to superiors). To falsify this explanation would require that we construct experiments that might disconfirm it. Yet this would require going beyond the experimenter's question, "What limits or produces obedience?" to ask, "What responses and conditions might make obedience unnecessary?" And it might require looking beyond existing responses and arrangements for an answer. Yet pursuing this kind of inquiry would violate the norms of this tradition, making Milgram's explanation nonfalsifiable within it—unless of course such possibilities arose naturally over time, an unlikely event since these kinds of explanations a priori rule out such possibilities. Put more generally, any social science explanation that assumes that existing social arrangements reveal the true or necessary nature of things risks creating solutions that contain errors that this tradition cannot detect.

An alternative line of inquiry should further articulate this argument. In the domain of action theory Harmon's (1981) consideration of accountability rules ended up with a proposition that, if tested, might show that obedience is not in fact necessary for social coherence. Yet at first, Harmon's formulation of the obedience dilemma was quite similar to Milgram's. He understood obedience within the same hierarchical context of accountability rules and arrangements that mete out rewards and punishments for obeying and disobeying and that lead to social norms that are internalized and followed. His understanding of the premise underlying accountability arrangements was also similar to Milgram's: In social situations, if the action of one person affects another, he should take the other into account in the interest of consistency and fairness. Finally, like Milgram he pointed out that the accountability arrangements designed to ensure consistency separate the "doer" from the "decider," thereby fostering our propensity to perform harmful acts without feeling personally responsible. If Harmon had stopped here, we would be no further along than we were with Milgram. He would be left with the notion that what is done in the interest of consistency undermines a sense of personal responsibility for one's actions.

But Harmon neither assumed the necessity of existing ac-

countability arrangements, nor was he bound by strictures to leave normative concerns alone. He thus went on to invent an alternative form of accountability that might better manage the obedience dilemma. It is as if he asked the question, "Given the value of the premise underlying the notion of accountability—but also given the consequences of current accountability arrangements—what form of accountability and what organizational arrangements might we create to satisfy consistency and maximize a sense of personal responsibility?" On two counts such a question falls outside the purview of the empirical-analytic tradition. It is explicitly normative in that it critically examines and puts forth what values or ends we should choose (consistency and personal responsibility), and it does not preclude inventing possibilities that exist outside of current arrangements in order to bring them about.

In answering this question, Harmon first eliminated possibilities unlikely to work on logical grounds. He ruled out unilateral discretion by those at the local level because "it runs the risk of being unchecked and arbitrary" (1981, p. 127). In so doing he rejected the only alternative to obedience in Milgram's formulation. Moreover, he did so for a reason similar to Milgram's: Fostering unilateral discretion would jeopardize consistency and coherence. He thus believed, as Milgram did, that the existing responses of ceding control or unilaterally taking it would be unlikely to manage the dilemma of conflicting requirements. Harmon therefore suggested adding an alternative to existing accountability rules, that is, a decision-rule that does not so sharply split "decider" and "doer" and that preserves consistency "without at the same time reducing a sense of personal responsibility" (p. 127). His own invention is a consensus rule under which participants must bilaterally negotiate their different views and interests, with no one person unilaterally imposing decisions on others. Under these conditions, he hypothesized that it would be less likely for one to act without feeling both personally responsible and accountable to others.

Without doubt this decision-rule is a significant departure from existing structures. Perhaps because of this Harmon speculated on what conditions might be necessary for such a

rule to be implemented: the creation of mutual trust, the specification of conditions under which the rule would be best used, the belief that it can work (the success of rules may in part be a self-fulfilling prophecy), and practice and experience in such decision modes. But our point here is not so much whether or not this particular decision-rule would work. Our point is that we cannot a priori assume that it will not. It is not only a normative question, it is also an empirical one. To ignore its empirical content is to risk constructing explanations that are wrong without being able to discover that they are wrong—a violation of the norm of falsifiability. At the same time, the empirical question is inextricably tied up with normative concerns, so that pursuing this line of inquiry would violate the fact-value rule. Either way the basic researcher in the empirical-analytic tradition would be stuck.

For similar reasons the ethnographers' solutions fall short of the criteria within their tradition. Philips and Erickson both assumed that rules of interaction and meaning making are essentially unalterable: They are so automatic and complex that they cannot be brought into awareness, reflected on, or stopped at will. But how do we know they cannot? What if no data exist to suggest that they are alterable simply because these changes have not yet occurred naturally? Then the only way to discover whether we can reflect on, stop at will, or alter these rules is by trying to do so. But this kind of inquiry would violate the rules that ethnographers must follow. Their role is to leave untouched what they see. But as a result they too may miss some very basic features in how we construct and interact in the world.

Both traditions have criteria by which to judge a good solution. What this analysis suggests is that some criteria get in the way of others. In both traditions basic researchers must follow rules that in some form say, "Describe what is accurate" and "Do not delve into normative concerns." Yet this latter rule makes it likely that both traditions will generate descriptions that contain mistakes they will be unable to discover.

Practitioner's Problem. Just as a researcher's solution is expected to meet certain criteria, so is a practitioner's. As we

saw in Part One, practitioners seek to understand in order to act; they try to transform, not to leave untouched, what they see; and they continuously grapple with conflicting values in the problematic situations they face. It therefore follows that their solutions should be of a certain sort. They should emphasize causal factors that are potentially within their control; they should inform practitioners how to transform what they see, even if that requires going beyond what now exists; and they should articulate some normative stance that will enable practitioners to manage conflicting values and ends. For the practitioner to be effective, he cannot ignore these requirements any more than a researcher can his. The problem is that the existing requirements for researcher and for practitioner may be quite incompatible, making the prevailing division of labor model quite questionable.

We already know that the basic premise underlying this model is that the findings of social science can contribute to the solving of social ills (see Part One). Society is to hand its problems over to the social sciences, and the social sciences are to give back theory to be applied toward their solution (Schön, 1983; Geuss, 1981). For their part basic researchers are supposed to offer explanations that can better frame social problems, thereby helping practitioners to solve them. But do they? The way a problem is framed *can* influence the solutions that are chosen (Schön, 1983; Kahnemann and Tversky, 1984). But as illustrated, the problem frames in our case studies imply solutions that fall short of the requirements a practitioner must face. These researchers gave little if any guidance on normative concerns, and they emphasized causal factors assumed to be outside a practitioner's control: historical factors (early socialization at home and at school); situational factors (inherent and necessary organizational or systemic constraints); genetic factors (membership in racial or ethnic groups); and responses thought to be automatic (nonverbal cues, reasoning processes outside our conscious awareness, and highly learned actions). At the same time they simply ruled out the possibility of gaining control over such factors. Philips and Erickson argued that our automatic responses are so far beyond our control that they

cannot be altered. Milgram argued that the situational factors that foster obedience are so necessary that our potential for obedience is actually inbred. Only McDermott and Gospodinoff implied the possibility of some form of social system change, and even they said they hadn't the "foggiest notion" of how to go about achieving it. So once practitioners come face to face with these problems, there is little they can do. They have no control over the key causal factors involved. Hence the solutions of basic research become our dilemmas.

The paradox is that it may be the very efforts of these researchers to be fair and empathic that generates the practitioner's dilemma. Without exception every researcher stressed that his or her participants did not intend the consequences they described. Philips emphasized that teachers and students were not to be blamed for not comprehending one another, because this lack of comprehension resulted from early socialization processes. Erickson underscored that his counselors were neither malevolent nor "incompetent" but individuals who acted "professionally" and yet could not help but size people up in ways that might lead to unfair results (1975, p. 68). McDermott and Gospodinoff spoke of "conscientious teachers" with no choices, given the circumstances they faced (1981, p. 228). And Milgram spoke of obedience as a distressing but necessary and functional response to organizational necessities. Such stances toward participants has the positive effect of taking into consideration what practitioners are up against in the world. Actors (participants) are thus more apt to feel understood. Observers (readers) will be less apt to take a "holier than thou" perspective that can blind them to their own potential to act similarly. And we will all be less naive about the obstacles that must be faced in managing these dilemmas. The problem is that these effects are bought at the price of leaving us helpless to act differently.

But what if a researcher recognized the constraints and good intentions of actors, while also inquiring into their responsibility for acting in ways that necessarily create unintended consequences? Such a stance would require an alternative set of assumptions. The first would be that actors have and make

choices—no matter how tacit they may be. The second would be that it is possible for such tacit choices to lead to consequences unintended at a fully conscious level. And the third would be that under certain conditions, it may be possible to gain access to and control over such choices. Under this set of assumptions, actors are not viewed as morally reprehensible for creating these consequences, but they can and should be held personally responsible for creating them. Later on we will describe how a researcher enacts this stance in relation to participants during the research process (see Chapter Nine). But for now, let us consider how it leads to qualitatively different solutions.

One such example can be found in Schön's (1983) study of how practitioners reflect in action. In one case study he described how a town planner found himself caught in a dilemma, in this case between obligations toward developers and obligations toward local regulatory bodies. To describe this dilemma, Schön began by inquiring into, and providing a rich description of, the contextual factors a planner faces, including a historical analysis of the conflicts inherent in the role as it has developed over time. But then he brought to the foreground other factors —most importantly, how the planner himself chose to frame his role as he interacted with the two parties and how this led him to construct a balancing act that put him in a dilemma. Notice that this account of the planner's dilemma recognizes both role and situational constraints, while highlighting what the practitioner *chose* to do to compound them, thereby generating the dilemma he faced.

At the same time, Schön regarded the town planner as well intentioned, "an individual who likes to reflect on his practice" (p. 228). He was thus faced with making sense of what prevented the planner from discovering and correcting his mistakes. Previous researchers have explained these puzzles by assuming that their participants' actions were necessary and/or outside of their control. But if Schön ascribed choice, how was he now to account for someone's choosing to act against his own intentions? Like some of the other researchers, Schön

started out by assuming that the town planner was unaware of his inconsistency. But he did not then go on to assume that this unawareness was either necessary or out of the planner's control. Instead he hypothesized that the planner limited his reflection by focusing only on his strategies and ignoring how he framed his role and the situation before him. Moreover, this role itself was reinforced by theories of action that led the planner and those around him to keep private understandings that might have increased awareness and stimulated reflection had they been public.

So far Schön has formulated an explanation of the town planner's dilemma and his unawareness of the factors contributing to it. In contrast to the previous researchers, he has focused on a set of possible causes that may potentially be within the practitioner's control and therefore alterable. To pursue this possibility, Schön also pushed his inquiry past the point where the other researchers stopped. He asked the question, "What *might* have happened if, contrary to fact, the planner had become aware of his mistake? In what direction might his inquiry have gone?" (p. 230). As Schön wrote, this is a "peculiar question" because, according to his analysis, it would require the town planner to hold an alternative and rare theory of action. But as peculiar as it might be, the question was pivotal to the solution that Schön was developing, one that might help the planner work through his dilemma, while exploring whether and how this might be possible.

In the end Schön's research led to a solution that explained not only what led to the planner's dilemma but what he might do to manage it better, namely, learn an additional theory of action. To arrive at that insight, Schön had to develop a different line of inquiry. After asking the questions "what if" and "what stopped him from being aware," he had to ask questions that went "contrary to fact" and to look for different causal factors: not only the constraints inherent in the planner's role and the situation but his methods of constructing both and how he might have reconstructed them within those constraints. This inquiry itself depended on a somewhat differ-

ent assumption. Schön had to assume not only that individuals construct their behavioral worlds but that they can reconstruct them if they so choose. As a result, Schön's inquiry could lead to a solution that might enable the planner to better manage the dilemma described in it.

This is not to say that the other researchers in our case studies offered no suggestions for getting out of the dilemmas they described. But they approached them from a different angle. As we saw in the case studies, most of them implied or suggested changes at the level of policy or structure alone without rigorously considering the implications of such changes or how to implement them at the level of action. For instance, Philips suggested matching teachers with students by ethnic or racial membership without addressing the possibility that this could foster further intergroup alienation at the expense of the less powerful group. And in most basic research such possibilities do go unexplored. It is not the basic researcher's job to rigorously consider the institutional and human implications of their suggestions. A brief conjecture on the "practical" implications of their results is sufficient for their purposes. Consequently, these researchers are unlikely to ever learn whether their advice generates more problems than it solves and are even less likely to take up the question of "why."

Conclusion. Each of the basic researchers in our case studies set out to solve some theoretical problem that involved a critical social problem. Although the empirical-analytic and hermeneutic traditions differed in the "facts" they sought and in the solutions they devised, both emphasized causal factors assumed to be largely outside of a practitioner's control. As a result they ended up formulating solutions that put practitioners into formidable dilemmas, and it was unclear whether their advice would get them out or generate new ones (see Table 4). But is is not only the practitioners who find themselves in a dilemma. The researchers do as well. By following the rule "Do not delve into normative concerns," they cannot fully satisfy the rule "describe the world accurately" or generate solutions that might be more helpful to practitioners.

Table 4. The Framing of Problems and Solutions (Basic Research).

Form of Research	*Questions Asked*	*Causal Factors Found*	*Assumptions Made*	*Solution Formulated*	*Knowledge Produced*
Ethnographers	What is it? How does it happen to be?	Emphasizes: • Differences and similarities in socialization among cultural groups • Existing rules of interaction and meaning making; communication codes; social identity. Recognizes: • Situational factors	Basic features of the world are revealed by describing the world as is Rules of interaction and meaning making are highly automatic and skilled, not apt to be altered.	Causal explanation that describes and accounts for the world as is	For science: Description of existing causal relationships that may be: • incomplete: they miss the deep structures that maintain it • inaccurate: they contain assumptions that can be difficult to falsify No descriptions of fundamental alternatives to what exists.

Experimentalists	What is it? How does it happen to be?	Emphasizes: • Situational factors • Existing psychological and behavioral responses Recognizes: • Socialization processes	Basic features of the world are revealed by describing the world as is Existing arrangements and responses are functional and not alterable without jeopardizing organizational survival.	Causal explanation that describes and accounts for the world as is	For practitioners: • Insight into how factors outside of a practitioner's control lead to dilemmas • Little insight into what practitioners do within those constraints to maintain and reinforce them. • Little insight into how practitioners might act to transform dilemmas once face-to-face with them.

Figuring out How to Achieve a Given Set of Ends

Applied researchers work within the same two traditions as their basic research colleagues, but they use the tools of these traditions to figure out how to achieve some given set of ends. Hence, while they scan the solutions of basic research for clues, they then adhere to a distinctive set of rules and assumptions that allow them to use these clues to solve practical problems without violating the basic tenets of their respective traditions. This kind of building process can first be illustrated by looking at how the applied ethnographers set out to solve the problem of school failure. Like their descriptive colleagues, they subscribed to a "difference" rather than a "deficiency" model to explain failure at school. But for these researchers their problem began rather than ended here, because their task was to solve the problem of what can be done to help children to succeed at school (see Chapter Six). In Jordan's view this translated into making schools "compatible with the culture of the client population in ways that contribute to effective education" (1981, p. 16). In Heath's view it meant helping children to learn the four Rs: "to 'learn school,' meaning its rules and expectation, just as they . . . 'learn readin', writin', and 'rithmetic' " (1983, p. 281). With these different ends in mind, both researchers then went after the same question. As Heath described it, "The question was *how*?" (p. 281).

To answer this question, Heath and Jordan independently looked for ways they might use teachers' and students' existing cultural knowledge, skills, and rules in order to bridge the differences between them. Jordan emphasized that she was not looking for "radical" change in school practices (1981, p. 16), while Heath stressed that she and her teachers sought "to accommodate" group differences while teaching students mainstream rules of interaction (1983, pp. 284, 354). In taking this tack, their inquiries stayed squarely within their research traditions. They adhered to the prevailing ethnographic norm that admonishes against disrupting the rules and norms of different cultural groups, and they went after the same kinds of ethnographic "facts" sought earlier by basic researchers. At the same

time, neither accepted the solutions of their descriptive colleagues without significant modification. For instance, while Heath drew on some of Philips' insights, she emphasized verbal rather than nonverbal rules of interaction, and assumed a far greater capacity to overcome differences in communicative codes among different cultural groups. For Heath's purposes such modifications were essential. As we saw earlier, without them the problem of how to bridge these differences in face-to-face interaction becomes insoluble.

A similar building process can be traced in the field of organizational behavior. Among our case studies we considered the use of assessment models such as that of Hackman and Oldham (1975) in diagnosing and advising organizational clients (see Chapter Five). Such models are often used in organizations because they are designed to explain how a complex set of interdependent variables lead to certain outcomes or goals thought to be related to organizational effectiveness. In this instance the researchers were interested in such outcomes as employee motivation, satisfaction, and productivity. But such models might focus on any number of goals from a long and diverse list of possibilities—a fact of organizational life that has rendered efforts to study and measure effectiveness problematic. How does one ever know which goals to set as appropriate criteria? In practice, most researchers have answered this question by picking some goal of "substantial interest" to scholars and participants and studying how organizations may reliably be expected to achieve it (Mohr, 1982, pp. 190-191). The important question therefore is not what ends to choose but how to achieve them.

Hackman and Lawler (1971) initially pursued this question in the field of social psychology, where they drew on the theories of Lewin (1938) and Tolman (1959) to develop a conceptual framework for understanding job design. Several years later Hackman and Oldham (1975) expanded on this work by elaborating a model that could be applied by organizations interested in evaluating jobs and job redesign. The model itself holds a strong resemblance to the causal structure found in most theories within social psychology. Certain antecedent

conditions (mostly situational) are thought to bring about certain key psychological states, modified by individual characteristics and leading to certain behavioral consequences (compare the causal logic in Milgram's (1974) formulation of obedience in the previous section).

At this point the applied researchers had framed their problems. Because their traditions diverge, however, their frames focused on different kinds of causal factors. The ethnographers emphasized communicative codes, rules of interaction, and interactional contexts, while the assessment researchers in organizational behavior stressed situational factors such as task identity and skill variety and their effect on psychological and behavioral variables. Nevertheless each researcher pursued those causal relationships thought to be pivotal in achieving the ends they had set.

So now the question once again becomes: What kinds of solutions do these lines of inquiry yield? Just as the causal factors found by basic researchers did not in and of themselves constitute a solution, neither does the description of key causal relationships constitute a solution for applied researchers. Instead they must describe how these causal factors might be manipulated in order to produce the desired result. No matter how implicit or loosely formulated, some theory of intervention must be developed before their problem is solved.

In the case of the ethnographers we have two theories of intervention designed to meet somewhat different ends. To solve the problem she set, Heath developed strategies aimed at teaching children the four Rs—not only school subjects but rules of school interaction. She sought to do so by developing ways for teachers, parents, and children to discover and build on students' existing rules, so that they might meet existing requirements for interaction at school and later on in life. In contrast, the KEEP project described by Jordan was aimed at a somewhat different set of ends. Its policy was to adapt schools to children's culture instead of asking children to acquire the rules of the school's culture: "The assumption is that the correct course, for both practical and ethical reasons, is not to attempt to change the children or their families to fit the schools,

but rather to modify the schools in ways that will allow them to serve minority children more effectively" (Jordan, 1981, p. 16).

As this suggests, Jordan and Heath may have started out with the same model to explain school failure, but they ended up stressing somewhat different ends and strategies for closing the gap between school and children. Heath's teachers, and presumably Heath, considered it only "humane" to prepare children as soon as possible for what lies ahead by adding to their repertoire of rules (1983, p. 281). In contrast Jordan thought it only "practical and ethical" to ask schools to change to fit children's rules. Yet despite these differences a sense of obviousness pervaded the legitimacy of the different ends they each set. Neither researcher indicated that she regarded them as choices to be critiqued and probed. Jordan referred to, but did not make explicit, the "practical and ethical" reasons behind KEEP's policy, as if its "correctness" was so apparent that it could go unstated.

Working within a different tradition and with a different problem, Hackman (1983) and Hackman and Oldham (1980) built on their previous work by developing a set of principles that described how to manipulate the key situational factors identified in their framing of the problem. These included redesigning structures and policies thought to affect such features of job design as task significance, autonomy, and feedback in order to engender psychological states believed to increase productivity and satisfaction at work: a sense of meaningfulness, responsibility, and knowledge. Like the ethnographers, these researchers act as if they too regarded the ends they set as somehow given in the problem. Nowhere did they question whether they were the right ends to set, nor did they suggest how they might be evaluated in light of other organizational outcomes or ends.

What, then, are the implications of these kinds of solutions? In their solutions the basic researchers did not rigorously pursue the question of how practitioners might solve the problems they had framed. It was not that they ignored or lacked concern for the question but that, within the division of labor model, the task of answering it is assigned to applied researchers

and practitioners. How well we now think the applied researchers have done depends once again on whose problem we consider: the applied researcher's problem of how to achieve some given set of ends, or the practitioners' problem of how to understand and take action in a real-life context with the conflicting ends and values it poses. We will now look at the solutions of applied researchers in light of both sets of criteria.

Applied Researcher's Problem. What qualifies as a good solution for the applied researcher is a tricky question, because it involves both explicit and tacit criteria. On the one hand, the solution is supposed to tell us how to achieve the ends set in the framing of the problem; while on the other hand, it should be constructed in accord with a set of tacit rules that keep applied researchers within the norms of their particular tradition. One such rule governs the process of choosing among ends, while a second set tells researchers how to search for and choose among strategies for achieving these ends. It is assumed that, by following such rules, researchers can steer clear of normative concerns while meddling in practical affairs, thereby protecting their status as scientists. But at the same time these rules may hinder their ability to solve the problems they set without creating new ones. What follows is a consideration of these rules and their implications for problem-solving effectiveness.

• *Rule 1:* Ends should be regarded as "given" in the problem. We just saw that the question for applied researchers was not "what ends ought we to choose" but "given these ends, *how* do we achieve them?" They thus regarded the ends they set as somehow given in the problems they framed. But obviously ends do not materialize through a process of spontaneous generation. They must be set by someone somehow and without stepping into the normative realm. To do so, most applied researchers subscribe to the goal-oriented logic described earlier. If researchers or participants hold an interest in a goal, then this is a sufficient criterion for making the goal worthy of pursuit and for inquiring into efficacious ways of achieving it (Mohr, 1982).

For different reasons this criterion holds in both traditions. In the empirical-analytic tradition organizational research-

ers speak of pursuing ends in the service of effectiveness, and they strive to keep values separate from fact in the course of this pursuit by confining their inquiry to what they regard as the empirical question: What is the most efficacious and reliable way of achieving this set of ends? In the hermeneutic tradition we arrive at a similar destination but by way of a different rationale. The ethnographers like Hymes speak of pursuing ends in the service of communicative competence (Philips, 1983), and they strive not to impose their own or others' values or ends on participants by confining their inquiry to a similar question: How can we help participants achieve the ends they have set? This assumption has a normative bent in a dual sense. It asserts that there is value in avoiding value questions, and it assumes that it is neither necessary nor desirable to make ends the object of inquiry.

But there is a problem with this logic. Practitioners and institutions alike hold multiple and often conflicting ends that they have an interest in satisfying (Kelly, 1955; Pfeffer, 1981; Mohr, 1982; Keeley, 1984). In the pursuit of one set of ends it is thus not unlikely to violate or come up against others. Yet the applied researchers set their problems as if they were unaware of this possibility or, at the very least, regarded it as peripheral to their inquiry. But Heath's project suggests that it may not be peripheral. As her research unfolded, Heath discovered that Trackton students were unfamiliar with the rules of politeness used by teachers in giving commands and that as a result they neither understood nor followed them. In an impromptu experiment she thus asked her teachers if they would try using more directive rules for about a month. The teachers agreed, and for the following month they made explicit commands instead of hinting or making indirect requests. So rather than say something like, "Can we get ready on time?" they more often said things like, "Put your toys back where you took them from. We have to line up for lunch" (Heath, 1983, p. 283; also see Heath, 1982, p. 112, for an additional description of the experiment).

What they discovered provides important insights into the problem posed by conflicting ends. Despite the teachers' desire

to adopt rules familiar to students, they were dissatisfied with what happened once they succeeded in doing so: "They reported they felt they did not involve their children when they used statements. They received no sense of interaction and felt they were 'preaching' to a third party; they could not be sure they were being heard. They viewed questions as a way to 'share talk' with children of this age" (Heath, 1982, p. 112).

This suggests that the most sincere efforts to adopt compatible rules may come up against other values or ends in which teachers also have a stake. In this case the teachers felt that the new rules ran counter to their highly valued sense of shared talk and interaction. So their wish to adopt directive rules violated their simultaneous desire to experience shared interaction. Once brought to the surface, however, this dilemma of conflicting values was never pursued. Instead, for unstated reasons some teachers simply returned to their own rules of politeness, whereas others continued to use directives while they taught students how to use hints and indirect requests (Heath, 1983, p. 283). But either way they bypassed the twofold question of whether their sense of shared talk was in fact shared and whether it was in the interest of student learning. And it may not be in their interest since shared talk is predicated on rules of indirectness that are by nature ambiguous and a source of misunderstandings; moreover, in this case the student neither shared nor followed these rules.

If we take this possibility seriously, then the question of what ends teacher and student ought to pursue itself becomes worthy of pursuit. The ends at stake are multiple, they are conflicting, and still new ones may be discovered as the inquiry unfolds further. But most important, which ends are pursued holds critical empirical and normative implications. As we saw in this case, shared talk may not in fact be shared, and its pursuit may not serve learning. Thus ends are not "given" but are a matter of continual choice, and the question is not *whether* we ought to make normative choices, but *how* we and our participants ought to make them. At present, we regard such choices as obvious. Alternatively we might regard them as choice points subject to critical inquiry, and we might make and

revise our choices explicitly on the basis of mutual self-interest and in light of the empirical data that emerge as the inquiry goes forward. If we do the latter, then our task as researchers is to create conditions that would enable participants not just to achieve certain ends but to choose among them under conditions of free and informed choice (see Geuss, 1981; also see Keeley, 1984, for an interesting discussion on the conflicting criteria used to adjudicate conflicting ends).

• *Rule 2:* Scan basic research in your field of inquiry for problem-solving clues and discard those that do not fit applied purposes. This two-step rule governs the way applied problems are set by guiding the researcher's search process. The first step is characteristic of all normal science, and it specifies what facts and problems are legitimate, not by way of explicit rules but by way of exemplars and models that tacitly guide a researcher's search. As such it acts as a kind of "box." It is unlikely to "call forth new sorts of phenomena; indeed those that will not fit the box are often not seen at all" (Kuhn, 1970b, p. 24). But as Kuhn also points out, its very restrictions are what enables science to expand the scope and precision of its knowledge. It is thus a two-sided box. It at once advances existing knowledge and makes fundamentally new insights less likely.

Both sides of this building process can be seen in the applied social sciences as well. Over the course of a decade Hackman and his colleagues--Hackman and Lawler, 1971; Hackman and Oldham, 1975, 1980; and Hackman, 1983—went from the field of basic research to developing change principles that could be used in organizations. In retracing this process, we can see the implications of the search rule not only for science but also for applied researchers' ability to solve the problems they set. To review: In conceptualizing their problem, Hackman and Lawler (1971) first scanned the theories within their own research tradition for problem-solving clues, and they then drew on its instruments to pursue these clues, generating and testing propositions that built on Lewin's (1938) and Tolman's (1959) work within social psychology. Several years later Hackman and Oldham (1975) then organized these propositions into a comprehensive model characteristic of most theories within social

psychology. Yet as they did so, they made sure that the model was useful. They designed it so that each major class of variables could be measured; they developed a job diagnostic survey that could be used to assess jobs and redesign programs; and finally they outlined a set of design principles that described how the situational factors identified in the model could be manipulated, primarily through policy or structural changes (Hackman, 1983).

This building process enabled Hackman and his colleagues to contribute to knowledge in their field while making it more useful to practitioners. At the same time, the case study on the consulting firm (see Chapter Five) suggests that this process might also lead practitioners to miss facts critical to solving their problem. Recall that the firm's management set forth a policy on feedback, but then found themselves unable to implement it because the managers lacked the requisite interactional rules to do so and were unaware that this was the case. Neither Hackman and Oldham's model nor their diagnostic instrument is apt to discover this gap or to give us much guidance on how to fill it. Their community of inquiry does not ask its practitioners to look for these kinds of facts (tacit rules of interaction), nor has it developed the instruments that enable us to see them. These facts, along with the instruments that allow us to see them, belong to the ethnological tradition and thus are apt to go unnoticed in this one. As a result policies that cannot be implemented are apt to get approved; and since everyone remains unaware of the gaps between them and our rules of interaction, they may create more rather than less dissatisfaction. This way the blinders we wear as researchers could end up reinforcing those of practitioners. Our own change strategies may remain insufficient for solving the problems we set. And worse yet, we may not be able to *see* what the difficulty is.

Another side to this box comes in the form of how applied researchers select clues as they scan their respective fields for them. We saw that Heath took from basic research those insights and assumptions that were useful in setting her problem, thus adopting the difference model of basic researchers like Philips. But she discarded those assumptions that would make

the problem insoluble, thus rejecting the notion that these differences are either necessary or irreconcilable. This way she could use basic research to set her applied problem without letting its assumptions get in the way. But what she did not do is actively set out to disconfirm these assumptions, feeding back her results in order to revise basic theory and research. It is a rare event for applied results to ever come back to the basic realm, since it is still not regarded as a theory-building endeavor and applied researchers seldom regard their own roles in that light (see Bickman, 1981). Consequently their change efforts do not tell us when our basic assumptions about the world are unwarranted. Applied research thus overlooks one of the most critical pathways for the advance of science, if not the most critical one.

- *Rule 3:* Pick problem-solving strategies that fit within the existing constraints and norms of the practitioner's community. This is a selection rule that asks researchers to solve their problems with strategies that are compatible with existing organizational arrangements and norms of interaction. At a minimum it rules out strategies that fundamentally question or challenge what exists. The applied researchers in the case studies discussed here adhered to such a rule as they formulated each of their solutions. Jordan stressed that she was not necessarily recommending radical change in school practices but rather "an effort to select from the wide spectrum of available teaching practices and curricula, those that are compatible with the culture of the client population" (1981, p. 16). Similarly, once Heath's teachers found their new rules to be incompatible with their own values, they retreated from the change effort, dissatisfied with the results. On the one hand, then, strategies within "available" practices are sought. But on the other hand, once it becomes evident that a strategy departs from those practices, retreat is the preferred course.

There is much to be said for recognizing existing constraints and norms, since this will prevent us from underestimating what we are up against. The problem lies in a priori accepting them as nonnegotiable, thereby missing ways of solving the problems we set. In Chapter Six we showed how the oscilla-

tion between direct strategies and indirect strategies made it impossible for teachers to fully resolve the dilemma of conflicting rules that they faced. The indirect rules were ambiguous; the direct ones preachy. Either way the teachers could not be certain that they had been heard. But suppose we invented an alternative that combined directness with an inquiry into the others' reactions? We might state this rule as "Combine advocacy with inquiry," and it falls within a Model II theory-in-use. As such it is often espoused but rarely practiced (more typical is the oscillation between advocacy and inquiry described in Chapter Six). Therefore it lies outside of existing rules, and learning it would require reexamining existing norms such as those of politeness. Nevertheless it may resolve the teachers' dilemma in a way that oscillating between the two existing rules cannot. However, the point is not whether it would, because that is an empirical question that cannot be answered here. The point is that an unspoken rule stops applied researchers from considering possibilities that go beyond what exists, and yet some dilemmas may require just that, if we are to solve them. This rule thus diminishes our problem-solving effectiveness by a priori ruling out strategies that might solve the problems we set.

Practitioner's Problem. Practitioners do not evaluate outcomes by a singular set of ends given ahead of time. Some purposes practitioners may bring to the problematic situations they face; others they may discover only once they are in them. They thus evaluate outcomes in the light of multiple values and purposes, some of which may not be discovered until they act to transform situations. The teachers in Heath's project illustrated this. They achieved what they set out to do only to discover that the goal of compatible rules was incompatible with others they held, thus leaving them dissatisfied with the results. At an organizational level the consulting firm solved one mismatch by legislating the feedback that the consultants demanded, but it simultaneously created a new mismatch for the officers who lacked the skills to provide it. As these practitioners considered such results, the question they asked was not just "Did we achieve the ends we set" but "Do we like what we

get?" and "Is it congruent with our fundamental values and theories?" (Schön, 1983, pp. 132-133). This suggests that effectiveness holds a special meaning for practitioners. It is not sufficient to achieve a desired end. It is necessary to do so without unknowingly creating undesired ends. So practitioners must figure out not only how to achieve a given end but how to negotiate and renegotiate the often conflictual ends they discover in problematic situations. For the practitioner, the question of "what ends" takes center stage.

Without doubt this is a messy question. No obvious criteria exist for choosing among the ends involved in setting problems and evaluating solutions. Nevertheless for practitioners it is an unavoidable task, and they receive no guidance on it from applied research as it is now defined. Problem solving for the applied researcher is confined to yielding reliable knowledge on how to achieve some end, ordinarily in the service of effectiveness. Just as Hackman and Oldham put satisfaction and production at work in the context of organizational effectiveness, so did Jordan speak of KEEP's policy in terms of educational effectiveness. At the center of applied research is the question, "how do we achieve a given end," and the question of "what ends" is relegated to the periphery. We saw already that ends are regarded as given, their correctness so obvious it can go unstated (see Jordan, 1981, p. 16) and their worthiness justified on the basis that participants hold them as a goal of interest (Mohr, 1982, pp. 190-191).

Such problem-solving logic is predicated on the assumption that it is neither necessary nor desirable to discriminate among ends. But this raises the question: not necessary or desirable for whom? As Keeley points out: "For theorists, it may be convenient to adopt a thoroughgoing relativism, but not for those who actually take part in administering complex organizations" (1984, p. 5).

To Keeley, it is not that these conflicts go unnoticed by researchers, nor is it that they are seen as unimportant for administrators. Rather he believes it is an extreme form of relativism that "permits them to say little about means of resolution and [to] feel no embarrassment about leaving such con-

flicts unresolved" (p. 5). But it may be that for researchers, it *is* necessary and desirable to adopt this relativist viewpoint and to leave these conflicts alone. As long as researchers can assume that ends are given by others and are not a matter of their own normative choice, Mohr writes, they can be normative in a "special sense": "The instruction is not that organizations should behave in a manner derivable from the nature of God, human beings, or the healthy society, as in much pure normative philosophy; rather the emphasis is on how an organization should behave in order to be effective and efficient. The advice is therefore based . . . on the *empirical* hypothesis that certain structures or behaviors will be functional, or efficacious, in performing a task" (1982, pp. 2-3, our italics).

Such a view permits researchers with an interest in practical affairs to take them up without giving up their status as researchers. Their question remains an empirical one; their concerns normative only in a special sense. There is much validity to this view. Whether an end can be met by one means as opposed to another *is* an empirical question. But it does not yet answer the question of how we choose among possible ends to study, and the criterion of "sufficient interest" does not allow us to circumvent the question. As Keeley points out, it is not obvious why organizational goals have more objective validity than other evaluative standards, such as those derived from individual rights (1984, p. 2) or, in light of Mohr's distinction, those derived from some notion of a healthy society. Nevertheless the applied researcher may have to veer away from this question in order to remain faithful to the norms of science. So the very question the practitioner must answer, the applied researcher must leave alone. In this sense applied research might be regarded as quite impractical despite its concern with practical affairs (see Keeley, 1984).

Conclusion. The communities in which scientists practice hold norms that tell us what questions and facts to go after, what constitutes a good solution, and what to do and avoid as we go about solving problems. Like all practice norms, they give shape, meaning and direction to our task by defining what lies within and outside its boundaries. This chapter has tried to

identify these norms, the way they bind our inquiry, and the implications of this for solving the problems that researchers and practitioners face. What we found is that existing scientific norms may lead to dilemmas that cannot be resolved within them. The basic researchers formulated solutions that placed practitioners between necessary and conflicting requirements. To Milgram, the obedience that leads us to harm others is necessary for social coherence and organizational survival. To Philips and Erickson, the cultural differences that lead to school failure are based on processes that are so highly skilled and automatic, perhaps even neurologically based, that they are necessary for competence and perhaps beyond our control.

Such conclusions were derived from describing the world as is in accord with the norms of scientific communities. Yet to falsify them or to discover systematic gaps in them would require going beyond these norms. It would require that we ask whether what exists is necessary for existence, that we invent fundamental alternatives that might resolve these dilemmas, and that we submit them to experimentation. Moreover, since it would be neither practical nor ethical to design such alternatives on an arbitrary basis, we would need to construct them in light of existing empirical evidence and normative analysis, as Harmon (1981) did in developing his decision rules. Otherwise such experiments might do more harm than good or be a waste of time. But to move in this direction would be to violate the rules "Do not delve into normative concerns" and "Do not pose fundamental alternatives to what is." So just as practitioners are left in a dilemma, so are the researchers, and theirs seals shut the one they construct for practitioners.

In the applied realm the opportunity exists to break open these dilemmas. Applied researchers can intervene in practical affairs and manipulate causal variables to bring about desired outcomes. But to protect their status as scientists, they must circumvent normative questions and consequently cannot give practitioners much guidance on dilemmas of value. The ethnographers do not question the different and conflicting values held within and among cultural groups, while organizational researchers provide no suggestions on how organizations might ad-

Table 5. The Framing of Problems and Solutions (Applied Research).

Form of Research	*Questions Asked*	*Causal Factors Found*	*Assumptions Made*	*Solution Formulated*	*Knowledge Produced*
Applied Ethnographers	How do we achieve a given set of ends? What are the key causal factors involved?	Emphasizes: • Differences and similarities in socialization among cultural groups • Existing rules of interaction and meaning making; communication codes; social identity. Recognizes: • Situational factors	Ends can be regarded as given Conflicting ends do not need to be taken into account Solutions can be found within the existing constraints and norms of both the researchers' and the practitioners' communities	Theory of intervention designed to bridge differences in rules of interaction and cultural contexts through accommodating existing rules	For science: • Knowledge on how to achieve a given end within existing constraints • Few fundamentally new insights: —fundamental alternatives are not produced —assumptions of basic research are not revised in light of applied knowledge

Organizational Assessment	How do we achieve a given set of ends? What are the key causal factors involved?	Emphasizes: • Situational factors • Existing psychological and behavioral responses Recognizes: • Interpersonal factors	Ends can be regarded as given Conflicting ends do not need to be taken into account Solutions can be found within the existing constraints and norms of both the researchers' and the practitioners' communities	Theory of intervention designed to manipulate situational factors through policy and structural changes.	For practitioner: • Insight into how to achieve a certain end within existing constraints • Little insight into how to negotiate conflicting ends in the problem-solving process • Little insight into fundamentally new options or into new criteria by which to evaluate them.

judicate conflicting interests. Instead, they take a set of ends held by some group of participants, and then inquire into how to achieve them according to the search and selection rules described previously (see Table 5). What lies outside of existing norms for inquiry and practice is considered peripheral and/or goes unnoticed. Fundamental alternatives are not invented, and conflicting values or interests are bypassed rather than engaged. Consequently, the assumptions of basic research do not get tested, and the conflicting requirements often embedded in them do not get resolved. In this way what is done to satisfy the demands of science and practice may in fact thwart the advance of both.