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KNOWLEDGE FOR THEORY AND PRACTICE

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We examine three related ways in which the gap between theory and practice has been framed. One approach views it as a knowledge transfer problem, a second argues that theory and practice represent distinct kinds of knowledge, and a third incorporates a strategy of arbitrage—leading to the view that the gap is a knowledge production problem. We propose a method of engaged scholarship for addressing the knowledge production problem, arguing that engaged scholarship not only enhances the relevance of research for practice but also contributes significantly to advancing research knowledge in a given domain.

Understanding the relationship between theory and practice is a persistent and difficult problem for scholars who work in professional schools, such as business, engineering, social work, medicine, agriculture, education, public administration, journalism, and law. Professional schools typically build their raison d'être on the mission of developing knowledge that can be translated into skills that advance the practice of the professions (Kondrat, 1992; Simon, 1976; Tranfield & Starkey, 1998). But, as evidenced by the often lamented gap between theory and practice, this mission remains an elusive ideal.

Several special issues in leading academic journals[1] have highlighted growing concerns that academic research has become less useful for solving practical problems and that the gulf between theory and practice in the professions is widening (Anderson, Herriot, & Hodgkinson, 2001; Rynes et al., 2001). There is also increasing criticism that findings from academic as well as consulting studies are not useful to practitioners and do not get implemented (Beer, 2001; Gibbons et al., 1994). Academics are being criticized for not adequately putting their research into practice (Beyer & Trice, 1982; Hodgkinson, Herriot, & Anderson, 2001; Lawler, Mohrman, Morhman, Ledford, & Cummings, 1985). Professional knowledge workers, as well, are criticized for not being aware of relevant research and not doing enough to put their practice into theory (Van de Ven, 2002; Weick, 2001). As a result, organizations are not learning fast enough to keep up with the changing times.

In this paper we focus on the relationship between theory and practice in the field of management. We do not attempt a comprehensive review of the debate, either in general or with respect to the management literature. Rather, we examine three ways in which the gap between theory and practice has been framed, and we then focus on one approach that we believe moves the discussion forward in a productive way.

The gap between theory and practice is typically framed as a knowledge transfer problem. This approach is based on the assumption that practical knowledge (knowledge of how to do things) in a professional domain derives at least in part from research knowledge (knowledge from science in particular and scholarship more broadly). Hence, the problem is one of translating and diffusing research knowledge into practice. A second approach views knowledge of theory and practice as distinct kinds of knowl-

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[1] The relationship between management science and practice has received much attention in special issues of the *Academy of Management Journal* (Rynes, Bartunek, & Daft, 2001), *Academy of Management Executive* (Bailey, 2002), *Administrative Science Quarterly* (Hinings & Greenwood, 2002), *British Journal of Management* (Hodgkinson, 2001), and several other more specialized management journals.

edge. Each reflects a different ontology (truth claim) and epistemology (method) for addressing different questions. To say that the knowledge of theory and practice are different is not to say that they stand in opposition or they substitute for each other; rather, they complement one another. This leads to a third view—namely, that the gap between theory and practice is a knowledge production problem. After reviewing the problems and assumptions of the first two approaches, we propose a method of *engaged scholarship* in which researchers and practitioners coproduce knowledge that can advance theory and practice in a given domain.

The following is a preview of our argument. Relating theory and practice poses the important question of how individuals and organizations develop the means for addressing complex problems in the world. To bridge the gap between theory and practice, we need a mode of inquiry that converts the information provided by both scholars and practitioners into actions that address problems of what to do in a given domain—thus, our proposed method of engaged scholarship is a means of creating the kind of knowledge that is needed to bridge this gap. We define engaged scholarship as a collaborative form of inquiry in which academics and practitioners leverage their different perspectives and competencies to coproduce knowledge about a complex problem or phenomenon that exists under conditions of uncertainty found in the world. Engaged scholarship is consistent with an evolutionary realist philosophy of science, which is a pluralistic methodology for advancing knowledge by leveraging the relative contributions and conceptual frameworks of researchers and practitioners. Engaged scholarship also frames a given problem as an instance of a more general case so that theoretical propositions can be developed and applied in specific contexts of practice.

Our argument for engaged scholarship is based on the concept of arbitrage—a strategy of exploiting differences in the kinds of knowledge that scholars and practitioners can contribute on a problem of interest. Arbitrage is commonly known in financial circles as the exploitation of price differentials (Harrison, 1997). But, as noted by Friedman (2000), one can do arbitrage in literature as well as in markets. In his analysis, Friedman goes on to show how arbitrage can lie at the heart of sensemaking in a world of diverse and distributed knowledge. Indeed, Ghemawat (2003) has suggested arbitrage as a basis for the globalization of business strategy.

Intellectual arbitrage is a common (although often unstated) objective of interdisciplinary research. We propose that by making the concept of arbitrage explicit and by extending it to the activities of research teams composed of scholars and practitioners, we can gain significant insight into the means needed to address problems in today's world. By leveraging their distinct competencies, groups composed of researchers and practitioners have the potential to ground and understand complex problems in ways that are more penetrating and insightful than they would be were either scholars or practitioners to study them alone.

Because arbitrage is a dialectical form of inquiry, participants often experience conflict and interpersonal tensions that are associated with juxtaposing people with different views and approaches. We argue that managing conflict constructively is not only important but lies at the heart of engaged scholarship. Focusing, as we have in the past, on tensions between scholars and practitioners is a mistake, for it blinds us to the very real opportunities that are possible from exploiting the differences underlying these tensions in the knowledge production process.

Past arguments for collaborative research have tended to be one-sided and to focus on the relevance and use of academic research *for* practice. Less attention has been given to how scholarship that is engaged *with* practice can advance basic research knowledge. We adopt the perspective that engaged scholarship not only enhances the relevance of research for practice but also advances research knowledge in a discipline. We agree with Hodgkinson et al. (2001) and Pettigrew (2001) that research needs to achieve the dual objectives of applied use and advancing fundamental understanding.

A KNOWLEDGE TRANSFER PROBLEM

The gap between theory and practice is typically formulated as a knowledge transfer problem. Practitioners fail to adopt the findings of research in fields, such as medicine (Denis & Langley, 2002), human resources (Anderson et al., 2001; Rynes, Colbert, & Brown, 2002), and management (Rogers, 1995; Tranfield, Denyer, & Smart, 2003), because the knowledge that is pro-

duced is not in a form that can be readily applied in contexts of practice. Action scientists such as Argyris and Schön (1996) have focused on the characteristics and behaviors of researchers to explain this lack of implementation of research knowledge. They argue that scientific knowledge will be implemented only if researchers, consultants, and practitioners jointly engage in interpreting and implementing study findings (Schein, 1987; Whyte, 1984).

Academic researchers are criticized for paying little attention to transferring the knowledge they produce (Beyer & Trice, 1982; Lawler et al., 1985). Beer (2001), for example, recommends that researchers take responsibility for specifying how the knowledge they produce should be implemented. He also discusses how customary knowledge transfer practices often inhibit implementation of proposed solutions, such as use of authoritarian or coercive styles of imparting knowledge, defensiveness routines by teachers and researchers, and self-interested recommendations by consultants that maintain or increase clients' dependence on their consulting services.

Mohrman, Gibson, and Mohrman (2001) empirically examined the perceived usefulness of research by practitioners in a context where researchers were not playing an action-oriented interventionist role. They found that practitioners in ten companies undergoing change viewed research results as useful when they were jointly interpreted with researchers and when practitioners had opportunities to self-design actions based on the research findings. Mohrman et al. concluded that "perceived usefulness requires far more than simply doing research in relevant areas" (2001: 369). Moreover, "it would seem that researchers must do more than work collaboratively with organizational members to understand research findings. Perhaps they must become part of an organization's self-design activities if they wish to promote usefulness" (2001: 370).

Utilization researchers have focused on the characteristics of potential users that inhibit the adoption and diffusion of knowledge—much like the adoption and diffusion of innovations (Backer, 1991; Beyer & Trice, 1982; Rogers, 1995). As Estabrooks states, "Many factors get in the way of using research, and empirically, we know very little about what makes research use happen or not happen" (1999: 15). For example, one issue currently being studied is what happens to knowledge during the transfer and adoption process. Golden-Biddle, Locke, and Reay (2002) have questioned the prevailing view that knowledge remains the same in its movement from researcher to user. They have found that the nature and use of knowledge changes dramatically as it is adopted and appropriated. Users selectively interpret and use knowledge as it serves their own purposes, fits their unique situations, and reflects their relations with their practicing community. Further studies like this will provide an understanding of what influences users to adopt and modify selective bits of information in a message and to ignore the rest.

What makes information convincing and, therefore, utilized is a rhetorical question (Van de Ven & Schomaker, 2002). Rhetoric is the use of persuasion to influence the thought and conduct of one's listeners. To Aristotle, the art of persuasion comprises three elements: (1) *logos*—the message, especially its internal consistency (i.e., the clarity of the argument, the logic of its reasons, and the effectiveness of its supporting evidence); (2) *pathos*—the power to stir the emotions, beliefs, values, knowledge, and imagination of the audience so as to elicit not only sympathy but empathy; and (3) *ethos*—the credibility, legitimacy, and authority that a speaker both brings into and develops over the course of the argument or message (Barnes, 1995). Logos, pathos, and ethos together shape the persuasiveness of any communication.

The persuasiveness of a theory is in the "eyes" of the listener (not just the speaker) and requires appreciating the context and assumptions of the audience or listeners. For example, Davis (1971, 1986) argues that what influences readers to view a theory as interesting or classical is the degree to which the writer challenges the readers' assumptions. In a nutshell, a classic work speaks to the primary concerns or assumptions of an audience, whereas an interesting theory speaks to the secondary concerns of an audience. Interesting theories negate an accepted assumption held by the audience and affirm an unanticipated alternative. Knowledge transfer is not only a function of the logic and data supporting a message but also the degree to which the speaker is viewed as a credible witness and is able to stir the human emotions of listeners.

One problem of viewing the gap between theory and practice as a knowledge transfer problem is the assumption that practical knowledge

derives from research knowledge. The divide between academics and practitioners is no accident. Many academic scholars have been socialized in a "trickle down" view of the knowledge supply chain: knowledge is created and tested by academic researchers, taught to students by instructors, adopted and diffused by consultants, and practiced by practitioners (sic). However, as Boyer (1990), Starkey and Madan (2001), and Van de Ven (2002) point out, academic researchers do not have a monopoly on knowledge creation. Practitioners and consultants discover anomalies and insights from their practices, just as teachers do with their students and scholars do with their research. But the knowledge that researchers, teachers, consultants, and practitioners create by themselves is different and partial. It is also highly dependent on context and purpose, as we discuss next.

THEORY AND PRACTICE AS DISTINCT FORMS OF KNOWLEDGE

In her review of the theory-practice gap, Kondrat (1992) points out that what has been missing from the discussion is empirical studies of knowledge from practice. What knowledge does the practitioner of an occupation or profession use, and how does he or she obtain it? What does the practitioner think, and how does he or she go about constructing thought and action (e.g., see Johnson, Grazioli, Jamal, & Berryman, 2001; Johnson et al., 2002; Johnson, Zualkernan, & Tukey, 1993)? What does the competent practitioner know, and how does he or she go about knowing "in" practice (Schön, 1987)? These and similar questions have received limited attention, but they are crucial to understanding the relationship between theory and practice (Dreyfus & Dreyfus, 1998).

Instead of beginning with definitions of knowledge "for" practice, Kondrat (1992) argues that the starting point should be these empirical questions. They reverse the usual order of business, which privileges formal-technical scientific knowledge and assigns a derivative status to the "practical" as a secondary way of knowing. Rather than regard practical reasoning and knowledge as a derivative of scientific knowledge, these questions address the epistemological status of "practical knowledge" as a distinct mode of knowing in its own right (see also Wallace, 1983). "When this status is granted, the practical takes its place alongside the scientific as constitutive elements of professional knowledge" (Kondrat, 1992: 239).

The recognition that research and practice produce distinct forms of knowledge has been long-standing in the literature. In "The Nicomachean Ethics," Aristotle (1955) made distinctions between techne (applied technical knowledge of instrumental or means-ends rationality), episteme (basic knowledge in the pursuit of theoretical or analytical questions), and phronesis (practical knowledge of how to act prudently and correctly in a given immediate and ambiguous social or political situation).[2] Habermas (1971) made explicit distinctions between technical and practical knowledge, which overlap Aristotle's distinctions. He viewed practical knowledge as tacit and embodied in action and technical knowledge as formal, explicit, propositional, and discursive. Polanyi (1962), Latour (1986), and Nonaka (1994) made similar distinctions but pointed out that both scientific and practical knowledge have tacit and explicit dimensions.

Studies of technical workers by Barley (1986), Orr (1990), and Lave and Wenger (1994) describe how immersion in a task or job produces an inseparable mixture of technical understandings and tacit, informal practices that shapes how people take actions on an everyday basis and what sorts of activities are relevant and effective for task performance in different situations. So also, studies of working scientists and scholars by Garfinkel, Lynch, and Livingston (1981), Latour (1986), and Knorr-Cetina and Amann (1990) indicate that improvisation underlies the process in which scientists actually construct models, enact experimental runs, design and interpret data, report on their methods and findings, and as-

[2] Flyvbjerg (2001) proposes that social science should eliminate its quest for epistemic scientific knowledge and should focus instead on developing practical phronetic knowledge. His proposal implies that social scientists should become policy makers. Both science and policy fulfill crucial roles in society, and each should be preserved and strengthened in its own right. Moreover, we will argue that leveraging the different perspectives that epistemic and phronetic knowledge can bring to bear on complex problems provides a way to dissolve the gap between theory and practice.

sign credit for discovery. Both practitioners and scientists engage in what Levi-Strauss (1966) termed *bricolage,* improvising with a mixed bag of tools and tacit knowledge to adapt to the task at hand.

Scholarly work and managerial work differ, however, in the context, processes, and purposes of their practices. The context of the practitioner is situated in particular problems encountered in everyday activities (Hutchins, 1983; Lave, 1986). As such, managers develop a deep understanding of the problems and tasks that arise in particular situations and of means-ends activities that make up their solutions (Wallace, 1983). Knowledge of management practice is typically customized, connected to experience, and directed to the structure and dynamics of particular situations (Aram & Salipante, 2003). In contrast, scholarship is committed to building generalizations and theories that often take the form of formal logical principles or rules involving causal relationships. "Scientific knowledge involves the quest for generality in the form of 'covering' laws and principles that describe the fundamental nature of things. The more context free, the more general and stronger the theory" (Aram & Salipante, 2003: 1900).

Both forms of knowledge are valid; each represents the world in a different context and for a different purpose. The purpose of practical knowledge is knowing how to deal with the specific situations encountered in a particular case. The purpose of scientific and scholarly knowledge is knowing how to see specific situations as instances of a more general case that can be used to explain how what is done works or can be understood.

Objectivity has often been viewed as at the heart of scientific knowledge; its methods of argumentation, criticism, and empirical mapping of reality have been considered to be based on efforts to justify, test, and replicate the integrity of what is known as distinct from the perspective of the one who knows it (Popper, 2002; Zald, 1995). But since the demise of the received view of positivism and logical empiricism in the philosophy of science, it is now widely recognized that scientific knowledge cannot be known to be objective and true in an absolute sense (Suppe, 1977). Rather, from an evolutionary realist perspective,[3] there is a real world out there, but our attempts to understand it are severely limited and can only be approximated. For example, Giere states:

> Imagine the universe as having a definite structure, but exceedingly complex, so complex that no models humans can devise could ever capture more than limited aspects of the total complexity. Nevertheless, some ways of constructing models of the world do provide resources for capturing some aspects of the world more or less well than others (1999: 77).

"In the absence of unambiguous foundational truth in the social sciences, the only sensible way forward can be conscious pluralism" (Pettigrew, 2001: S62). Researchers construct models that represent or map intended aspects of the world and compare them with rival plausible alternative models (Azevedo, 2002; McKelvey, 1997). Research knowledge advances through a comparison of the relative contributions and perspectives provided by different models. As Azevedo (1997) discusses, it is through the coordination of multiple models and perspectives that robust features of reality can be distinguished from those features of reality that are merely a function of a single model or framework. A research finding, principle, or process is judged to be robust when it appears invariant

[3] Baum and Rowley observe that "organization theorists have *never* been positivists. . . . Organization theorists of all orientations appear, instead, to practice a logic-in-use that is primarily 'scientific realist,' which is the most widely accepted epistemology among current philosophers (Azevedo, 1997; Suppe, 1997; 1989)" (2002: 20, 21). Realism is the thesis that a real world exists "out there," independent of what we think, but our attempts to know it are limited, and we can only know it through a socially constructed language system (Zald, 1995). All facts, observations, and data are theory laden and embedded in language. As a consequence, "all knowledge is presumptive" (Campbell, 1988: 487). No form of inquiry can be value free and impartial; instead, each model and perspective is value full. That being the case, any given conceptual model is a partial representation of reality reflecting the perspective and interests of the model builder. A researcher must therefore be critically reflexive, stating clearly whose point of view and interests are served in a model proposed to represent reality (Van Maanen, 1995). This critical realism should be distinguished from "relativism," which holds that truth testing is problematic because the external world does not exist beyond that which is perceived and socially constructed by individuals and cultures. Relativists argue that truth is relative to a specific paradigm, and competing paradigms are considered incommensurable because each possesses its own language and logic (Baum & Rowley, 2002).

(or in common) across at least two (and preferably more) independent contexts, models, or theories. A pluralist approach of comparing multiple plausible models of reality is therefore essential for developing objective scientific knowledge. Campbell (1988) adds that the models that better fit the problems they were intended to solve are selected by users, and the gradual winnowing down of plausible rival models or hypotheses by the scholarly community produces an evolutionary conception of the growth of scientific knowledge.

In contrast, practical knowledge advances through a more subjective involvement of one who knows and acts. The personal standpoint of the individual engaged in praxis yields a kind of knowledge that is critical to effective, practical action (Barley, 1986; Dreyfus & Dreyfus, 1998; Hayek, 1945; Wenger, 1998). Knowing how to do something emerges through continuous dialogue among practitioners (Nonaka, 1994). Understanding is transactional, open ended, and inherently social. The inquirer does not stand outside the problematic situation like a spectator; he or she is in the situation and in transaction with it (Schön, 1983). Being in the situation—and fully referenced to it—is a prerequisite for understanding it through action. Thus, knowledge of practice "is in the action" (Schön, 1983: 56).

But the subjective knowledge of the practitioner is also complemented by detachment. Any interactive situation supplies multiple, possibly valid perspectives, available only to the individuals who can relinquish their personal standpoint. This form of objectivity "allows us to transcend a particular viewpoint and develop an expanded consciousness that takes the world in more fully" (Nagel, 1986: 5). Schön maintains that, in situations of ambiguity or novelty, "our thought turns back on the surprising phenomenon, and at the same time, back on itself" (1987: 68), as a form of abductive reflection-in-action. Such reflection, in fact, is one way that practical knowledge becomes refined and extended into practice wisdom. As Schön states, "When someone reflects while in action, he becomes a researcher. He is not dependent on the categories of established theory and technique but constructs a new theory of the unique case" (1983: 68). Furthermore, Nonaka observes that "people do not just passively receive new knowledge; they actively interpret it to fit their own situation and perspectives. What makes sense in one context can change or even lose its meaning when communicated to people in a different context" (1994: 30).

Users of both scientific and practical knowledge demand that it meet the dual hurdles of being relevant and rigorous in serving their particular domains and interests (Pettigrew, 2001). However, different criteria of relevance and rigor apply to scientific knowledge and practical knowledge because their purposes, processes, and contexts are different. The relevance of each form of knowledge should be judged in terms of how well it addresses the problematic situation or issue for which it was intended (Dewey, 1938). Relevance, we suggest, is a matter of degree. In terms of the traditional quartet of description, explanation, prediction, and control (Rescher, 2000: 105), the relevance of some knowledge to a given problematic situation may entail any (or all) of the following:

- description (answering *what* and *how* questions about the problematic situation),
- explanation (addressing *why* questions about the problematic situation),
- prediction (setting and achieving expectations about the problematic situation), and
- control (effective intervention in the problematic situation).

Management scholars debate these and other criteria of usefulness. As Brief and Dukerich (1991) discuss, the debate often turns on whether the usefulness of knowledge to managers and organizational practitioners should focus on the control criterion (contain actionable knowledge that prescribes what to do to resolve a problem) or whether it should include more broadly the other criteria (knowledge that describes or explains a phenomenon and thereby provides a model for viewing and understanding "what *may* be, and not to predict firmly what *will* be" [Brief & Dukerich, 1991: 328]). Argyris and Schön (1996), Beer (2001), Starkey and Madan (2001), and Cummings and Jones (2004) argue that knowledge must be actionable if it is to be useful to managers. March (2000), Grey (2001), Kilduff and Kelemen (2001), and Weick (2001), among others, caution against restricting useful knowledge to this control criterion because it is far too narrow, instrumental, and may lead to focusing on shallow and short-sighted questions of performance improvement instead of addressing larger questions and fundamental issues.

The above criteria of relevant knowledge are not mutually exclusive. Indeed, Baldridge, Floyd, and Markoczy (2004) empirically found a positive relationship between the academic quality (number of citations) and practical relevance (judged by a panel of executives, consultants, and human resource professionals) of a sample of 120 articles published in top academic management journals. However, they caution that the relatively low correlation (r = .20) leaves significant room for cases where judgments diverge or there is no relationship at all (Baldridge et al., 2004: 1071). The relationship between academic quality and practical relevance often evolves over time. Thompson warns against the pressure for immediately applicable research results, because it

> leads to the formulation of common-sense hypotheses framed at low levels of abstraction, without regard for general theory, . . . and thereby reduces the ultimate contributions of the research to administrative science. Morover, [it] . . . often leads to the application of ideas whose unintended and unrecognized costs may be greater than their positive contributions (1956: 110).

We may have misunderstood the relationship between practical and scholarly knowledge, and this has contributed to our limited success in bridging these two forms of knowledge in arenas of human activity. Exhortations for academics to put their theories into practice and for managers to put their practices into theory may be misdirected because they assume that the relationship between knowledge of theory and knowledge of practice entails a literal transfer or translation of one into the other. Instead, we take a pluralistic view of science and practice as representing distinct kinds of knowledge that can provide complementary insights for understanding reality.

Each kind of knowledge is developed and sustained by its own professional community, which consists of people who share a common body of specialized knowledge or expertise (Van Maanen & Barley, 1986). Each community tends to be self-reinforcing and insular, and limited interactions occur between them (Cook, Scott, & Brown, 1999; Zald, 1995). Each form of knowledge is partial—a way of seeing is a way of not seeing (Poggi, 1965). Strengths of one form of knowledge tend to be weaknesses of another. Once different perspectives and kinds of knowledge are recognized as partial, incomplete, and involving inherent bias with respect to any complex problem, then it is easy to see the need for a pluralistic approach to knowledge coproduction among scholars and practitioners. In the next section we suggest a strategy of engaged scholarship to leverage the different perspectives of researchers and practitioners.

A KNOWLEDGE PRODUCTION PROBLEM

There is growing recognition that the gap between theory and practice may be a knowledge production problem. In part, this recognition is stimulated by critical assessments of the status and professional relevance of practice-oriented social science (Gibbons et al., 1994; Hinings & Greenwood, 2002; Simon, 1976; Whitley, 1984, 2000). Huff (2000) and Starkey and Madan (2001), among others, have questioned the traditional mode of research practiced in business and professional schools over the past fifty years and have proposed alternative modes of knowledge production. Common to these assessments is the view that a key defining characteristic of management research is its applied nature. For example, Tranfield and Starkey (1998) have suggested that the central concern of management scholarship should be the general problem of design. They argue that the development of practice-based scientific knowledge represents a distinctive role for management researchers. Producing this kind of knowledge locates the field in the nexus between practice and contributing disciplines, hence positioning management research within the social sciences as equivalent to engineering (in the physical sciences) or medicine and agriculture (in the biological sciences).

A variety of suggestions have been made for producing this kind of practice-based knowledge. Many suggestions have been institutional in nature, such as modifying academic tenure and reward systems, funding criteria for competitive research grants, and editorial policies and review procedures of academic journals and creating additional outlets for transmitting academic findings to practitioners (Anderson et al., 2001; Dunnette, 1990; Hodgkinson et al., 2001; Lawler et al., 1985). Structural reforms such as these are important institutional arrangements that enable and constrain research. But analyses of structural reforms tend to overlook the activities of individual researchers. In what fol-

lows we focus on recommendations that have more immediate relevance to individual scholars engaged in the knowledge production process.

At the level of the individual researchers, Pettigrew formulates the problem this way:

> If the duty of the intellectual in society is to make a difference, the management research community has a long way to go to realize its potential. . . . The action steps to resolve the old dichotomy of theory and practice were often portrayed with the minimalist request for management researchers to engage with practitioners through more accessible dissemination. But dissemination is too late if the wrong questions have been asked (Pettigrew, 2001: S61, S67).

He goes on to say that a deeper form of research that engages both academics and practitioners is needed to produce knowledge that meets the dual hurdles of relevance and rigor for theory as well as practice in a given domain (see also Hodgkinson et al., 2001).

Pettigrew sketches a vision that is not limited to business school research but, rather, reflects a much larger movement of engaged scholarship to transform higher education (Zlotkowski, 1997–2000). Indeed, to Ernest Boyer (1990), a leading proponent of this movement, engaged scholarship consists of a set of reforms to break down the insular behaviors of academic departments and disciplines that have emerged over the years. Engaged scholarship implies a fundamental shift in how scholars define their relationships with the communities in which they are located, including other disciplines in the university and practitioners in relevant professional domains. Engagement is a relationship that involves negotiation and collaboration between researchers and practitioners in a learning community; such a community jointly produces knowledge that can both advance the scientific enterprise and enlighten a community of practitioners. Instead of viewing organizations as data collection sites and funding sources, an engaged scholar views them as a learning workplace (idea factory) where practitioners and scholars coproduce knowledge on important questions and issues by testing alternative ideas and different views of a common problem. "Abundant evidence shows that both the civic and academic health of any culture is vitally enriched as scholars and practitioners speak and listen carefully to each other" (Boyer, 1996: 15).

These notions of engaged scholarship are extended with the strategy of intellectual arbitrage (Harrison, 1997)—to exploit the differing perspectives that scholars from different disciplines and practitioners with different functional experiences bring forth to address complex problems or questions. Arbitrage represents a dialectical method of inquiry where understanding and synthesis of a common problem evolve from the confrontation of divergent theses and antitheses. Arbitrage is not a strategy for addressing narrow technical problems where one chooses experts whose judgments converge on a correct answer. Instead, it is a strategy for triangulating on problems by involving individuals whose perspectives are different (Mitroff & Linstone, 1993). In a complex world, different perspectives make different sorts of information accessible. By exploiting multiple perspectives, the robust features of reality become salient and can be distinguished from those features that are merely a function of one particular view or model.

An arbitrage strategy is essentially a pluralistic methodology. Azevedo (2002) points out that communication across perspectives is a precondition for establishing robust alternative models of a problem. She adds:

> Individual theories are not considered true or false. Rather their validity is a function not only of how well they model the aspect of the world in question but of how connected they are, in terms of consistency and coherence, with the greater body of scientific knowledge. These connections can be established a number of ways not unproblematical, but communication across perspectives and willingness to work toward establishing coherence is a precondition (Azevedo, 2002: 730).

One of the problematic interpersonal aspects of arbitrage is conflict, which is the generating mechanism of a dialectical process of inquiry. Conflict is an inevitable part of work among diverse investigators who hold pluralistic views of a given reality. A research team that is organized along the principles discussed below guarantees the existence of conflict. An understanding of arbitrage must therefore place conflict and power at the center of inquiry. In such circumstances, creative conflict management is a central challenge of engaged scholarship research teams. A strategy that oppresses conflict

among investigators suppresses freedom of inquiry and learning. Research teams that encourage task-oriented conflict but manage interpersonal conflict provide a more effective form of inquiry than do forms of scholarship that are discursive, detached, and consensus dependent (Jehn, 1995; Flyvbjerg, 2001).

How might this dialectical form of engaged scholarship be undertaken to exploit the differing perspectives of scholars and practitioners? Although scholars debate alternative proposals, we believe that the following dimensions make up the basic strategy of arbitrage that lies at the heart of the knowledge production process. Flyvbjerg (2001) and Aram and Salipante (2003) offer suggestions that complement ours.

Design the Project to Address a Big Question or Problem That Is Grounded in Reality

Engaged scholarship is a collaborative form of research, because the real-world problems that it is designed to address are too complex to be captured by any one investigator or perspective (Azevedo, 1997). Caswill and Shove (2000) point out that there are many significant questions and problems whose formulation and theoretical development depend on engagement and close interaction between scholars and practitioners. Big questions have no easy answers and seldom provide immediate payoffs to practitioners or academics (Pettigrew, 2001). By definition, big questions often do not have clear solutions until after the research has been conducted and policy questions have been addressed. Big questions also require a process of arbitrage, in which researchers and practitioners engage with one another to coproduce solutions whose demands exceed the capabilities of either researchers or practitioners by themselves (Hodgkinson et al., 2001). Thus, at the time a research project is designed, prospective solutions to research questions are secondary compared to the importance of the research question being addressed. A good indicator of a big question is its self-evident capability to motivate the attention and enthusiasm of scholars and practitioners alike. Indeed, as Caswill and Shove state, practitioners are "often more attracted by new ideas and concepts than by empirical materials" (2000: 221).

A frequent suggestion for studying important research questions is to involve scholars from different disciplines and practitioners from different functional areas (Gibbons et al., 1994; Hinings & Greenwood, 2002; Pettigrew, 2001; Simon, 1976; Van de Ven, 2000). This suggestion is based on the arbitrage strategy that scholars can significantly increase the likelihood of advancing knowledge for theory and practice when they interact with practitioners in undertaking four interrelated activities during the research process:

1. Ground the research question or problem in concrete and observable phenomena in order to appreciate and situate its multiple dimensions and manifestations.
2. Develop plausible concepts and models that represent the main aspects of the observed phenomena and that thereby provide a base for new theories to address the central research question.
3. Use appropriate methods to design the research and obtain empirical evidence of the concepts and plausible models for examining the question about the phenomenon being examined.
4. Apply and disseminate the research findings to address the research question from the perspectives of different academic and practitioner users (Aram & Salipante, 2003; Van de Ven, 2000).

Critics have argued that practitioner involvement in formulating research questions may steer the questions in narrow, short-term, or particularistic directions (Brief & Dukerich, 1991; Grey, 2001; Kilduff & Kelemen, 2001). Ironically, this argument seems to assume that academics must be left to formulate researchable questions. Yet when interacting with practitioners, the interests of researchers may be co-opted by the interests of powerful stakeholders. Like Anderson et al. (2001), we take a more humble view of the academic researcher who stands in a more egalitarian relationship with practitioners and other stakeholders when trying to arbitrage an important research question or phenomenon. Big research questions tend to reside in a "buzzing, blooming, confusing world" (Van de Ven, 1999: 1). Learning the nature of the question or phenomenon in such ambiguous settings is facilitated by obtaining divergent perspectives of numerous stakeholders. Heedful accommodation and integration of diverse viewpoints yields a richer gestalt of the question being investigated than the sensemaking of a single stakeholder (Morgan, 1983; Weick, 1995).

Caswill and Shove (2000) critique the assumption that theoretical advances require academic detachment and that collaborative research merely implements and exploits, but does not advance, social theory.

> The trouble is that arguments about independence and interaction, and about theory and application are readily and sometimes deliberately confused. In everyday discussion, it is sometimes asserted, and often implied, that interaction outside the academy is so demanding of time and mental energy that it leaves no room for creative thought. In addition, when distance is equated with purity, and when authority and expertise is exclusively associated with analytic abstraction, it is easy (but wrong) to leap to the conclusion that calls for interaction threaten academic inquiry (Caswill & Shove, 2000: 221).

Indeed, the belief that interactions between people with different views and approaches advance academic (and practical) inquiry lies at the heart of the arbitrage process.

Design the Research Project to Be a Collaborative Learning Community

One of the most widely endorsed suggestions for improving the exchange of knowledge between researchers and practitioners is for academics to collaborate with practitioners in designing, conducting, and implementing research in real-world settings (Anderson et al., 2001; Beyer & Trice, 1982; Lawler et al., 1985; Miller, Greenwood, & Hinings, 1997; Rynes, McNatt, & Bretz, 1999). Research teams in which one or more members are relative insiders in a setting and one or more members are relative outsiders have been argued to offer distinct advantages for integrating diverse perspectives on the problem or phenomenon being investigated (Evered & Louis 1981; Louis & Bartunek, 1992; Van de Ven & Ferry, 1980).

While the composition of coinvestigators varies with the topic and question, the heart of this activity is that research projects should be collective achievements in learning among collaborating faculty, students, and practitioners. Only rarely would a researcher undertake a study as a lone fieldworker—as is the typical experience of many researchers. Instead, the research team would consist of coinvestigators from different disciplines and practices who would meet repeatedly to design and conduct the study and to interpret how its findings advanced an understanding of the research problem or question (Tranfield & Starkey, 1998). Through repeated meetings over extended periods of time, team members would come to know and respect each other by sharing different but complementary perspectives on problems and topics of common interest. In addition, they could push one another to appreciate issues in ways that were richer and more penetrating than before.

Underlying this suggestion is the proposition that research collaborations facilitate learning and enhance the likelihood of achieving the double hurdles of quality and relevance for scholars and practitioners (Hatchuel, 2001; Pettigrew, 2001). Anderson et al. (2001) and Hodgkinson et al. (2001) argue that user involvement in the research increases the impartiality of it by incorporating the diverse perspectives of multiple stakeholders, including management, trade unions, and consumers. Research collaborations that incorporate such diversity spur novelty and creativity through exposure to diverse assumptions, objectives, and ways of viewing phenomena (Rynes et al., 1999), as well as through the motivational effects of working on real-world problems (Lawler et al., 1985). It also promotes what Wilson (1999) calls "concilience," integrating fragmented perspectives and bits of knowledge into a larger (gestalt) appreciation of the question being addressed.

Collaboration that fosters arbitrage among researchers and practitioners can be designed into research teams as well as research review panels and advisory boards. This is a stated goal in a variety of university-industry research initiatives of the U.S. National Science Foundation and National Institutes of Health, as well the AIM initiative in the United Kingdom that is funded by two of the main research councils (ESRC/EPSRC).

One of us, for example, was engaged in one such university-industry research consortium, in which criteria for selecting proposals to be funded stipulated that members of each project team represent two or more university departments and at least one of the sponsoring companies. Teams of supported projects also agreed to make annual presentations of their progress and to adjust their work based on feedback from a review panel. The review panel for each project consisted of leading scholars in the project domain and practitioners from the com-

panies. The annual review consisted of a day-long site visit by the review panel. Typically, each project team made a single presentation in the morning to an audience comprising its review panel plus other interested members from the companies and the university community. In the afternoon the project team met with the review panel to discuss its feedback and suggestions. Following this meeting, the panel submitted a written report to the program's advisory board (consisting of university administration and company executives). Continued funding was contingent on a favorable response by the review panel and an overall evaluation of progress by the advisory board. The program led to a number of government-funded follow-up projects, as well as several projects that were funded by individual companies. Personal communications with investigators who participated in this program revealed that it was one of the most productive learning experiences of their professional lives.

Several concerns about studying real-world problems in research collaborations have been expressed. These include the difficulties of meeting conventional scientific requirements of internal and external validity (Cook & Campbell, 1979; Sackett & Mullen, 1993). Furthermore, practitioner involvement may compromise the independence and objectivity of the academic researcher (Beyer & Trice, 1982; Grey, 2001; Hackman, 1985), and participating organizations may view the research findings as proprietary and, thus, not available for dissemination in the public domain (Lawler et al., 1985). Moreover, although collaborative research has the potential to yield important contributions to theory and practice, it also exposes a research project to pragmatic organizational pressures and events that may compromise or sacrifice research goals and methods while the project unfolds (Rynes et al., 1999).

These concerns (and others) represent risks inherent to any collaborative research venture (Van de Ven & Poole, 2002). Some of them originate in the way projects are designed and negotiated at the outset. Researchers with unclear objectives or little experience in the arbitrage process may unwittingly find themselves trapped in such difficulties because they did not carefully negotiate the initial terms and understandings of the research project with all participants. Hatchuel (2001) emphasizes that research collaborations require clear objectives and careful negotiation of the identities and roles of participants, the rules of engagement and disengagement, and the dissemination and use of study findings. A collaborative research project represents a joint venture, to which many of the principles for negotiating and managing strategic alliances and interorganizational relationships apply (Galaskiewicz, 1985; McEvily, Perrone, & Zaheer, 2003; Ring & Van de Ven, 1994).

Amabile et al. (2001) note, however, that research projects are collaborations among individuals or teams of different professions (academic disciplines and business functions), not between organizations, and that collaborators are not all members of the same organization. These researchers examined if and how the success of such "cross-profession" collaboration is influenced by collaborative team, environment, and process characteristics. Based on a case study of their four-year T.E.A.M. Study (the Team Events and Motivation Study), they found that creating a successful collaborative research team is difficult, and they made five recommendations for designing an academic-practitioner research team: (1) carefully select academics and practitioners for diverse and complementary skills and backgrounds, intrinsic motivation in the problem being investigated, and a willingness to work with people of different cognitive styles and different professional cultures; (2) clarify commitments, roles, responsibilities, and expectations at the outset and continually update them as they evolve; (3) establish regular, facilitated communication, especially if team members are not located in the same place; (4) develop ways for academics and practitioners to get to know and trust each other as people with possible cultural differences; and (5) occasionally set aside time for the team to reflect on itself and to explicitly discuss task, process, and relationship conflict. These recommendations appear advisable for any heterogeneous working group (Hackman, 1991).

Design the Study for an Extended Duration

Time is critical for building relationships of trust, candor, and learning among researchers and practitioners (Mintzberg, 1979; Pettigrew, 2001). The importance of spending more time on site to build direct and personal relationships

with organizational participants has been argued not only to facilitate the implementation of research findings (Mintzberg, 1979; Lawler et al., 1985) but also to increase the likelihood of making significant advances in a scholarly discipline (Daft, 1984; Lawrence, 1992; Weick, 2001).

Empirical evidence for these claims is provided by Rynes et al. (1999), who examined 163 articles published in 4 leading management journals from 1993 to 1995 and conducted a questionnaire survey of their authors. They found that the hours spent by academic researchers at organizational sites were significantly related to the implementation of research findings. Their explanation for this finding was that increased "face time" increases affective trust of organizational members toward the researcher (e.g., Osborn & Hagedoorn, 1997; Saxton, 1997) and keeps the study salient in their minds. In addition, time spent on site is likely to bring the researcher closer to the phenomenon he or she is studying, as well as to increase his or her awareness of the ways in which organizational members are framing the topic or problem under investigation (Beyer, 1997). Both of these types of insight are likely to increase the chances that the research process will lead to eventual implementation by organizational practitioners (Rynes et al., 1999).

Moreover, Rynes et al. (1999) established a significant empirical relationship between research site time and scholarly contribution of the research. The factor that was most strongly associated with the impact of research (measured by paper citation rates) was the time spent by researchers at their research sites. One explanation is that it takes an extensive amount of direct and personal investigation to become acquainted with the dimensions and context of a phenomenon. Simon (1991), for example, argued that it takes ten years of dedicated work and attention to achieve world-class competence in a domain.

While we might quibble with the amount of time it takes to achieve competence, the point is that one-time cross-sectional organizational studies only provide a single snapshot of an issue being investigated. Cross-sectional studies seldom provide researchers sufficient time and trials to become knowledgeable in their research topic.[4] Longitudinal research promotes deeper learning because it provides repeated trials for approximating and understanding a research question or topic. Becoming "world class" is a path-dependent process of pursuing a coherent theme of research questions from project to project over an extended period of time.

A basic, but often overlooked, fact of most academic research is that researchers are exposed to only the information that people in research sites are willing to share. Interviews in cross-sectional studies or initial interviews in longitudinal studies with research sites tend to be formal and shallow. Greater candor and penetration into the subject matter seldom occur until a sufficient number of interactions over time have occurred for participants to come to know and trust one another. Perhaps the "one-minute manager" is an unfortunate social construction of the one-minute researcher.

One indication of comfort with a researcher is how practitioners treat the researcher. One of us conducted the fourth yearly interview with a participant in a longitudinal field study of organizational change. In greeting, the participant stated, "Normally I wear a coat and tie when outside visitors come. This morning I noticed that you were coming. So I decided not to wear a coat and tie."

Candid information comes not only with familiarity and trust but also with more knowledgeable and penetrating probes in responses to questions. A common self-assessment of field researchers is "If I only knew then what the study findings would be, I would have asked more probing questions." Repeated interviews and meetings with practitioners in longitudinal research provide important opportunities to penetrate more deeply into the subject matter being investigated.

Employ Multiple Models and Methods to Study the Problem

Multiple frames of reference are needed to understand complex reality. As Azevedo states, "Through the coordination of multiple perspec-

[4] We also think that too many management scholars dilute their competencies by conducting an eclectic and unrelated series of cross-sectional studies in their careers.

tives the robust features of reality can be distinguished from those features that are merely a function of the theoretical framework used. Scientific methodology is essentially pluralist" (1997: 191). Any given theory is an incomplete abstraction that cannot describe all aspects of a phenomenon. Theories are fallible constructions that model a partial aspect of reality from a particular point of view and with particular interests in mind. If we use only a single model or framework to investigate a problem, we may not be able to detect error, and our methodology may be misleading. Comparing and contrasting multiple models that reflect different perspectives is essential for discriminating among error, noise, and robust information about a complex problem being investigated. Examining plausible alternative models and methods is essential to a strategy of intellectual arbitrage.

The choice of models and methods varies with the particular context and purpose of each project. Triangulation of methods and models increases reliability and validity. It also maximizes the kind of learning that is at the heart of the arbitrage process. Presumably, each strategy reflects the unique hunches and interests of different members of the research team. Sharing approaches and findings enhances learning among coinvestigators. Each strategy represents a different thought trial to frame and map the subject matter. As Weick (1989) argues, undertaking multiple independent thought trials facilitates good theory building (see also Wilson, 1999).

The typical strategy used in most research projects is to empirically examine a single theory or explanation. This strategy does not entail arbitrage. One has a much greater likelihood of making important knowledge advances to theory and practice if the study is designed so that it juxtaposes and compares competing plausible explanations of the phenomenon being investigated (Kaplan, 1964; Poole, Van de Ven, Holmes, & Dooley, 2000; Singleton & Straits, 1999; Stinchcombe, 1968).

Stinchcombe (1968), for example, advises researchers to develop "crucial" propositions that "carve at the joints" (as Plato described) by juxtaposing or comparing competing answers. Examining plausible alternatives promotes a critical research attitude. It also leverages knowledge differences by examining the extent to which evidence for competing alternative models compares with status quo explanations. Knowledge of many topics has advanced beyond the customary practice of rejecting a null hypothesis when a statistical relationship is different from zero. Such a finding is a cheap triumph when previous research has already shown this to be the case. More significant knowledge is produced when tests of rival plausible hypotheses are undertaken. Such tests are likely to add significant value to theory as well as practice. Testing rival plausible hypotheses also provides the insurance of a win-win outcome for investigators—no matter what test results are obtained, the research, if properly executed, will make an important contribution.

Reexamine Assumptions About Scholarship and the Roles of Researchers

As the above ingredients imply, problem-driven research requires engaged scholars to be more aware and self-reflective of their roles. While scholars generally agree with the purpose of such research, they may disagree on the scientific status of practical intervention versus detached observation as methods of inquiry. Those advocating intervention argue that the only way to understand a social system is to change it through deliberate intervention and diagnosis of responses to the intervention (e.g., Argyris & Schon, 1996; Beer, 2001; Schein, 1987). Proponents of this interventionist model typically view the researcher as a consultant who uses methods of action science to solve a client's problems (Argyris, Putnam, & Smith, 1985). More traditional social scientists start with the assumption that a social system is there to be understood and left intact. Whereas action researchers tend to be visible and proactive change agents helping a client solve a problem, social scientists traditionally advocate a "hands-off" policy of minimal intervention by being the unseen "fly on the wall."

> The professional ethics of the [social scientist] . . . have much more to do with how to obtain valid information without influencing or disturbing the system being studied any more than is necessary. One should learn about the culture without changing it too much, so the investigator must make himself or herself as much a part of the scene as possible, and must not intervene in ways that would knowingly and deliberately perturb and change the system. Typically, the ultimate goal is to obtain valid data for "science," not

> to change, help, or in any other ways influence the system being studied (Schein, 1987: 22).

Whether researchers adopt action-interventionist or detached observer roles is influenced not only by their preferences and training but also by the nature of the question or problem being studied. Some issues and policies are better understood by observing them in their natural states, whereas others may require experimental intervention.

Although more traditional scholars and action researchers may have different goals, they can both employ methods of arbitrage in conducting their research. Schein (1987) notes that both have common commitments to scientific objectivity in the collection and analysis of data: to learn to observe, to develop relationships with clients and the people being studied, to listen attentively, to elicit information in conversations and interviews, and to use structured devices for gathering and analyzing data. Both academic and action researchers require intimate access to the people and organizations being investigated. More importance is placed on direct observations of concrete occurrences and events and less on reports or secondary information about the events. Investigators should witness firsthand what they propose to understand.

In the course of most longitudinal field studies, clinical and scholarly roles become highly intertwined. One implication is that the field researcher "must be able to function in both the clinical and scientific roles and furthermore, must be highly aware of when he or she is in which role so that neither relationship is fundamentally compromised" (Schein, 1987: 29). Another implication can be found in Whyte's reflections on his fifty-year career of fieldwork:

> As I gained experience in a wider range of research situations, I found myself gradually abandoning the idea that there must be a strict separation between scientific research and action projects. Through the rest of my career, I have been exploring how research can be integrated with action in ways that will advance science and enhance human progress at the same time (Whyte, 1984: 20).

Noting that scientists and clinicians often produce different kinds of knowledge, Schein comes to a similar conclusion regarding what we have discussed in terms of intellectual arbitrage. "We will never fully understand organizations until clinicians and [scientists] . . . begin to work together to pool their insights or until a generation of clinician/scholars is trained in both sets of roles and skills" (Schein, 1987: 44).

CONCLUSION

The reader might wonder if the ingredients for engaged scholarship imply that scholars should conduct more applied and less basic research. We do not believe that they do. Instead, engaged scholarship as we have defined it represents an arbitrage strategy for surpassing the dual hurdles of relevance and rigor in the conduct of fundamental research on complex problems in the world. By exploiting differences in the kinds of knowledge that scholars and practitioners from diverse backgrounds bring to bear on a problem, engaged scholarship produces knowledge that is more penetrating and insightful than knowledge produced when scholars or practitioners work on a problem alone. More specifically, we have argued that the quality as well as the impact of research improves substantially when researchers do four things: (1) confront questions and anomalies existing in reality, (2) organize the research project as a collaborative learning community of scholars and practitioners with diverse perspectives, (3) conduct research that systematically examines not only alternative models and theories but alternative practical formulations of the question of interest, and (4) frame the research and its findings to contribute knowledge to academic disciplines and to one or more domains of practice.

We think that these steps of engaged scholarship are appropriate for a wide variety of research studies. The reality in which a given problem originates may exist in either the practical world of affairs or in a theoretical discipline. In either case, we have argued that it is the intended research question about the problematic situation that spells out the boundary conditions for undertaking engaged scholarship. In particular, decisions about how to undertake the steps of an engaged scholarship project should be guided by the nature of the problem and whether the research question intends to describe, explain, predict, or control the problematic situation.

There are, of course, many management research studies that are undertaken outside the boundaries we have proposed for engaged

scholarship. They include research studies that are oriented either very generally toward exploring a phenomenon with no specific end in mind or very specifically toward examining a detailed question with a predetermined model or hypothesis in mind. In the former case, it may be premature to implement the steps of engaged scholarship, because it is not clear what research question might be addressed, who will be involved, what alternative models might be examined, and what contributions the exploration might make. Further exploration of the phenomenon may be necessary before having sufficient information to launch the steps of engaged scholarship. In the latter case, when the research question, model, users, and contribution have already been determined, it may be too late or inadvisable to undertake all the steps of the engaged scholarship process. Between these extreme cases, however, there is a wide range of research situations for which engaged scholarship is appropriate.

Engaged scholarship is fundamentally a pluralistic process undertaken to understand a complex phenomenon. Expanding on the concept of arbitrage, which we have argued is a basic principle underlying interdisciplinary research, engaged scholarship leverages the likelihood of creative understanding by combining the unique insights of scholars from different disciplines and practitioners with different functional experiences related to a given problem. As argued by Simon (1976), significant invention in the affairs of the world calls for two kinds of knowledge: (1) practical knowledge about issues and needs from the perspective of those involved and (2) scientific knowledge about new ideas and processes that are potential means for addressing these issues and needs.

Invention tends to be easy and the most likely to produce incremental contributions when it operates among like-minded individuals. Thus, Simon observes that we find applied researchers who tend to immerse themselves in the problems of the end users and then apply available knowledge and technology to provide solutions for their clients, and we find pure disciplinary scholars immersed in their disciplines to discover what questions have not been answered and then apply research techniques to address these questions. In either case, if the researchers cannot answer their initial questions, they modify and simplify them until they can be answered. As this process repeats itself, the questions and answers become increasingly specific contributions to narrow domains of problems and inquiry (Simon, 1976).

Tranfield and Starkey (1998) point out that researchers locate themselves in different communities of practice and scholarship at different times:

> But they cannot stay fixed in either the world of practice (without risking epistemic drift driven by politics and funding) or in the world of theory (without retreating to academic fundamentalism). The problems addressed by management research should grow out of the interaction between the world of practice and the world of theory, rather than out of either one alone (1998: 353).

In the conduct of engaged scholarship, researchers are equally exposed to the social systems of practice and science and are confronted with real-life questions that are at the forefront of the kind of knowledge and policies that are used to address problems in the world. This setting increases the chance of significant innovation. As Louis Pasteur stated, "Chance favors the prepared mind." Research in this context is also more demanding, because scholars do not have the option of substituting simpler questions if they cannot solve real-life problems.

Engaged scholarship is difficult because it entails a host of interpersonal tensions and cognitive strains that are associated with juxtaposing investigators with different views and approaches to a common problem. But focusing on the tensions between scholars and practitioners, as has often been the case in the past, may blind us to the very real opportunities that can be gained from exploiting their differences in the coproduction of knowledge. As Simon (1976) has observed, if research becomes more challenging when it is undertaken to answer questions posed from outside an academic discipline, it also acquires the potential to become more significant and fruitful.

The history of science and technology demonstrates that many extraordinary advancements often have been initiated by problems and questions posed from outside the scientific enterprise. Necessity is indeed the mother of important invention. Scholarship that engages both researchers and practitioners can provide an exceedingly productive and challenging environment; it not only fosters the creation of the kind of knowledge that solves practical prob-

lems but also makes irrelevant the argument for a gap between theory and practice in the arenas of professional and public life.

> Any scientist of any age who wants to make important discoveries must study important problems. Dull or piffling problems yield dull or piffling answers. It is not enough that a problem should be interesting—almost any problem is interesting if it is studied in sufficient depth . . . the problem must be such that it matters what the answer is—whether to science generally or to mankind (Medawar, 1979: 13).

Finally, some readers have questioned the scientific contribution of engaged scholarship since its principles have been documented in both the applied and action research literature. Instead of viewing engaged scholarship as some sort of applied or action research (for which it can be used), we think of engaged scholarship as a mode of inquiry that translates into management research the evolutionary critical realist perspective of modern science. McKelvey (1997, 2002), Azevedo (2002), and Moldoveanu and Baum (2002) provide detailed and instructive translations of this epistemology for organization science.

Consistent with an evolutionary realist philosophy of science, engaged scholarship adopts a pluralistic methodology that advances knowledge by leveraging the relative contributions and conceptual frameworks that researchers and practitioners bring to bear on a given problem or question. Through arbitrage, the robust features of alternative models and perspectives can be distinguished from those features that are merely a function of a single model or framework. A research finding is robust when it appears invariant (or in common) across at least two (and preferably more) independent theories. A pluralistic approach of comparing multiple models of reality is therefore essential for developing valid knowledge. The models that better fit the problems they were intended to solve are selected by users, and the gradual winnowing down of plausible rival models or hypotheses by the scholarly community produces an evolutionary conception of the growth of scientific knowledge.

We believe that the concepts of arbitrage, dialectical inquiry, and constructive conflict management among researchers and practitioners are both central and novel to our formulation of engaged scholarship. We recognize that some of these concepts have already been documented in the literature. This is inevitable in light of what Alexander Gray once said: "No point of view, once expressed, ever seems wholly to die. . . . Our ears are full of the whisperings of dead men [and women]" (quoted in Filley, House, & Kerr, 1976: 3). Hopefully, we have surfaced and recombined these concepts in a useful way, and thereby sketched a new vision of how research might be conducted to make fundamental advances to the process for creating knowledge for theory and practice.

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