TWENTY-ONE

Reflections on Research for Theory and Practice

From an Engaged Scholarship Perspective

ANDREW H. VAN DE VEN

THIS BOOK FOCUSES on the relationships between theory and practice, research and action, and basic and applied knowledge. A core assumption is that research knowledge should be useful for advancing science and practice. As Mohrman and Lawler (Introduction and Overview) state, the central question in this book is how should such research be designed, carried out, and disseminated to achieve the twin goals of advancing both theory and practice?

This book addresses this question in a variety of ways. Amy C. Edmondson (Chapter 2, Crossing Boundaries to Investigate Problems in the Field), Susan Albers Mohrman and Allan Mohrman (Chapter 3, Collaborative Organization Design Research at the Center for Effective Organizations), and Lynda Gratton (Chapter 4, A Ten-Year Journey of Cooperation) provide three exemplars of practicing scholars who are carrying out research that has the dual purpose of generating academic knowledge and enabling more effective practice. They describe the choices they make and the tactics they employ. They are followed by Philip Mirvis and Edward E. Lawler (Chapter 6, Rigor and Relevance in Organizational Research), C. K. Prahalad (Chapter 7, Can Relevance and Rigor Coexist?), Michael Beer (Chapter 8, Making a Difference *and* Contributing Useful Knowledge), and Michael L. Tushman (Chapter 9, On Knowing and Doing), five scholars whose research careers have clearly influenced both theory and practice.

In addition, Ruth Wageman (Chapter 10, Academic-Consultant Collaboration), Ram V. Tenkasi (Chapter 11, Integrating Theory to Inform Practice), Jean M. Bartunek and Edgar H. Schein (Chapter 12, Organization Development Scholar-Practitioners), Wayne F. Cascio (Chapter 13, Professional Associations), Denise M. Rousseau and John W. Boudreau (Chapter 14, Sticky Findings), and George S. Benson (Chapter 15, Popular and Influential Management Books) examine various pathways through which academic knowledge reaches practice, including professional associations, evidence-based

management resources, books, tools, and professions such as consultants and organizational development. A key institution that shapes the field—MBA programs—is also examined. These chapters plus section commentaries (Chapter 5, Walking on Three Legs; Chapter 16, Observations Concerning Pathways for Doing "Useful Research") contribute a gold mine of practical experience-based wisdom for doing research that is useful for theory and practice.

I was asked to reflect on these contributions from the perspective of engaged scholarship. To do this, I will summarize how our model of engaged scholarship addresses the question of how to do research that is useful to theory and practice. In doing so, I will indicate how chapter authors complement and extend our view of engaged scholarship. These contributions focus not only on research methods but also on building the capabilities of scholars and practitioners to co-produce and use research knowledge for theory and practice.

Engaged Scholarship

We (Van de Ven & Johnson, 2006; Van de Ven, 2007) proposed a method of *engaged scholarship* for studying complex social problems that often exceed our limited individual capabilities to study on our own. *Engaged Scholarship* is a participative form of research for obtaining the advice and perspectives of key stakeholders (researchers, users, clients, sponsors, and practitioners) to understand a complex problem or phenomenon. By exploiting differences in the kinds of knowledge that scholars and other stakeholders can bring forth on a problem, we argue that engaged scholarship produces knowledge that is more penetrating and insightful than when scholars or practitioners work on the problems alone.

Using a diamond model, as illustrated in Figure 21.1, I argue (in Van de Ven, 2007) that researchers can significantly increase the likelihood of producing knowledge that advances theory and practice by engaging others whose perspectives are relevant in each of four study activities:

- *Problem formulation.* Situate, ground, and diagnose the research problem by determining who, what, where, when, why, and how the problem exists up close and from afar. Answering these journalist questions requires meeting and talking with people who experience and know the problem, as well as reviewing the literature on the prevalence and boundary conditions of the problem.
- *Theory building.* Develop plausible alternative theories (or propositions) that address the problem as it exists in its particular context. Developing these alternative theories requires conversations with knowledge experts

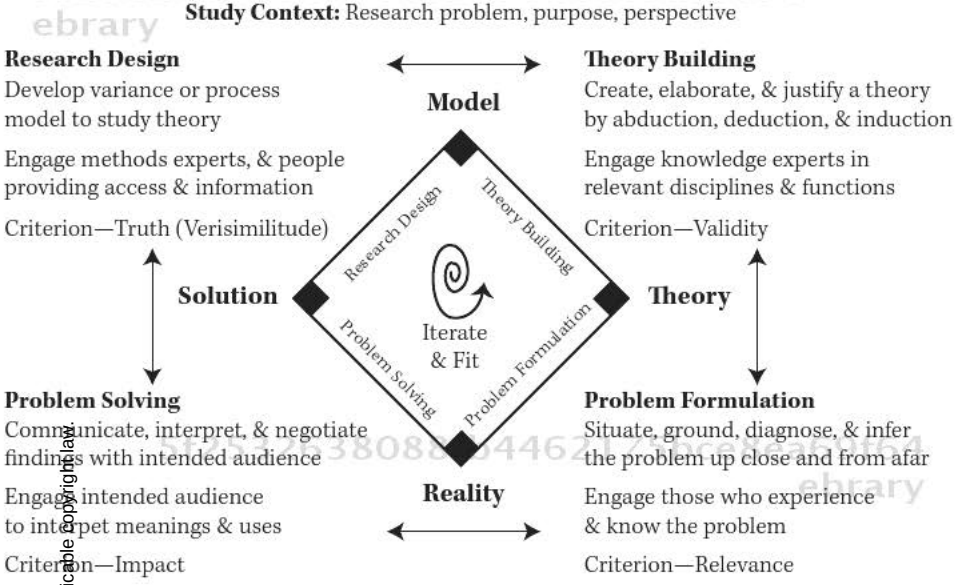

FIGURE 21.1 ENGAGED SCHOLARSHIP DIAMOND MODEL

from the relevant disciplines and functions that have addressed the problem, as well as a review of relevant literature.

- *Research design.* Gather empirical evidence to compare the plausible alternative models that address the research problem. Doing this well typically requires getting advice from technical experts in research methodology, the people who can provide access to data, and of course, the respondents or informants of information.
- *Problem solving.* Communicate, interpret, and apply the empirical findings on which models better answer the research question about the problem. The greater the difference in content-specific knowledge between researchers and stakeholders, the more they need to communicate in order to understand and use the research findings. Communications might begin with written reports and presentations for knowledge transfer, then go on to conversations to interpret different meanings of the report, and then end with pragmatic and political negotiations to reconcile conflicting interests.

Engaged scholarship can be practiced in many different ways, including the four approaches outlined in Figure 21.2. These different approaches depend on (1) whether the purpose of a research study is to examine basic questions of description, explanation, and prediction or applied questions of design, evaluation, or action intervention, and (2) the degree to which a researcher

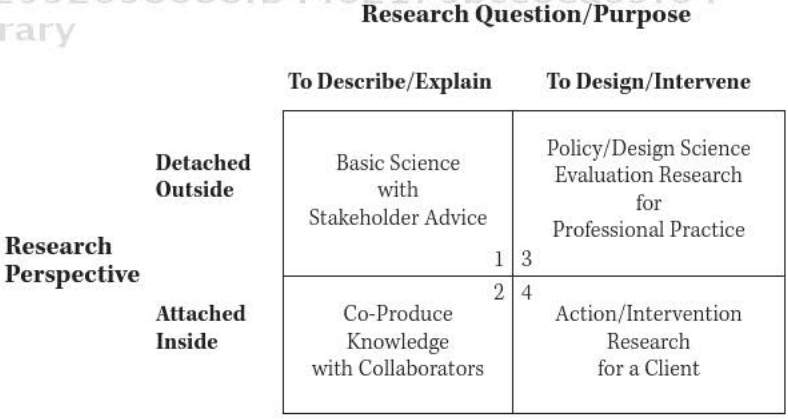

FIGURE 21.2 Alternative Forms of Engaged Scholarship

examines the problem domain as an external observer or an internal participant.

1. *Informed basic research* is undertaken to describe, explain, or predict a social phenomenon. It resembles a traditional form of basic social science where the researcher is a detached outsider of the social system being examined, but it also solicits advice and feedback from key stakeholders and inside informants on each of the research activities (as listed in Fig. 21.1). These inside informants and stakeholders play an advisory role, and the researcher directs and controls all research activities.
2. *Collaborative basic research* entails a greater sharing of power and activities among researchers and stakeholders than informed research. Collaborative research teams are often composed of insiders and outsiders who jointly share the activities (as listed in Fig. 21.1) in order to co-produce basic knowledge about a complex problem or phenomenon. The division of labor is typically negotiated to take advantage of the complementary skills of different research team members, and the balance of power or responsibility shifts back and forth as the tasks demand. Because this collaborative form of research tends to focus on basic questions of mutual interest to the partners, it has much less of an applied orientation than the next two forms of engaged scholarship.
3. *Design and evaluation research* is undertaken to examine normative questions dealing with the design and evaluation of policies, programs, or models for solving practical problems. Variously called "design or policy science" or "evaluation research," this form of research goes beyond describing or explaining a social problem but also seeks to obtain evidence-based knowledge of the efficacy or relative success of alternative solutions

to applied problems. Evaluation researchers typically take a distanced and outside perspective of the designs or policies being evaluated. Inquiry from the outside is necessary because evidence-based evaluations require comparisons of numerous cases, and because distance from any one case is required for evaluation findings to be viewed as impartial and legitimate. But engagement of stakeholders is important so they have opportunities to influence and consent to those evaluation study decisions that may affect them. These decisions include the purposes of the evaluation study (problem formulation), the criteria and models used to evaluate the program in question (research design), and how study findings will be analyzed, interpreted, and used (problem solving).

4. *Action/intervention research* takes a clinical intervention approach to diagnose and treat a problem of a specific client. Kurt Lewin, a pioneer of action research, suggested a learning strategy of both engaging with and intervening in the client's social setting. The foundation of this learning process was client participation in problem solving using systematic methods of data collection, feedback, reflection, and action. Since Lewin's time, action research has evolved into a diverse family of clinical research strategies in many professional fields. Action research projects tend to begin by diagnosing the particular problem or needs of an individual client. To the extent possible, a researcher utilizes whatever knowledge is available from basic or design science to understand the client's problem. However, this knowledge may require substantial adaptation to fit the ill-structured or context-specific nature of the client's problem. Action research projects often consist of *N*-of-1 studies, where systematic comparative evidence can only be gained through trial-and-error experiments over time. In this situation, action researchers have argued that the only way to understand a social system is to change it through deliberate intervention and diagnosis of responses to the intervention. This interventionist approach typically requires intensive interaction, training, and consulting by the researcher with people in the client's setting.

Sometimes advocates of a particular research approach make disparaging remarks about other forms. This is unfortunate, because all four forms of engaged scholarship are legitimate, important, and necessary for addressing different research questions posed by science and practice (description, explanation, design, or control of a problematic situation). Which approach is most appropriate depends on the research question and the perspective taken to examine the question. Pragmatically, the effectiveness of a research approach should be judged by how well it addresses the research question for which it was intended (Dewey, 1938).

Although the four forms of engaged scholarship entail different kinds of relationships between the researcher and stakeholders in a study, engagement is the common denominator. The more ambiguous and complex the problem, the greater the need for engaging others who can provide different perspectives for revealing critical dimensions of the nature, context, and implications of the problem being studied.

Elaborating and Extending the Model

This book elaborates and extends these goals and methods of engaged scholarship in several important ways. They include insights on doing problem-driven, boundary-crossing fieldwork, spending time in field research sites to gain penetrating knowledge of a topic being investigated, appreciating the challenges and opportunities of academic-practitioner research collaborations, examining big questions that ignite academics and practitioners to pursue their alternative models on the questions, communicating and using research findings with intended audiences, becoming engaged scholars, and developing a personal identity and empathy with the stakeholders of a study. The rest of this chapter discusses each of these insights.

Problem-Driven, Boundary-Crossing Fieldwork

Edmondson's discussion of her approach in Chapter 2 (Crossing Boundaries to Investigate Problems in the Field) of starting with problems, going into the field, and reaching across boundaries is an important elaboration of engaged scholarship.

She suggests starting with problems. "Problems provide a natural connection with practice. Studying a compelling problem, researchers are motivated to care about action. Problems matter!"

She advises researchers to go into the field. "Unless you have an unusual office location, sitting at your desk is unlikely to be the most conducive situation for gaining insight into the kinds of organizational phenomena described previously. Although one can learn about an industry or company from written materials, fuller understanding and new ideas are more likely when meeting and observing people who work in that setting."

She also advises collaboration across knowledge boundaries. "In most field research sites, it takes time to understand both the organization and the industry. Collaborators who bring different perspectives and expertise can

accelerate the learning needed to get up to speed and offer novel insights. For me to understand drug errors in hospitals, for example, would have been extremely difficult without working closely with the physicians and nurses in the larger research project."

Edmondson (Chapter 2, Crossing Boundaries to Investigate Problems in the Field) also makes good suggestions on using the literature for each step in the diamond model in Figure 21.1: problem formulation, theory building, research design, and problem solving.

> The literature—that is, prior research that informs and shapes the research question—plays a crucial role in helping to keep these elements working together for research purposes. The literature is an integrating force, helping to shape research to make its best contribution. Familiarity with what has come before in a given field that relates to your research question makes sure prior findings are integrated, elaborated, or refuted in the current work. More specifically, with respect to understanding a problem, finding out what others have done to understand that problem lowers the risk of reinventing the wheel.... The literature is crucial for shaping methodological and scope decisions, and hybrid approaches allow researchers to test associations between variables with quantitative data and to explain and illuminate novel constructs and relationships with qualitative data (Edmondson & McManus, 2007). When collaborating across academic disciplines, it is important to identify the literature one hopes to advance. The insights from different fields are likely to enrich one's thinking, as noted previously, but then it must be tamed and focused. The literature provides a force to focus and sharpen what one has found, before communicating it to others. This helps to avoid the trap of the overly broad or superficial observation in favor of offering precise statements on the advance that has been made.

Spending Time in Field Research Sites

Edmonson (Chapter 2, Crossing Boundaries to Investigate Problems in the Field) cautions young scholars that her approach slows you down and may even harm your career. Like Sara L. Rynes (Chapter 19, Counterpoint), I take a more optimistic view. Edmondson's approach to problem/phenomena-driven and boundary-crossing field research is well worth the time for it generates more profound and penetrating knowledge that leads to faster career advances than the status quo alternative of the researcher going it alone analyzing secondary data files in his/her office.

Time is critical for building relationships of trust, candor, and learning among researchers and practitioners. Sara L. Rynes (Chapter 19, Counterpoint)

discussed the importance of spending more time on site to build direct and personal relationships with organizational participants to facilitate implementation of research findings and to increase the likelihood of making significant advances to a scholarly discipline.

One explanation for these findings 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 over 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 about their research topic.[1] 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.

Candid information comes not only with familiarity and trust, but also with more knowledgeable and penetrating probes in responses to questions. Mohrman and Mohrman (Chapter 3, Collaborative Organization Design Research at the Center for Effective Organizations) state that to examine nanoscale phenomena, it takes two years or more for different disciplines to learn enough about one another's frameworks to be able to combine knowledge. Repeated interviews and meetings with practitioners in longitudinal research provide important opportunities to penetrate more deeply into the subject matter being investigated.

Of course, a series of related cross-sectional studies on the same problem or phenomenon can achieve penetrating insights as well. Mohrman and Lawler (Introduction and Overview) note that through a series of related studies, researchers can collaborate with a variety of organizations and researchers from other disciplines, apply multiple theories, and hone in on the

1. I also think that too many scholars dilute their competencies by conducting an eclectic and unrelated series of cross-sectional studies in their careers.

best understanding of the complex problems and phenomena in question. The QWL (quality of work life) studies in Michigan operated in this manner, drawing on the knowledge of dynamic teams of interventionists and researchers conducting related studies in multiple organizations, each of which wanted to put in place more effective work systems.

Academic-Practitioner Research Collaboration

When chapter authors described their joint academic-practitioner research projects, most seemed to reflect the second (collaborative co-producing knowledge) and fourth (action/intervention research) forms of engaged scholarship (as shown in Fig. 21.2). These two forms of engaged scholarship tend to present greater challenges than the first (basic science with stakeholder advice) and third (evaluation research). Because of this, *I tend to advise junior scholars to undertake and learn the first and third forms of engaged scholarship before launching into collaborative academic-practitioner projects. Moreover, don't launch into these studies alone; engage and rely on senior and experienced colleagues.* In addition to all the technical research skills that Edmondson (Chapter 2, Crossing Boundaries to Investigate Problems in the Field) describes, chapters by Gratton (Chapter 4, A Ten-Year Journey of Cooperation), Wageman (Chapter 10, Academic-Consultant Collaboration), and Bartunek and Schein (Chapter 12, Organization Development Scholar-Practitioners) point out that collaborative and action research projects include the challenges of finding mutual interests, boundary spanning, power sharing, and task coordination between academics and practitioners.

Gratton (Chapter 4, A Ten-Year Journey of Cooperation) discusses a number of important prerequisites for cooperation. First, however motivated individuals may be to cooperate, the actual act of cooperation is more likely to occur when they engage in a task, a big question or a vision that excites and motivates them and forms what we called the "ignition." Second, she observes that while this point of ignition can potentially pull people together, the opportunity for them to be creative also depends on the extent and breadth of boundary spanning across networks. Third, the ability to actively exchange ideas and contacts within these ignited networks depends on the habits and attitudes of individuals to cooperation, which she calls a "cooperative mindset." Finally, Gratton refers to a series of "productive practices" that enable team ideas and creativity to translate into performance and innovation. These practices require competencies in conflict resolution, commitment making, and creating opportunities for reflection.

Wageman (Chapter 10, Academic-Consultant Collaboration) provides useful advice on managing academic-practitioner research teams, which

appears to apply to most boundary-crossing work teams. Based on her and Richard Hackman's academic-consultant research team experience with the Hay Group, she discusses two key process losses, "(1) failure of participants to do their 'homework' between meetings, and (2) conflicting time pressures—leading to "short, intensive bursts of focus by the team, between long periods of little or no progress, and little preparation in advance of the collaborative work" (Wageman, Chapter 10, Academic-Consultant Collaboration). Based on these experiences, she provides a number of practical suggestions for enabling collaboration and overcoming its obstacles.

The Need for Alternative Models to Examine Big Questions

Given all of the challenges and efforts discussed by chapter authors to make academic-practitioner research collaborations work, we should ask, Why do it? What kind of research problems require academic-practitioner engagement? Several contributors (Gratton, Chapter 4, A Ten-Year Journey of Cooperation; Tushman, Chapter 9, On Knowing and Doing; Edmondson, Chapter 7, Crossing Boundaries to Investigate Problems in the Field; and Beer, Chapter 8, Making a Difference *and* Contributing Useful Knowledge) argue that *the more complex the problem or the bigger the research question, the greater the need to engage researchers from different disciplines and practitioners with different functional experiences.* Engagement of others is necessary because most real-world problems are too complex to be captured by any one investigator or perspective. As Beer suggests, there are many big questions and problems that are very difficult to study in a productive way without engagement and close interaction among scholars and practitioners.

Big questions have no easy answers, and they seldom provide an immediate payoff 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 each other to co-produce solutions whose demands exceed the capabilities of either researchers or practitioners. Thus, at the time of designing a research project, prospective solutions to research questions are secondary in comparison with the importance of the research question that is being addressed. As Gratton suggests, *a good indicator of a big question is its self-evident capability to ignite the attention and enthusiasm of scholars and practitioners alike* (Chapter 4, A Ten-Year Journey of Cooperation).

Study of big questions requires developing and comparing plausible alternative theories or models. Multiple models and frames of reference are needed to understand complex reality. Any given theory is an incomplete abstraction that cannot describe all aspects of a phenomenon. Theories are fallible

human constructions that model a partial aspect of reality from a particular point of view with particular interests in mind. Comparing and contrasting plausible alternative models that reflect different perspectives are essential for discriminating between credible, erroneous, and noisy information. The choice of models and methods varies, of course, with the particular problem and purpose of a study. *The more complex the problem or question, the greater the need to map this complexity by employing multiple and divergent models.* Triangulation of methods and models increases reliability and validity. It also maximizes learning among members of an engaged scholarship team. Presumably different models reflect the unique hunches and interests of different participants in the research project. Sharing approaches and findings enhance learning among co-investigators. 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.

Communicating and Using Research Findings

Susan Albers Mohrman and Allan Mohrman (Chapter 3, Collaborative Organization Design Research at the Center for Effective Organizations) discuss Mohrman, Gibson, and Mohrman's (2001) study that provided empirical evidence of useful knowledge generated with collaborative research. Practitioners reported research as being more helpful the more they incorporated the findings into their internal sense making and organizational design processes and made changes to their organization taking the findings into account. Usefulness also related to whether the company participants experienced the researchers as having incorporated the perspective of practice into the research and whether time had been spent mutually interpreting the data patterns. Interestingly, usefulness was not related to practitioner involvement in the design and conduct of the research. This suggests that the usefulness of research knowledge depends on bridging the meaning gap between the communities of theory and practice—communities with very different contexts, beliefs, and practices. The study made clear, however, that the onus is not only on the academic researcher. Usefulness depends on practitioners and users to engage in the hard and intentional work of applying the knowledge to make changes in regards to how they function.

Rousseau and Boudreau (Chapter 14, Sticky Findings) discuss a pervasive problem of evidence-based knowledge—it is not "sticky" and hence seldom used as a basis for management practice. They discuss a number of insightful communication principles for building a practitioner's capacity to use sticky evidence-based management knowledge. They cite many studies providing evidence for these principles. If that is not convincing, Everett Rogers (2003,

p. xviii) estimates no less than 5,200 studies have examined these principles on the adoption and diffusion of innovations. He states, "No other field of behavior science research represents more effort by more scholars in more disciplines in more nations" (Rogers, 2003, p. xviii).

Given the extensive research evidence supporting the principles for communicating and implementing research findings that Rousseau and Boudreau summarize, why do so few academics and practitioners follow this evidence-based advice? I suspect that proponents of evidence-based knowledge have overlooked the role of rhetoric in communicating and implementing research findings. *Research reports are more likely to be adopted by a specific audience when they are presented in a rhetorically persuasive way.* What makes information convincing and, therefore, utilized is a rhetorical question. 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 to elicit not only sympathy, but empathy as well, 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 are the elements of the rhetorical triangle. Combined, they shape the persuasiveness of any communication.

Mohrman's study findings indicated that persuasiveness 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 assumptions of the reader. In a nutshell, a classic work speaks to the primary concerns or assumptions of an audience, while 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. The key rhetorical message is that 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. Hence, from a rhetorical perspective, a researcher can increase the likelihood of influencing his or her intended audience by going beyond logos (the logical, technical research findings) to include pathos (persuasiveness and incentives) and ethos (the ethical and appropriateness) of the findings in research presentations, discussions, and reports.

Becoming an Engaged Scholar

There does not appear to be a turnkey method for learning how to do research that is useful for theory and practice. Philip Mirvis and Edward E. Lawler (Chapter 6, Rigor and Relevance in Organizational Research), C. K. Prahalad (Chapter 7, Can Relevance and Rigor Coexist?), Michael Beer (Chapter 8, Making a Difference *and* Contributing Useful Knowledge), and Michael L. Tushman (Chapter 9, On Knowing and Doing), whose work has significantly influenced theory and practice, each received their formal graduate training at schools with a fairly standard curriculum. Beyond formal training, each of their research careers and experiences followed different progressions. *This suggests that there is no one best way; instead there are many different ways to learn how to perform research that is useful for theory and practice.*

Mirvis and Lawler (in Chapter 6, Rigor and Relevance in Organizational Research) reflect on their experiences as matching the person and research (P-R fit) through socialization and career experiences over time. Lawler seems to have begun his career doing basic science (cell 1 in Fig. 21.2) and then over time expanded his repertoire by engaging in the other kinds of engaged scholarship (as shown in Fig. 21.2). Specifically, they state:

> Lawler's journey matches that of many other senior organizational behavior scholars who "end up" doing useful research. Lawler was socialized early on as a mainstream scientific researcher: exploring core psychological phenomena with traditional methods and tools. His early career experiences at Yale opened him up to other methodological perspectives and the exigencies of running a research shop at Michigan and later USC pulled him to more action-oriented scholarship and research methods.
>
> Later career writings turned from *JAP* to *HBR* and from books aimed at scholars and students to those aimed at practicing managers. As for person-environment fit, changes in his scholarly identity seemed to progress from Yale to Michigan to USC. As for his own motives for turning toward useful research, Lawler, when pressed, explained his outlook, "I get a kick out of doing work that makes people more effective and satisfied. It is rewarding to me. I'd rather do that than solve a crossword puzzle or deal with some technical issue."

Mike Beer, on the other hand, started his career as a practitioner doing a form of action research (cell 4 in Fig. 21.2), then learned basic science in his PhD program, and since has done all other forms of engaged scholarship. In Chapter 8 (Making a Difference *and* Contributing Useful Knowledge), he states:

> I have used my lifelong experience as a scholar-consultant and field researcher to develop principles for those who wish to develop knowledge

useful for theory and practice. I hope this narrative will be helpful to those aspiring to fashion such a professional identification. The work of developing useful knowledge is challenging. As I have tried to show, it requires passion for both making a difference *and* developing theory. It requires readiness to be drawn into ill-defined practical problems that one does not fully know how to approach or solve, to be comfortable with or foolish enough to live with uncertainty. And one has to be able to live in two worlds with different norms and rules for knowing.

Prahalad (Chapter 7, Can Relevance and Rigor Coexist?) and Tushman (Chapter 9, On Knowing and Doing) have also engaged in all four types of research (as shown in Fig. 21.2). However, my sense is that they have stayed closer to engaging in basic science, whereas Lawler and Beer appear to be more involved in applied research. My intentions in saying this are to show that *there are many ways and perspectives for doing research that is useful for theory and practice*. Prahalad has been an intense observer of diverse practices, which enables him to develop inventive insights on next practices. He states, "It is the understanding of the outliers that allows us to construct a mid-level theory,' a map that allows managers to understand the emerging competitive landscape and navigate it. We go back to the phenomena to illustrate the concepts. This process is very different from starting with a theory and then looking at the phenomenon." Prahalad's career has been one of crossing boundaries; this increases the likelihood of recognizing or creating organization and management inventions that provide a sustainable basis for contributing to and leading theory and practice.

In Chapter 9 (On Knowing and Doing), Tushman discusses how customized executive education programs are an underleveraged vehicle in reducing the rigor-relevance gap between business schools and the world of practice. His experience shows that teaching and research are inextricably linked and that executive education provides an ideal setting for engaging in use-inspired basic research (Pasteur's Quadrant in Stokes's Quadrant Model), which he argues is what business school research should emphasize. Tushman's suggestion that business school scholars focus on Pasteur's Quadrant is a nice expansion on Herbert Simon's proposal for the design of a business school (in the appendix of his 1976 version of *Administrative Behavior*). It provides a way forward to Cummings's insightful analysis of how and why business schools are in their problematic state today.

Simon (1976) proposed that a basic challenge for scholars in professional schools is to contribute to both the science and the practice of management—not either-or. He noted that the information and skills relevant to accomplishing this objective come from the social system of practitioners and the social system of scientists in the relevant disciplines. These social systems

have elaborate institutions and procedures for storing, transmitting, developing, and applying knowledge. As the chapters by Cascio (Chapter 13, Professional Associations) and Benson (Chapter 15, Popular and Influential Management Books) demonstrate, academics and practitioners live in different communities, and the main way to understand each community is to participate in it.

Bartunek and Schein (Chapter 12, Organization Development Scholar-Practitioners) discuss some of the challenges of scholars becoming conversant in these different communities of science and practice. Practitioner-scholar doctoral programs have the purpose of creating such professionals who bridge management theory and practice. Tenkasi (Chapter 11, Integrating Theory to Inform Practice) discusses such a program at Benedictine University in Chicago. Such practitioner-scholar PhD programs also exist at Case Western Reserve, the University of Southern California, and the DBA program at Harvard, and it would be instructive to compare their curricula. Tenkasi uses Aristotle's "phronesis" to characterize the kind of combinatorial knowledge of theory and practice to be learned in practitioner-scholar doctoral programs. Levi-Strauss's (1966) ideas about the science of the concrete and science of the abstract could also provide challenging ways to bridge the two kinds of knowledge.

In Chapter 9 (On Knowing and Doing), Tushman, like Simon (1976), argues that significant invention in the affairs of the world calls on two kinds of knowledge: applied knowledge about practical issues or needs of a profession and scientific knowledge about new ideas and processes that are potentially possible. Invention is easy and likely to produce incremental contributions when it operates among like-minded individuals. Thus 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 to their clients, and we find pure scientists 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 researchers cannot answer their initial questions, they modify and simplify them until they can be answered. As this process repeats itself, the research questions and answers become increasingly specific contributions to narrow domains of problems and inquiry.

Research in regards to Pasteur's Quadrant requires researchers to be equally exposed to the social systems of practice and science. At this boundary, they are likely to be confronted with real-life questions that are at the forefront of the kind of knowledge and policies that—as Prahalad suggests—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 single problem. But focusing on the tensions between scientists and practitioners is a mistake, for it may blind us to the very real opportunities that can be gained from exploiting their differences in the co-production of knowledge. As Tushman (Chapter 9, On Knowing and Doing), and Simon (1976) suggest, if research becomes more challenging when it is undertaken to answer questions posed from outside science, it also acquires the potential to become more significant and fruitful.

The reflective chapters of distinguished career scholars such as Lawler, Prahalad, Beer, Tushman, Schein, and Bartunek attest to this. So also do the biographical stories of 23 "Great Minds" of Management about their theory and research development journeys (Smith & Hitt, 2005). Indeed, doing engaged scholarly research that crosses the boundaries and is useful to theory and practice is a common theme in the stories of successful and highly respected scholars. The history of science and technology demonstrates that many of the extraordinary developments in pure science have often been posed by problems and questions from outside the scientific enterprise. Necessity is indeed the mother of important invention. Scholarship that engages both scientists and practitioners can provide an exceedingly productive and challenging environment; it not only fosters the creation of the kind of knowledge that solves practical problems but also makes irrelevant the argument for a gap between theory and practice in the arenas of professional and public life.

Researcher's Identity and Empathy

In conclusion, I want to emphasize that *producing research that is useful for theory and practice is not a solitary exercise; instead, it is a collective achievement*. Engagement means that scholars step outside themselves to obtain and be informed by the interpretations of others in performing each step of the research process: problem formulation, theory building, research design, and problem solving. For example, Mohrman and Lawler (Introduction and Overview) state that "combining theoretical knowledge from different disciplines with knowledge from practice . . . requires familiarity with each other's knowledge and building conceptual bridges such as prototypes or other boundary objects to link the knowledge bases" of theorists and practitioners.

For this to happen, Mirvis and Lawler (Chapter 6, Rigor and Relevance in Organizational Research) discuss a researcher's empathy for closing the theory-practice gap. "Close contact with practitioners in field studies, and

their active engagement in the research process and interpretation, yields a sense of affinity and empathy across the research-practice divide. This more interactive and participatory process thrusts the scholar into the organizational system that, in traditional scientific terms, risks objectivity as the researcher 'goes native.' At the same time, it gives us a deeper and richer feel for the subject matter and people under study. In the process, the "self" becomes a research instrument that can stimulate insights into what is going on and provoke more grounded theorizing."

As Mirvis and Lawler imply, *engaged scholarship entails a fundamental shift in how researchers define their relationships with the communities in which they are located, including faculty and students from various disciplines in the university and practitioners in relevant professional domains.* Edward Zlotkowski, a leading proponent of engaged scholarship in American higher education, captures the identity and empathy of an engaged scholar.

> It's about faculty members having a profound respect for those other than themselves, whether they be practitioners or students.... There is a profound emphasis on the concept of deep respect and, I might even say, humility vis-à-vis other kinds of knowledge producers. Not because we don't have an important and distinctive role to play in knowledge production, but because we don't have the exclusive right to such production. As we begin to engage in partnerships with both our students and outside communities of practice on the basis of such deep respect, we allow ourselves to become real-world problem solvers in a way that is otherwise not possible. Indeed, I would suggest that unless we learn to develop deeper respect for our non-faculty colleagues, we run the risk of becoming "academic ventriloquists"— speaking for our students, speaking for the communities we allegedly serve—but not really listening to them or making them our peers in addressing the vital issues that concern all of us. (Edward Zlotkowski, in Kenworthy-U'ren, 2005, p. 360)

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 and clients as data collection sites and funding sources, an engaged scholar views them as a learning workplace (idea factory) where practitioners and scholars co-produce 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, p. 15).

Conclusion

This chapter summarized our perspective on engaged scholarship and discussed how chapters in this book elaborate and extend this perspective in many important ways. The chapter also reviewed various ways to practice engaged scholarship, including basic, collaborative, evaluation, and action research as illustrated in Figure 21.2, and how to use the approach that best addresses the research question. Most doctoral students and junior faculty begin their research career addressing basic questions devoted to describing, explaining, or predicting various phenomena. I encouraged young researchers to do this basic science with stakeholder advice because such engagement produces more significant advances to knowledge than when researchers do their studies alone. Moreover, I suggested to *not* do it alone; engage and rely on senior and experienced colleagues for mentoring, networking, and accessing potential research sites and stakeholders. Engaged scholarship is a collective and developmental achievement. As researchers learn the technical and social skills of engaging stakeholders in basic science, then they can begin to address additional challenges of finding mutual interests, boundary spanning, power sharing, and task coordination between academics and practitioners in the other forms of engaged scholarship.

In the final analysis, the "proof is in the pudding." As many chapter authors have suggested, I believe that researchers who adopt the engaged scholarship model will produce research findings that make more significant advancements to theory and practice than the traditional approach of going it alone. As a result, research reports based on engaged scholarship should win out in competitive reviews for research funding, publications in journals, presentations at professional conferences, and professional training and development programs over those based on unengaged or disengaged research. The cumulative record should result in career advancements and promotions for engaged scholars at disproportionately higher rates than disengaged scholars who go it alone in conducting their research. Time will tell.

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ABOUT THE AUTHOR

Andrew H. Van de Ven is Vernon H. Heath Professor of Organizational Innovation and Change in the Carlson School of the University of Minnesota. He received his PhD from the University of Wisconsin at Madison in 1972 and taught at the Wharton School of the University of Pennsylvania before his present appointment. Van de Ven directed the Minnesota Innovation Research Program during the 1980s and 1990s. Since 1994, he has been studying changes unfolding in health care organizations. In addition to organizational innovation and change, Van de Ven's research has dealt with the Nominal Group Technique, program planning, organization assessment, and interorganizational relationships. He is co-author of 12 books, including *The Innovation Journey* (1999, 2008), *Organization Change and Innovation Processes* (2000), *Handbook of Organizational Change and Innovation* (2004), and *Engaged Scholarship* (2007), which won the 2008 Terry Best Book Award from the Academy of Management.