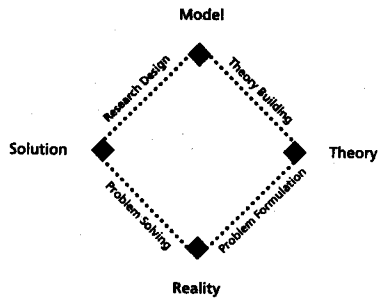
1 Engaged Scholarship in a Professional School



[Academics] appear to have entered a period of non-engagement, cherishing their autonomy over engagement and retreating into the ivory tower.

(Patrick Saveau quoted in Cushman 1999: 328)

Scholarship means something more than research, and engagement is the means for scholarship to flourish.

(Chapter 1, this volume, p. 9)

Understanding how research can advance scientific and practical knowledge is an ongoing challenge for scholars who work in professional schools, such as business, engineering, social work, medicine, agriculture, education, public administration, journalism, and law. A central mission of scholars in professional schools is to conduct research that both advances a scientific discipline and enlightens practice in a professional domain (Simon 1976). Professional schools typically build their raison d'être on the mission of conducting research knowledge that advances both science and practice (Simon 1976; Kondrat 1992; Tranfield and Starkey 1998). But this mission remains an elusive ideal.

Studies show that practitioners often fail to adopt the findings of research in fields such as medicine (Denis and Langley 2002; Dopson et al. 2002), human resources (Anderson et al. 2001; Rynes et al. 2002), social work (Kondrat 1992) and management (Tranfield et al. 2003; Rousseau 2006).

Van de Ven, A. H. (2007). Engaged Scholarship: A Guide for Organizational and Social Research. Oxford, UK: Oxford University Press.

Many top journals[1] have highlighted growing concerns that academic research has become less useful for solving practical problems and that the gulf between science and practice in a profession such as management is widening. There are also growing criticisms 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). Management scholars, for example, are being criticized for not adequately putting their abstract knowledge into practice (Beyer and Trice 1982; Lawler et al. 1985; Hodgkinson et al. 2001). Practicing managers, as well, are criticized for not being aware of relevant research and not doing enough to put their practice into theory (Weick 2001; Van de Ven 2002). As a result, organizations are not learning fast enough to keep up with the changing times.

Academic researchers sometimes respond to these criticisms by claiming that the purpose of their research is not to make immediate contributions to practice; instead it is to make fundamental advances to scientific knowledge that may eventually enlighten practice. However, there is evidence that academic research is also not adequately advancing scientific knowledge. One important indicator of the impact and use of published research by the scientific community is the number of times this research is cited as informing subsequent scientific articles. Based on his citation analysis, Starbuck (2005) reports that papers published in management journals were cited on average only .82 times per article per year. Hence, much current academic research is not contributing in intended ways to either science or practice.

Ways of Addressing the Theory–Practice Gap

This book focuses on the relationship between theory and practice primarily in organization and management studies, which is my field of study. I do not attempt a comprehensive review of the debate, either in general or with respect to the management and organization literature. Rather, I review three ways in which the gap between theory and practice has been framed (as discussed by Van de Ven and Johnson 2006), and then focus on one approach that motivates proposing a method of engaged scholarship. A perusal of literature and discussions with scholars in other professional domains suggest that the principles below for addressing the gap between theory and practice apply equally well in many other professional fields.

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

A KNOWLEDGE TRANSFER PROBLEM

The limited use of research knowledge for science and practice is typically framed as a knowledge transfer problem. This approach assumes that practical knowledge (knowledge of how to do things) in many professional domains derives at least in part from scientific knowledge. Hence, the problem is one of translating and diffusing research knowledge into practice. I discuss this knowledge transfer problem in Chapter 8. Research knowledge is not often communicated in a form that facilitates its transfer, interpretation, and use by an audience as intended. I argue that a deeper understanding of communicating knowledge across boundaries and a more engaged relationship between the researcher and his/her audience are needed if research findings are to have an impact in advancing science and practice.

SCIENCE AND PRACTICE ARE DISTINCT FORMS OF KNOWLEDGE

A second approach to the theory–practice gap views scientific knowledge and practical knowledge as distinct kinds of knowing. Recognition that science and practice produce distinct forms of knowledge has been long-standing. It dates back to Aristotle, who in *The Nicomachean Ethics* (1955), distinguished between *techne* (applied technical knowledge of instrumental or mean–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). More recently, Polanyi (1962), Habermas (1971), Latour (1986), and Nonaka (1994) have made further distinctions between explicit epistemic scientific knowledge and more tacit practical knowledge, which overlap Aristotle's techne and phronesis distinctions. Each reflects a different ontology (truth claim) and epistemology (method) for addressing different questions. To say that the knowledge of science and practice are different is not to say that they stand in opposition or they substitute for each other; rather, they complement one another.

In her review of the theory–practice gap in social work, Kondrat (1992) points out that what has been missing from the discussion are empirical studies of knowledge from practice. What knowledge does the practitioner of an occupation or profession use, and how does he/she obtain it? So also, Schon (1987) asks what does the competent practitioner know? and how does he/she go about knowing 'in' practice? Rather than regard practical knowledge as a derivative of scientific knowledge, these kinds of questions address the epistemological status of 'practical knowledge' as a distinct mode of knowing in its own right. 'When this status is granted, the practical takes its place alongside the scientific as constitutive elements of professional knowledge' (Kondrat 1992: 239).

Scholarly work and managerial work differ in context, process, and purpose. The context of the practitioner is situated in particular problems encountered in everyday activities (Hutchins 1983; Lave and Wenger 1994). As such, managers develop a deep understanding of the problems and tasks that arise in particular situations and of means–ends activities that comprise their solutions (Wallace, 1983). Knowledge of practice in a professional domain is typically customized, connected to experience, and directed to the structure and dynamics of particular situations (Aram and Salipante 2003: 190). In contrast, science 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 and Salipante 2003: 1900). The purpose of practical knowledge is knowing how to deal with the specific situations encountered in a particular case. The purpose of scientific 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.

We may have misunderstood the relationship between knowledge of science and practice, and this has contributed to our limited success in bridging these two forms of knowledge. 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, I suggest taking a pluralist 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, consisting of people who share a common body of specialized knowledge or expertise (Van Maanen and Barley 1986). Each community tends to be self-reinforcing and insular, and limited interactions occur between them (Zald 1995; Cook et al. 1999). 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 co-production among scholars and practitioners. This leads to a third view of the theory–practice gap—namely, a knowledge production problem.

A KNOWLEDGE PRODUCTION PROBLEM

There is a 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-based social science (Simon 1976; Whitley 1984, 2000; Starkey and Madan, 2001; Hinings and Greenwood 2002). Gibbons et al. (1994) and Huff (2000), among others, question the status quo mode of research typically practiced in business and professional schools.

This status quo approach to social research has many variations, but it tends to reflect an unengaged process of inquiry. Researchers typically go it alone to study a research question without communicating with or being informed by other stakeholders (scholars from different disciplines, practitioners with different functional experiences, and other potential users and sponsors) who can make important contributions to understanding the problem domain being investigated. This status quo form of unengaged research is evident in the following characteristics of a research report: (1) a research problem or question is posed but little or no evidence is presented that grounds the nature and prevalence of the problem, its boundary conditions, and why it merits investigation; (2) a single theoretical model is proposed with little consideration given of plausible alternative models for addressing the research problem or question; (3) the research design relies on statistically analyzing questionnaire or secondary data files (such as PIMs, patent data, Compustat, or census files) without the researcher talking to any informants or respondents in the field; and (4) results are presented on the statistical significance of relationships with little or no discussion of their practical significance and implications. Because such research is not grounded in 'reality,' does not entertain alternative models for representing reality, nor is it informed by key stakeholders, it often results in making trivial advancements to science, and contributes to widening the gap between theory and practice. Anderson et al. (2001) characterize this kind of unengaged scholarship as 'puerile science' that is often low in both relevance and rigor. As a consequence, it joins the large proportion of research papers that are not used to advance either science or practice.

Many suggestions have been made for revising and improving this status quo approach to social science research. Many of these suggestions are institutional in nature, such as modifying academic tenure and reward systems, funding criteria for competitive research grants, editorial policies and review procedures of academic journals, and creating additional outlets for transmitting academic findings to practitioners (Lawler et al. 1985; Dunnette 1990). Structural reforms such as these are important institutional arrangements that enable and constrain research. But discussions of structural reforms like this tend to overlook the choices and actions available to individual scholars undertaking research in a professional domain. In this book I focus on methods and strategies that have more immediate relevance to individual scholars engaged in the knowledge production process.

Engaged Scholarship

At the level of the individual researcher, 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 reflects a much larger movement of engaged scholarship for transforming higher education (Zlotkowski 1997–2000). 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 faculty and students from various disciplines in the university and practitioners in relevant professional domains.

> 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 nonfaculty 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 quoted in Kenworthy-U'ren 2005: 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: 15).

Applying these notions of engaged scholarship to the full range of activities of faculty in universities, Boyer (1990) discussed the scholarship of *discovery, teaching, application,* and *integration.* These four dimensions interact to form a rich and unified definition of scholarship. Subsequently, Boyer (1996) further expanded his definition to include the *scholarship of engagement,* which emphasizes how academics relate their teaching, discovery, integration, and application activities with people and places outside the campus and ultimately direct the work of the academy 'toward larger, more humane ends' (Boyer 1996: 20).

For many American public universities, engaged scholarship represents a call to return to their charter mandate of a Land Grant University, as established by the Morrill Land Grant Act of 1862 (Schuh 1984). Three ideas of engagement were central to the founding ideals of a Land Grant University. First, it would provide upper-level education for the masses—a direct response at the time to the elitism and limited relevance of the private universities in the country. Second, the Land Grant University would generate new knowledge by addressing questions and problems of society. Although agriculture was dominant at the time, every area of activity was to be a legitimate subject of intellectual inquiry. Third, the Land Grant University would have a strong outreach mission, which is to provide intellectual leadership by applying the tools of science and technology to address the problems of society. These three ideas gave rise to the familiar tripartite mission of teaching, research, and service. As this brief history indicates, engaged scholarship represents a re-enactment of the founding values and roles of universities as institutions engaged in society and of individual scholars engaging students and community practitioners in their teaching, research, and service.

The engaged scholarship movement has proliferated into numerous university-based initiatives of community outreach, service-learning, clinical teaching, extension services, social emancipation causes, and community-based participatory research. As evident in a Google.com listing of 36,000 entries on 'engaged scholarship,' in November 2006, these initiatives are highly diverse and diffused. Service learning is perhaps the most widely diffused form of engaged teaching, due largely to efforts by national organizations and federal grants (such as Campus Compact, American Association for Higher Education, the National Community Service Trust Act of 1993, and others). Service learning is a credit-bearing educational experience in which students become involved in an organized service activity that augments understanding of topics covered in a university classroom with experiences as volunteers in local sites serving community needs, such as philanthropic agencies, primary and secondary schools, churches, old-age homes, half-way houses, etc. (Bringle and Harcher 1996; DiPadova-Stocks 2005). Professional schools tend to take less of a missionary and more of a training view of service learning through a wide variety of university–industry internships, mentorships, clinical research, and field study projects. An experiment conducted by Markus et al. (1993) found that students in service learning courses had more positive course evaluations, more positive beliefs and values toward service and community, and higher academic achievement. Bringle and Harcher (1996) review other research indicating that service learning has a positive impact on personal, attitudinal, moral, social, and cognitive outcomes for students.

Despite this diffusion and evidence, one of the major barriers to sustained faculty involvement in engaged scholarship is the risk associated with trying to achieve promotion and tenure. A number of national commissions and professions have begun to address these institutional barriers. For example, the 2006 report of the Commission on Community-Engaged Scholarship in the Health Professions focuses on recommendations for recruiting, retaining, and promoting community-engaged faculty members in health professional schools. In addition, the US Department of Education and the W. K. Kellogg Foundation co-sponsored the development of a Community-Engaged Scholarship Toolkit that guides faculty in preparing their career statements and records for faculty promotion and tenure in healthcare and other professional schools (Calleson et al. 2004).

This book applies the principles of engaged scholarship to social research, or what Boyer calls the *scholarship of discovery.*

> No tenets in the academy are held in higher regard than the commitment to knowledge for its own sake, to freedom of inquiry and to following, in a disciplined fashion, an investigation wherever it may lead. The *scholarship of discovery,* at its best, contributes not only to the stock of human knowledge but also to the intellectual climate of a college or university. Not just the outcomes, but the process, and especially the passion, give meaning to the effort. The advancement of knowledge can generate an almost palpable excitement in the life of an educational institution. As William Bowen, former president of Princeton University, said, scholarly research 'reflects our pressing, irrepressible need as human beings to confront the unknown and to seek understanding for its own sake. It is tied inextricably to the freedom to think freshly, to see propositions of every kind in ever-changing light. And it celebrates the special exhilaration that comes from a new idea. (Boyer 1990: 17)

In addition to conveying this passion for knowledge discovery, the term *engaged scholarship* reflects an important identity. *Scholarship means something more than research, and engagement is the means for scholarship to flourish.* Boyer resurrected the honorable term *scholarship,* gave it a broader and more capacious meaning that conveyed legitimacy to the full scope of academic work. 'Surely, scholarship means engaging in original research. But the work of the scholar also means stepping back from one's investigation, looking for connections, building bridges between theory and practice, and communicating one's knowledge effectively' (Boyer 1990: 16).

Pettigrew (2005: 973) asks the question, 'How many of us see ourselves as intellectuals, scholars, and/or researchers?' He states:

> An intellectual is a person having a well-developed intellect and a taste for advanced knowledge, while a scholar is a person with great learning in a particular subject. And a researcher is a person who engages in careful study and investigation in order to discover new facts or information. Even from these rather limited definitions, the narrowness of the researcher identity and role becomes very evident. . . . Scholarship to me implies not just great breadth of learning and appreciation, but also the duty to make these available in dedicated learning, teaching, and professing. An intellectual would be capable of the appreciative system of a scholar but would be harnessing that competence to engage way beyond the boundaries of academic and into the wider reaches of society. I wonder how many of us have made explicit choices of engagement with one or other of the three identities/roles? (Pettigrew 2005: 973)

This poses the important question of how an engaged scholar might formulate a research study of a complex problem in the world that advances both theory and practice? To do this a mode of inquiry is needed that converts the information obtained by scholars in interaction with practitioners (and other stakeholders) into actions that address problems of what to do in a given professional domain. Many research questions and problems exceed the capabilities of individual researchers to study them alone. A methodology is needed that significantly expands researchers' capabilities to address such complex problems and questions.

I propose a method of engaged scholarship for expanding the capabilities of scholars to study complex problems and create the kind of knowledge that advances both science and practice. *Engaged scholarship* is defined as a participative form of research for obtaining the different perspectives of key stakeholders (researchers, users, clients, sponsors, and practitioners) in studying complex problems. By involving others and leveraging their different kinds of knowledge, engaged scholarship can produce knowledge that is more penetrating and insightful than when scholars or practitioners work on the problems alone.

ENGAGED SCHOLARSHIP RESEARCH MODEL

Past arguments for collaborative research have tended to be one-sided and focus on the relevance of academic research *for* practice. I focus more attention in this book on the question of how scholarship that is engaged *with* (rather than *for*) practice can advance basic scientific knowledge? Engaged scholarship emphasizes that research is not a solitary exercise; instead it is a collective achievement. Engagement means that scholars step outside of 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.

Using a diamond model as illustrated in Figure 1.1, I propose that scholars can significantly increase the likelihood of advancing fundamental knowledge of a complex phenomenon by engaging others whose perspectives are relevant in each of these study activities:

- *Problem formulation*—situate, ground, diagnose, and infer the research problem by determining who, what, where, when, why, and how the problem exists up close and from afar. As discussed in Chapter 3, answering these journalist's 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*—create, elaborate, and justify a theory by abductive, deductive, and inductive reasoning (as discussed in Chapter 4). Developing this theory and its plausible alternatives requires conversations with knowledge experts from the relevant disciplines and functions that have addressed the problem, as well as a review of relevant literature.
- *Research design*—develop a variance or process model for empirically examining the alternative theories. As noted in Chapters 5–7, doing this well typically requires getting advice from technical experts in research methodology and 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 alternative models better answer the research question about the problem. Chapter 8 argues that increases in the difference, dependence, and novelty of knowledge between people at a boundary require more engaged forms of communication, starting with written reports and presentations for knowledge transfer, then conversations to interpret different meanings of the report, and then pragmatic and political negotiations to reconcile conflicting interests.

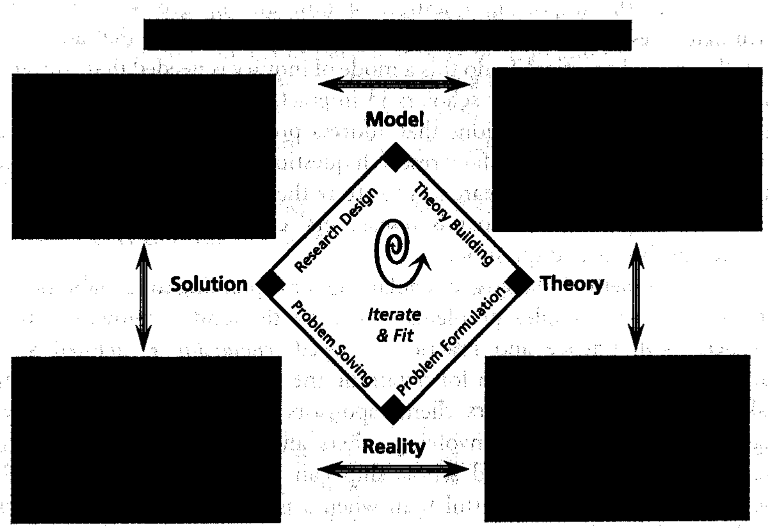


Figure 1.1. Engaged scholarship diamond model

These activities can be performed in any sequence. I discuss these research activities in a problem solving sequence beginning with problem formulation, then searching for theories relevant to the problem, testing them, and applying the findings. There are many other possible starting points and sequences. For example, some scholars may start with a theory and then search for a problematic situation that may be appropriate for applying and evaluating the theory. Other scholars may be methodologically inclined, and interested in finding problems and developing theories with their methodological tools (as was the case in early developments of social network analysis). Still others may begin with a solution or program that requires evaluation research in order to determine the particular kinds of problems and context for which it may be appropriate.

These different starting motivations and orientations quickly meld together in the course of a study because the four activities are highly interdependent and are seldom completed in one pass. Multiple iterations and revisions of these research activities are often needed throughout the duration of a study. In the process, many sub-problems emerge in performing each research activity, and all remain simultaneously active and need to be addressed as an interdependent set. It is only when the process is complete that a fairly coherent pattern emerges as reflected in Figure 1.1.

Maintaining balance in performing these tasks repetitively is important. Given finite resources for conducting a study, I recommend that scholars allocate their time and efforts about equally to problem exploration, theory building, research design and conduct, and problem solving activities. Spending too much time or effort on only one or two research activities often results in unbalanced or lop-sided results where some activities are 'over-engineered' while others are incomplete.

This suggestion of paying equal attention to all four research activities is not evident in many research methodology texts in the social sciences. They tend to focus on research design, and pay relatively little attention to the processes of problem formulation, theory building, and problem solving. In addition, while these texts provide good technical treatments of research designs and data analysis, they largely ignore social processes of engaging stakeholders in problem formulation, theory building, research design, and problem solving (as illustrated in Figure 1.1). Social research is an intensely social process. Throughout the book I emphasize that all four research activities are equally important in conducting a study, and that each activity entails a different set of tasks that can be accomplished better by engaging relevant stakeholders rather than going it alone.

The essential steps in performing the four activities of the diamond model are illustrated in Figure 1.1. They can be evaluated in terms of five criteria: relevance, validity, truth, impact, and coherence.[2] The problem should be grounded in a reality that is relevant to an intended research audience in the scholarly and professional communities. The theoretical model should be expressed clearly, it should consist of a logically valid argument. The design and conduct of the research should apply the standards and methods that a scientific community believes will produce a truthful solution. The findings of the research should have an impact in advancing science and enlightening practice in a profession. In addition to relevance, validity, truth, and impact, a fifth criterion—coherence—is equally important for evaluating the engaged scholarship process.

In the PhD seminar I teach on engaged scholarship, the major assignment is for students to develop a good research proposal.[3] A good research proposal is defined as one that adequately describes each of the research activities in terms of the criteria presented in Table 1.1. Students submit different parts of the proposal every few weeks of the semester. As an instructor, I provide students feedback on their in-progress proposals, and they revise their research proposals several times until it is judged to be acceptable. Thus, through several iterations of revising-and-extending their proposals, students develop a research proposal, which they submit for funding and implement either as a research project or as an initial draft of their dissertation proposal.

[2] Scholars from different philosophical persuasions often associate different meanings with these criteria. My interpretations of these criteria should become clear in subsequent chapters devoted to each of the research activities in the engaged scholarship model.

[3] The most recent version of this course can be accessed by following the link on *MGMT 8101, Theory Building and Research Design* from my faculty web page at the Carlson School of Management, University of Minnesota (available at: (http://umn.edu/~avandeve). This course web page provides a wealth of additional information, resources, and links that supplement the topics and issues discussed in this book.

Table 1.1. Criteria for evaluating a research proposal

1. Statement of the research problem: • is situated in terms of perspective, focus, level, and scope; • problem symptoms or elements are clearly defined & grounded in reality; • a diagnosis is made that analyzes patterns or relationships among elements; • based on the diagnosis, an inference (a claim with reasons) is made for the problem.	___
2. The research question: • is stated in analytical and researchable terms; • permits more than one plausible answer.	___
3. The research proposition (theory): • clearly states an expected relationship among concepts or events; • is supported with an argument (i.e., claim, reasons, evidence, assumptions, & reservations); • directly addresses the research question and problem; • is compared with a plausible alternative theory or the status quo answer; • travels across levels of abstraction.	___
4. The research design clearly spells out: • theoretical unit of analysis and unit of observation; • case/survey/experimental design for variance or process theory; • sample or replication logic and sample selection; • definitions and measurement procedures for variables or events; • threats to internal, statistical, external, & construct validities.	___
5. Research implementation and problem solving for theory and practice: • the contributions/implications of the research for science *and* practice are clearly stated; • methods for communicating and sharing findings with target audiences/users are discussed; • statement of how research findings will be used/applied is prudent; • relevant stakeholders are engaged in each of the above steps.	___
Comments:	Total Score: ___

Note: Please evaluate this report by using this five-point scale:
1 = not addressed or evident in the report
2 = attempt made but some errors occurred in the analysis/answer.
3 = attempt made but the result needs more work, elaboration, or refinement.
4 = attempt made with good result; issue accomplished; no further work needed.
5 = attempt made with excellent result; issue accomplished with distinction.

Engaged scholarship can be practiced in many different ways for addressing a variety of basic and applied research questions. For example, researchers might engage stakeholders in a study in order to: (1) obtain their perspectives and advice on a basic research question; (2) collaborate and co-produce knowledge; (3) design and evaluate a policy or program; or (4) intervene and implement a change to solve a client's problem. As these alternatives suggest, principles of engaged scholarship apply to many forms of basic or applied social research. They are discussed later in this chapter.

The four research activities in the engaged scholarship model illustrated in Figure 1.1 serve as the organizing framework of this book. Following an overview of the philosophy of science underlying this model of engaged scholarship, I discuss each of the four research activities in the engaged scholarship model. I also indicate how subsequent chapters treat detailed steps and procedures entailed in each research activity.

PHILOSOPHY OF SCIENCE UNDERLYING ENGAGED SCHOLARSHIP

Underlying any form of research is a philosophy of science that informs a scholar's approach to the nature of the phenomenon examined (ontology) and methods for understanding it (epistemology). Philosophers have debated these issues endlessly, and constructed a variety of philosophies for conducting research. Practitioners of science, in turn, are influencing how these philosophies are developed and expressed in their research. Chapter 2 attempts to provide a synthesis of this reciprocal relationship between the philosophy and practice of science with a historical review of four philosophies of science—positivism, relativism, pragmatism, and realism. It provides a discussion of how key ideas from each philosophy inform engaged scholarship, and how the practice of engaged scholarship might influence these philosophies of science.

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 true in an absolute sense (Suppe 1977: 649). Rather, from a critical realist perspective that I adopt, there is a real world out there, but our attempts to understand it are severely limited and can only be approximated. This perspective argues that all facts, observations, and data are theory-laden and embedded in language. Moreover, most phenomena in the social world are too rich to be understood adequately by any single person or perspective. Consequently, any given theoretical model is a partial representation of a complex phenomenon that reflects the perspective of the model builder. No form of inquiry is value-free and impartial; instead each model and perspective is value-full. This requires scholars to be far more reflexive and transparent about their roles, interests, and perspectives when conducting a study than they have in the past. For example, instead of assuming an authoritative and objective 'God's Eye view' of social phenomena, I follow Henrickx (1999) in proposing that engaged scholars adopt a participant frame of reference to learn about and understand a subject through discourse with other stakeholders.

Critical realism views science as a process of constructing models that represent or map intended aspects of the world, and comparing them with rival plausible alternative models (Rescher 2000). For example, Giere (1999: 77) 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.

Research knowledge of a complex phenomenon advances by comparing the relative contributions of different models. Azevedo (1997) discusses how the coordination of multiple models and perspectives may reveal the robust features of reality by identifying those features that appear invariant (or convergent) across at least two (and preferably more) independent theories. From her perspective, a pluralist approach of comparing multiple plausible models of reality is essential for developing reliable scientific knowledge.

But the engagement of different stakeholders in a study often produces inconsistent and contradictory perspectives of a problem domain being examined. Pluralistic perspectives should not be dismissed as noise, error, or outliers—as they are typically treated in a triangulation research strategy. Chapter 9 discusses how these different outcomes require an expansion of traditional explanations of triangulation that focus on convergent central tendencies to include explanations based on inconsistent findings through arbitrage and contradictory findings with methods for reasoning through paradoxical findings.

It is often easier to construct meaningful explanations in cases where the evidence is convergent. For example, Azevedo (1997) advocates the use of multiple models for mapping a problem being investigated, and argues that knowledge that is reliable is invariant (or converges) across these models. Convergent explanations rely on similarities, consensus, and central tendencies in explaining a problem or issue under investigation. Convergent explanations tend to treat differences and inconsistencies as bias, errors, outliers, or noise.

More difficult (but often more insightful) explanations emerge when different data sources yield inconsistent or contradictory information about a phenomenon. Arbitrage provides a strategy for developing holistic, integrative explanations based on different accounts of the same phenomenon. Friedman (2000: 24) points out that in academe and elsewhere, 'there is a deeply ingrained tendency to think in terms of highly segmented, narrow areas of expertise, which ignores the fact that the real world is not divided up into such neat little bits.' He argues that the way to see, understand, and explain complex problems in the world is to systematically connect the different dots, bits, and pieces of information through arbitrage—'assigning different weights to different perspectives at different times in different situations, but always understanding that it is the interaction of all of them together that is really the defining feature of the [system]' (Friedman 2000: 23–4). Arbitrage is a strategy of explaining differences by seeing the interdependencies and webs of entanglements between different and divergent dimensions of a problem, its boundaries, and context.

Finally, contradictory information from different sources may represent instances of conflicting values and interests among pluralistic stakeholders about the problem or issue being examined. Explanations of a problem domain should obviously reflect these contradictions when observed. In Chapters 8 and 9, I discuss four general methods for reasoning through paradoxes by either: balancing between opposites, shifting levels of analysis, alternating positions over time, and introducing new concepts that dissolve the paradox. Inconsistent and contradictory findings are important, for they represent anomalies that trigger theory creation.

Campbell's (1988: 389) evolutionary perspective of science provides a possible avenue for addressing the simultaneous need to establish valid and reliable representations of a problem domain being examined. He argues that the models that better fit the problems they were intended to solve are selected, 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. This evolutionary perspective is based on a pragmatic philosophy of science. Among the plausible alternative models competing to explain a given phenomenon, the model that wins out at a particular moment in time is the one that is judged to best represent the phenomenon. Fortunately, only a finite set of three to five plausible models tend to compete for selection at a given time, as indicated by Collins's (1998) historical review of competing models for explaining a phenomenon.

Explanations based on arbitrage and paradoxical reasoning represent dialectical methods of inquiry where understanding and synthesis of a complex problem evolve from the confrontation of divergent thesis and antithesis. Dialectical reasoning is not a strategy for addressing narrow technical problems where one looks for expert judgments to converge on a correct answer. Instead, it is a strategy for triangulating on complex real-world problems by involving individuals whose perspectives are far from the average (Mitroff and Linstone 1993: 69). 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 viewpoint or conceptual model.

Thus, engaged scholarship 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... but communication across perspectives and willingness to work toward establishing coherence is a precondition. (Azevedo 2002: 730)

Pluralism consists of not only multiple perspectives, but also a degree of openness and equality among them for addressing complex social phenomena. Participants often experience conflict and interpersonal tensions associated with juxtaposing people with different views and approaches to a problem. Managing conflict constructively is not only important but lies at the heart of engaged scholarship. Attempting to avoid tensions between scholars and practitioners, as we have in the past, is a mistake, for it blinds us to very real opportunities that are possible from exploiting the differences underlying these tensions in understanding complex phenomena.

PROBLEM FORMULATION

Problem formulation consists of situating, grounding, and diagnosing a research problem or issue in reality. Of course, different observers will see different 'realities.' In Chapter 3 I take a critical realist perspective and argue that there is a real world out there, but our representation and understanding of it is a social construction; reality does not exist independently of the observer's schemata or conceptual frame of reference (Weick 1989). As a consequence, the formulation of a research problem involves a complex sensemaking process of applying various conceptual templates or theories to determine what to look for in the real world and how to unscramble empirical materials into a recognizable and meaningful research problem.

Problem formulation plays a crucial role in conducting research and potentially affects succeeding phases, including theory building, research design and conduct, and conclusions. Yet problem formulation is often rushed or taken for granted. People tend to be solution-minded, rather than problem-minded. When problem formulation is rushed or taken for granted, in all likelihood important dimensions of the problem go undetected and opportunities are missed (Volkema 1995).

Social science today suffers from elaborating theories that are often based on insufficient grounding in concrete particulars. It also suffers from a lack of relevance as perceived by the intended audiences or users of the research (Beer 2001; Rynes et al. 2001). As a consequence, theories tend to be grounded in myths and superstitions. Those who generalize from experience can answer the questions, 'For example? From whose point of view? What is the point of view?' Lacking answers to these questions often leads to unfounded generalizations. In crime investigation, establishing the case is mandatory for pursuing it. Merton (1987: 21) cautioned that an important first element in the practice of science is '*establishing the phenomenon.*' Evidence and argument should clearly indicate that it is enough of a regularity to require and allow explanation. In this way 'pseudofacts that induce pseudoproblems are avoided' (Hernes 1989: 125).

Grounding the problem or phenomenon in reality is a crucial step in any research study. You might ask, what kind of research problems require engagement of others? I argue that the more complex the problem or the bigger the research question, the greater the level of engagement is required of 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. Caswill and Shove (2000*b*: 222) 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 they seldom provide an immediate pay-off 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 (Hodgkinson et al. 2001). 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. 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 (2000*b*: 221) state, practitioners are 'often more attracted by new ideas and concepts than by empirical materials.'

Critics have argued that practitioner involvement in formulating research questions may steer the questions in narrow, short-term, or particularistic directions (Brief and Dukerich 1991; Grey 2001; Kilduff and Kelemen 2001). Ironically, this argument seems to assume that academics know better how to formulate researchable questions than practitioners, but when interacting with practitioners, researchers may behave as 'servants of power' (Brief 2000) by cowering to the interests of powerful stakeholders. Like Anderson et al. (2001), I view an engaged scholar as being more humble and also standing in a more egalitarian relationship with practitioners and other stakeholders when trying to understand an important question or phenomenon that requires research. Big research questions tend to reside in a buzzing, blooming, confusing world. Learning about the nature of the question or phenomenon in such ambiguous settings is facilitated by obtaining the 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*a*, 2002*b*) critique the assumption that the advancement of theory requires 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 and Shove 2000*b*: 221)

Indeed, the belief that interactions between people with different views and approaches advances academic (and practical) inquiry lies at the heart of engaged scholarship.

THEORY BUILDING

Theory building involves the creation, elaboration, and justification of a body of knowledge that is relevant to the research problem. A theory is the mental image or conceptual framework that is brought to bear on the research problem. Theories exist at various levels of abstraction for representing knowledge. A formal classification of the structure of knowledge is the Dewey indexing system found in all libraries. It classifies all knowledge into ten categories with ten subcategories, another ten sub-subcategories, and so on. This classification system packages knowledge by disciplines, paradigms, schools of thought, and theories on various subjects. You may not like such a formal hierarchical structure of knowledge, but you need to know it if you hope to find a book in the library.

This nested hierarchical structure not only indexes bodies of knowledge, it also structures our views of reality by specifying what problems and what aspects of problems are relevant and not relevant. Selecting and building a theory is perhaps the most strategic choice that is made in conducting a study. It significantly influences the research questions to ask, what concepts and events to look for, and what kind of propositions or predictions might be considered in addressing these questions. Because a theory is so influential in directing (or tunneling) a research study, Chapter 4 examines the activities and patterns of reasoning involved in theory building, and the importance of engaging others in the process of theorizing.

Different and opposing views are often expressed about theory building. They range from those who emphasize theory creation and argue that trivial theories are often produced by hemmed-in methodological strictures that favor validation rather than imagination (Weick 1989; Mintzberg 2005), to those who focus on elaborating and justifying a theory by calling for clear definitions, internal logical consistency, and verifiability (Bacharach 1989; Peli and Masuch 1997; Wacker 2004). In part these writers are right in describing one theory building activity, but wrong in ignoring other activities involved in theory building. Many of these oppositions dissolve when theory building is viewed not as a single activity, but as entailing at least three activities—creating, constructing, and justifying a theory.

Chapter 4 discusses how these three activities entail different patterns of reasoning: (1) the creative germ of a promising (but often half-baked) conjecture is typically created through a process of abductive reasoning to resolve an anomaly observed in the world; (2) then a theory is constructed to elaborate the conjecture by using basic principles of logical deductive reasoning to define terms, specify relationships, and conditions when they apply; and (3) if the merits of the theory are to be convincing to others, the theory is justified by crafting persuasive arguments and using inductive reasoning to empirically evaluate a model of the theory in comparison with rival plausible alternative models. In other words, theory creation involves an abductive process of 'disciplined imagination' (Weick 1989), theory construction entails logical deductive reasoning, and theory justification requires inductive reasoning and argumentation. Hence, theorizing entails different patterns of reasoning, and much can be learned about the scientific enterprise by understanding the complementary relations among these different patterns of reasoning.

A key recommendation discussed in Chapter 4 is to develop alternative theories and methods to study a problem. Multiple frames of reference are needed to understand complex reality. As mentioned before, engaged scholarship is a pluralistic methodology. 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 and with particular interests in mind. Comparing and contrasting plausible alternative models that reflect different perspectives are essential for discriminating between error, noise, and different dimensions of a complex problem being investigated. Allison (1971) provides a good example of triangulating on the Cuban Missile Crisis with three models—a rational actor, organization behavior, and a political model. Each model is a conceptual lens that "leads one to see, emphasize, and worry about different aspects of an event" (Allison 1971: 5). Combined, complementary models provide richer insights and explanations of a phenomenon that would otherwise remain neglected.

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.

The typical strategy in social science research is to use a single theory to examine a given phenomenon. I argue that you have 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; Stinchcombe 1968*a*; Singleton and Straits 1999; Poole et al. 2000). Stinchcombe (1968*a*), for example, advises researchers to develop 'crucial' propositions that 'carve at the joints' (as Plato described) of positions 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 rival plausible hypotheses are examined. Such studies are likely to add significant value to theory and practice. Testing rival plausible hypotheses also provides the insurance of a win–win outcome for investigators—no matter what research results are obtained, if properly executed it can make an important contribution.

RESEARCH DESIGN

Building plausible theories that address the research question and problem typically sets the stage for designing operational models to empirically examine key aspects of the theories. Research design activities include developing specific hypotheses and empirical observation procedures (based on the theoretical model) that predict what data should be obtained if the model provides a good fit to the real world. A theory is typically not open to direct inspection, while a model makes operational some specific predictions of a theory, which can be subjected to empirical inspection. The theory and the hypothesis are related by reasoning or calculation, while the real world and the data are related by a physical interaction that involves observation or experimentation. As Giere states,

> it is understood that the model fits only in some respects and then only to some specified degree of accuracy.... If what is going on in the real world, including the experimental setup, is similar in structure to the model of the world, then the data and the prediction should *agree*. That is, the actual data should be described by the prediction. On the other hand, if the real world and the model are not similar in the relevant respects, then the data and the prediction may *disagree*. (Giere 1997: 30)

This process can be generalized when comparing alternative predictions or hypotheses from plausible alternative models. Empirical evidence can be obtained on alternative predictions and compared to determine which empirically-based prediction offers the better or stronger explanation. When data evaluating the hypotheses from one model offer worse explanations than hypotheses from other models, then presumably the former model is abandoned in favor of the latter models.

A wide variety of research designs can be employed to gather empirical evidence for evaluating the predictions or hypotheses from different models. Research methodology texts typically divide and discuss these research designs in terms of experiments (e.g., Kirk 1995), quasi-experiments (Shadish et al. 2002), comparative case studies (Yin 2003), and various qualitative research methods (Denzin and Lincoln 1994; Miles and Huberman 1994). Before delving into the operational details of these research designs in Chapters 6 and 7, Chapter 5 provides an overview of two basic approaches that are often undertaken to examine process versus variance models. These two models capture basic distinctions between research studies undertaken to investigate either: (1) variance or causal questions of 'what causes what'; or (2) process questions of 'how things develop and change over time.'

Mohr (1982) first distinguished variance and process models in an explanation of organizational behavior. In developing a formalism for the representation of social action, Abell (1987) contrasted variance and narrative approaches, while Abbott (1984, 1990) compared stochastic and narrative explanations in sociology. The common thread running through these works is the difference between scientific explanations cast in terms of statistical associations between independent and dependent variables versus explanations that tell a narrative or story about how a sequence of events unfolds over time to produce a given outcome. Chapter 5 discusses these divergent explanations between variance and narrative explanations. They constitute fundamentally different research approaches for examining variance theories that make causal predictions among variables, as distinct from process theories that examine progressions in the temporal development of how events unfold in a social entity, be it an individual, group, organization, or larger community.

An example from the study of organizational change may be useful to clarify these distinctions between variance theories and process theories. Van de Ven and Huber (1990) point out that studies of organizational change tend to focus on two kinds of questions:

- What are the antecedents or consequences of the change?
- How does a change process emerge, develop, grow or terminate over time?

The 'What' question usually entails a variance theory explanation of the input factors (independent variables) that statistically explain variations in some outcome criteria (dependent variables). The 'How' question requires a process theory explanation of the temporal order and sequence in which a discrete set of events occurred based on a story or historical narrative (Abbott 1988). In terms of causality, the 'What' question requires evidence of co-variation, temporal precedence, and absence of spurious associations between the independent and dependent variables. The 'How' question explains an observed sequence of events in terms of some underlying generative mechanisms that have the power to cause events to happen in the real world and the particular circumstances or contingencies when these mechanisms operate (Tsoukas 1989).

A researcher adopting a variance model is inclined to decompose organizational processes into a series of input–output analyses by viewing each event as a change in a variable (e.g., the number of product innovations), and then examining if changes in this variable are statistically associated with some other independent variable (e.g., R&D investment). From a variance theory perspective, events represent changes in the states of a variable, and these changes are the building blocks of variations among variables in an input–process–output model. But since the process question is not whether, but *how*, a change occurred, one needs to narrate a story of the sequence of events that unfolded as the product innovation emerged. Once the sequence or pattern of events in a developmental process is found, then one can turn to questions about the causes or consequences of the event sequence.

Having distinguished the two questions, it is important to appreciate their complementary relationship. An answer to the 'What' question typically assumes or hypothesizes an answer to the 'How' question. Whether implicit or explicit, the logic underlying an answer to a variance theory is a process story about how a sequence of events unfold to cause an independent (input) variable to exert its influence on a dependent (outcome) variable. For example, to say that R&D investment causes organizational innovativeness is to make important assumptions about the order and sequence in which R&D investment and innovation events unfold in an organization. Thus, one way to significantly improve the robustness of answers to the first (variance theory) question is to explicitly examine the process that is assumed to explain why an independent variable causes a dependent variable.

By the same token, answers to 'How' questions tend to be meaningless without an answer to the corresponding variance theory questions. As Pettigrew (1990) argues, theoretically sound and practically useful research on change should explore the contexts, content, and process of change through time. Just as change is only perceptible relative to a state of constancy, an appreciation of a temporal sequence of events requires understanding the starting (input) conditions and ending (outcome) results.

Given the different but complementary epistemologies of variance and process theories discussed in Chapter 5, I delve into detailed considerations for designing variance and process studies in Chapters 6 and 7. Chapter 6 focuses on experimental, quasi-experimental, and survey designs for empirically evaluating causal models in variance research. Chapter 7 discusses methods for designing and conducting longitudinal cases, historical, and field studies to examine processes of how phenomena develop and change over time.

You might question if this 'theory testing' approach admits to a more exploratory 'grounded theory building approach' to research? My response is that the difference between these two modes of inquiry is a matter of timing and sequence in performing the theory building and research design activities of the diamond model. In exploratory studies, propositions typically develop after data are collected and analyzed. Thus, I recommend that the methods discussed in Chapter 4 for developing theories be applied after or while the data are being collected and analyzed. However, as discussed in Chapter 2, all data, facts, and observations are laden with theories that are tacit or explicit in the minds of the investigators. Any observations presuppose a selective frame of reference of a chosen object and concepts. Before collecting data, the focus of an exploratory study can be significantly clarified by meeting with key study stakeholders to discuss and explain what concepts might be used to observe the phenomenon.

Most studies, of course, include elements of both theory building and theory testing. Numerous iterations in running the paths of the diamond model are typically required in conducting any research project. Seldom, if ever, can a researcher complete a study by running the paths in one linear sequence; much back-tracking and jumping from one base to another is the typical process sequence.

USING RESEARCH FOR PROBLEM SOLVING

The problem solving activity of the engaged scholarship process focuses on linking the research findings back to the problem observed in the practitioner and the scientific communities. Generally, this involves executing the research design to produce empirical evidence for a solution to the research problem and question that initially motivated the research. At a minimum, a research solution entails a report of research findings and a discussion of their implications for theory and practice. Many researchers consider their communication task completed when they publish their report in a scientific journal and make verbal presentations of it at professional conferences as well as to host organizations and practitioners who sponsored the research.

This practice assumes that communicating research findings entails a one-way transfer of knowledge and information from the researcher to an audience. The underlying assumption of this view is that if an idea is good enough, it will be used. But research knowledge based on sound empirical evidence

is often not used or adopted as intended by either scientists or practitioners. I argue that a deeper understanding of communicating knowledge across boundaries and a more engaged relationship between the researcher and his/her audience are needed if research findings are to have an impact in advancing science and practice.

It is one thing to write a research paper, and quite another to transfer, interpret, and implement study findings at the communication boundaries of both scientific and practitioner communities. Estabrooks (1999: 15) points out that 'Many factors get in the way of using research, and empirically, we know very little about what makes research use happen or not happen.' Recently, scholars have begun to reconceptualize knowledge transfer as a learning process in which new knowledge is shaped by the learner's pre-existing knowledge and experience. Individuals are not simply sponges, soaking up new information without filtering or processing. 'Knowledge use is a complex change process in which "getting the research out there" is only the first step' (Nutley et al. 2003: 132). Neither scientists nor practitioners simply apply scientific research, but collaborate in discussions and engage in practices that actively interpret its value to accomplish their tasks.

I anchor Chapter 8 in Carlile's (2004) framework of knowledge transfer, translation, and transformation. It provides useful insights into how researchers might communicate their study findings at the knowledge boundaries with different audiences. The framework emphasizes that communication across boundaries requires common knowledge among people to understand each other's domain-specific knowledge. When the difference, dependence, and novelty of domain-specific knowledge between people at a boundary increase, then progressively more complex processes of knowledge transfer, translation, and transformation are needed to communicate the meanings and potential uses of that knowledge.

When the people at a knowledge boundary share the same common lexicon and syntax for understanding their different and interdependent domain-specific knowledge, then it can be communicated using a conventional information processing view of knowledge transfer from a speaker to listeners through written and verbal reports. The major challenge of knowledge transfer is to craft a sufficiently rich message and medium to convey the novelty of the information from the speaker to the audience. For example, written reports, verbal presentations, and face-to-face interactions between the speaker and listeners represent three increasingly rich media for knowledge transfer. In addition, logos, pathos, and ethos represent three increasingly rich dimensions of a message.

Knowledge transfer, however, even when communicated in the richness of a rhetorical triangle, typically remains a one-way transmission of information from a sender to a receiver. The listener in knowledge transfer remains relatively silent, but is never inactive. Authors of research reports will not know this unless they engage in conversations with readers or listeners of a report. Then it becomes clear that listeners often have different interpretations and meanings of the novel information than the speaker intended. A research report is not treated as a social fact or as having a 'fixed' meaning. Rather, it is open to multiple and unlimited meanings, interpretations, and actions among participants (speakers and listeners) engaged in the text. Hence, when communicating research findings, a research report should be viewed as a first—not the last—step for researchers to engage in conversations with potential users, and thereby gain a broader and deeper appreciation of the meanings of research findings.

When interpretive differences exist in the meanings of research findings, then a more complex communication boundary of 'knowledge translation' must be crossed. At this boundary, speakers and listeners engage in conversations and discourse to mutually share, interpret, and construct their meanings of research findings. Speakers and listeners become co-authors in mutually constructing and making sense of their interactions. At the knowledge translation boundary, conversation is the essence and the product of research. Engaging in conversation and discourse with an audience requires researchers to adopt a hermeneutic 'participant view' rather than a 'God's Eye view' of research findings.

Communicating across knowledge transfer and translation boundaries may surface conflicting interests among parties that entails an even more complex political boundary where participants negotiate and pragmatically transform their knowledge and interests from their own to a collective domain. As Carlile (2004) states, 'When different interests arise, developing an adequate common knowledge is a political process of negotiating and defining common interests.'

Finally, seldom can knowledge transfer, translation, and transformation be accomplished with only one communication among people across boundaries. Numerous interactions are required to share and interpret knowledge, create new meanings, and negotiate divergent interests. The engaged scholarship process provides a strategy to approximate this by repeated engagements of stakeholders in each activity of the research process: problem formulation, theory building, research design, and problem solving.

FORMS OF ENGAGED SCHOLARSHIP

Engaged scholarship can be practiced in many different ways and for many different purposes. Figure 1.2 illustrates four different forms of engaged scholarship. As discussed in Chapter 9, these different forms of engaged scholarship depend on: (1) whether the purpose of a research study is to examine basic questions of description, explanation, and prediction or on

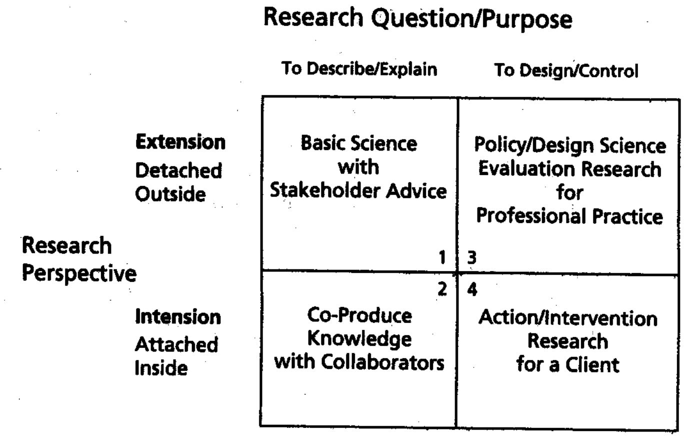


Figure 1.2. Alternative forms of engaged scholarship

applied questions of design, evaluation, or action intervention, and (2) the degree to which a researcher 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 solicits advice and feedback from key stakeholders and inside informants on each of the research activities as listed in Figure 1.1. 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 listed in Figure 1.1 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 of a profession in question. 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. In terms of the engaged scholarship model, 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 not apply or 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 form of research 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 (description, explanation, design, or control of a problematic situation). Which 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 in terms of 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 domain.

CAVEATS OF ENGAGED SCHOLARSHIP

Several caveats of engagement should be recognized. As discussed in Chapter 9, the practice of engaged scholarship raises a number of issues that are often not salient in traditional approaches to social research. They include: (1) the challenges of engagement; (2) being reflexive about the researcher's role in a study; (3) establishing and building relationships with stakeholders; and (4) spending time in field research sites. Engagement does not necessarily imply that a researcher loses control of his/her study, but it does entail greater accountability to the stakeholders involved in a study. Engagement often raises false expectations that concerns expressed will be addressed. Engagement does not require consensus among stakeholders; much learning occurs through arbitrage by leveraging differences among stakeholders. Negotiating different and sometimes conflicting interests imply that creative conflict management skills are critical for engaged scholars. Without these skills, engagement may produce the ancient Tower of Babel, where intentions to build a tower to reach heaven were thwarted by the noisy and confusing language of the people.

Engaging stakeholders (other researchers, practitioners, sponsors, users, or clients) in problem formulation, theory building, research design, and problem solving represents a more challenging way to conduct social research than the traditional approach of researchers going it alone. But throughout this book I argue that the benefits exceed the costs. By involving stakeholders in key steps of the research process, engaged scholarship provides a deeper understanding of the problem investigated than is obtained by traditional detached research. My argument assumes, of course, that the primary motivation of engaged scholars for undertaking research is to understand this complex world, rather than to get published and promoted. The latter is a by-product of the former.

Discussion

This chapter introduced a research process model of engaged scholarship that serves as the organizing framework for this book. This model incorporates a contemporary philosophy of science and a set of methods for undertaking research with the aim of advancing knowledge in both a scientific discipline and in the practice of a profession. I argued that a research project involves four activities:

1. Problem formulation—ground the research problem and question in the real world;
2. Theory building—develop or select a conceptual model that addresses the problem as it exists in its particular context;
3. Research design and conduct—gather empirical evidence to compare plausible alternative models that address the research question; and
4. Problem solving—communicate and apply the research findings to solve the research question about the problem existing in reality.

Subsequent chapters discuss ways to perform each of these activities in this process model of engaged scholarship. Scholars can cover the four bases of the diamond model in any order they like. But all the bases must be covered to complete a research project.

This engaged scholarship diamond model incorporates to a wide variety of research methods including: basic or applied; theory building or theory testing; variance or process theory; cross-sectional or longitudinal; quantitative or qualitative; and laboratory, simulation, survey, archival, or other observation methods. Depending on the problem or question being investigated, engaged scholarship may involve any of these different categories of research. While engaged scholarship entertains a wide variety of research methods, it directs the research process by specifying the core set of activities of a research project that need to be performed from start to finish. Because the core activities of problem formulation, theory building, research design, and problem solving are highly interdependent, so also are the methods that are selected for doing these activities. Thus, the critical task is to adopt and execute the research models and methods that fit the chosen research problem or question being addressed.

ALTERNATIVE RESEARCH MODELS

A basic proposition of the ES model is to compare and contrast a proposed model with plausible alternative models. To 'walk this talk,' I compare the engaged scholarship model with two other plausible alternative models for conducting social research: a general systems model of problem solving by David Deutsch (1997) and a model of the scientific episode by Ronald Giere (1999).

Deutsch's Problem Solving Model

Several scholars have observed that science can be seen as a problem solving activity (Campbell 1988; Azevedo 1997, 2002; Deutsch 1997). For example, David Deutsch, a quantum physicist at the University of Oxford, describes science in terms of five problem solving stages: (1) the *problem*; (2) a *proposed model* or conjectured solution; (3) *criticism* with experimentation; (4) a *solution* of replacing erroneous theories; and (5) a *new problem* that recycles the process. Deutsch (1997: 62) explains the problem solving stages as beginning when a problem surfaces. A problem starts when a theory is not adequate and a new theory is needed. It is defined not only as an emergency or the root of anxiety, but is more when ideas are not adequate and there may be a better explanation (Deutsch 1997). In other words and as discussed in Chapter 3, research often begins with an anomaly requiring abductive reasoning because the current explanation or theory may be too narrow or not broad enough to explain the anomaly.

Following the discovery of a problem (stage 1), the next stage is *conjecture.* This is 'where new or modified theories are proposed in the hope of solving the problem (stage 2). The conjectures are then *criticized*...using scientific methods of experimental testing. This entails examining and comparing them to see what offers the best explanation, according to the criteria inherent in the problem (stage 3)' (Deutsch 1997: 64). A conjectured theory is not adopted when it seems to provide explanations worse than other theories. But, if one of the principle theories is abandoned for a new one (stage 4), then the problem solving exercise is deemed a 'tentative' success. Deutsch says the success is tentative since later problem solving may involve replacing or changing these new theories and in some cases even going back to and revising the ideas that were deemed unsatisfactory. Deutsch states, 'the solution, however good, is not the end of the story: it is a starting point for the next problem solving process (stage 5)' (Deutsch 1997: 64).

Deutsch points out that the objective of science is not to find a theory that is deemed true forever; it is to find the best theory currently available. A scientific argument is intended to persuade us that a given explanation is the best one available. It cannot say anything about how that solution will fare in the future when it is subjected to a new type of criticism and compared with explanations that have yet to be invented. Deutsch says (1997: 64–5), 'A good explanation may make good predictions about the future, but the one thing that no explanation can even begin to predict is the content or quality of its own future rivals.'

As with the engaged scholarship model, Deutsch points out that the stages of specific problem solving are seldom completed in sequence at the first attempt. There is usually repeated backtracking before each stage is completed. Only when the process is finished does a coherent pattern emerge that reflects the five linear stages of problem solving.

> While a problem is still in the process of being solved we are dealing with a large heterogeneous set of ideas, theories, and criteria, with many variants of each, all competing for survival. There is a continual turnover of theories as they are altered or replaced by new ones. So all the theories are being subjected to *variation* and *selection.* (Deustch 1997: 68)

Deutsch cites Popper for this evolutionary epistemology. However, he cautions not to overstate the similarities between scientific discovery and biological evolution, for there are important differences. One difference is that biological variations (mutations) are random, blind, and purposeless. In human problem solving, the creation of models or theories is itself a complex, knowledge-laden process driven by human intentions. Perhaps an even more important difference is that there is no biological equivalent of *logical reasoning* and *argument*. The stronger the arguments for problems and theories, the more influential or persuasive they are. Science, like problem solving, justifies an explanation as being better than another currently available explanation.

Giere's Model of a Scientific Episode

Ronald Giere, a philosopher of science at the University of Minnesota, has been influential in introducing a pragmatic realist epistemology of science. This view downplays the idea that there might be universal natural laws encoded in true general statements. Rather, scientists are seen as engaged in constructing models that represent or fit the world in relatively better or worse ways. It is a kind of realism regarding the application of models to the real world, but it is a realism that is perspectival rather than objective or metaphysical (Giere 1999: 60–1). Giere states,

> My account of scientific epistemology pits one model, or family of models, against rival models, with no presumption that the whole set of models considered exhausts the logical possibilities. This means that what models are taken best to represent the world at any given time depends on what rival models were considered along the way. And this seems, historically, a contingent matter. So the models of the world held at any given time might have been different if historical contingencies had been different. (Giere 1999: 77)

Based on this perspectival realist epistemology, Giere (1997) proposed a model of the scientific episode (or research project) consisting of four components: (1) a *real-world* object or problem under investigation; (2) a theoretical *model* of the real-world object or process; (3) some operational hypotheses or *predictions* derived from the model including a research design of what the data should be like if the model really does match with the real world; and (4) some *data* (or solutions) that are obtained by observation or experimentation with the real world (Giere 1997). Giere arranges these components as shown in Figure 1.3, which correspond closely to the four research activities in the engaged scholarship diamond model. The figure illustrates four important relations.

1. The relationship between the real world and the model is expressed by a conceptual proposition or analogy asserting that the model fits the real-world problem or phenomenon being examined. It is understood that the

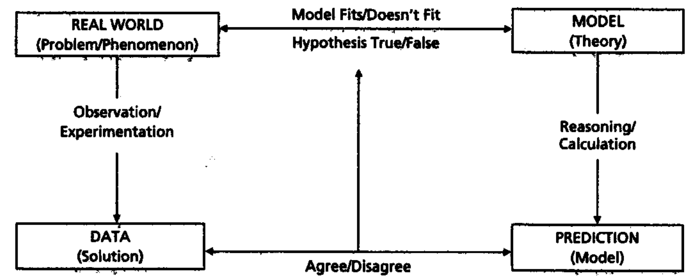


Figure 1.3. Ronald Giere's model of a scientific episode

Note: The activities of ES diamond model inserted into Giere's (1997: 30) figure of the four elements of a scientific episode.

model fits only in some respects and then only to some specified degree of accuracy. If the model does not fit accurately in the intended respects, then the theoretical model is false.
2. The model and the prediction (what we call theory) are related by *reasoning* or *argumentation*. The real world and the data are related by a *physical interaction* that involves observation or experimentation. 'If what is going on in the real world, including the experimental setup (our research design), is similar in structure to the model of the world then the data (solution) and the prediction or theoretical hypothesis should *agree*. That is, the actual data should be described by the prediction. On the other hand, if the real world and the model are not similar in the relevant respects, then the data and the prediction may *disagree*' (Giere 1997: 30).
3. The top half of the Figure 1.3 pictures the relationship between the real world and the model in question. Are the model and the real world similar in the respects under study and to an appropriate degree of accuracy? This relationship is typically not open to direction inspection. The bottom part of the figure, by contrast, pictures a relationship that can be evaluated by relatively direct inspection. Scientists can examine the data and see whether they agree with the predictions derived from the operational theory or model.
4. The left side of the figure illustrates relationships existing between the problem or phenomenon and data obtained from the real-world observations. The data are generated through physical interactions with bits of the real world. The right side of the figure between model and theory, by contrast, consists of conceptual relationships that are mainly symbolic. The model exists mainly as a description of a possible type of object.

Like the four bases of the engaged scholarship model and Deutsch's stages of problem solving, Giere's figure illustrates a fully developed scientific episode containing all four components of a research project arranged to make possible an evaluation of how well a model fits the real world. Giere points out that many scientific reports do not include all four components, and many do not unfold in the deductive, model-testing manner as outlined here.

> It is common for example, to find reports that describe only the part of the real world under investigation together with some new data. There may be no mention of models or predictions. Similarly, we often find discussions of new models of real-world entities or processes with no mention of data or predictions. Occasionally we find accounts of models of real-world things that include predictions but no discussion of data. We can learn a lot from such reports. Unless all four components are present, however, there may be nothing we can subject to an independent evaluation. (Giere 1997: 31)

Conclusion

You may wonder if engaged scholars in professional schools should conduct more applied and less basic research? The answer depends on the research question and perspective taken to study a problem domain. As Figure 1.2 illustrates, engaged scholarship can be practiced to study a variety of basic and applied questions. Engaged scholarship represents a 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 can bring forth on a problem, engaged scholarship produces knowledge that is more penetrating and insightful than when scholars or practitioners work on the problem alone. More specifically, the quality as well as the impact of research can improve substantially when researchers do four things: (1) confront questions and anomalies arising in practice; (2) organize the research project as a collaborative learning community of scholars and practitioners with diverse perspectives; (3) conduct research that systematically examines alternative models pertaining to the question of interest; and (4) frame the research and its findings to contribute knowledge to academic disciplines, as well as one or more domains of practice.

Simon (1976) argues that significant invention in the affairs of the world calls on two kinds of knowledge: practical knowledge about issues and needs from the perspective of a profession and scientific knowledge about new ideas and processes that are potential means for addressing these issues and needs. Historically invention is easier 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 for their clients. We also 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 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. Tranfield and Starkey (1998) point out that researchers may 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 likely to be 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 single 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 co-production of knowledge. As Simon (1976) 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 have often been initiated by problems and questions posed from outside the scientific enterprise (Rattan 2001). 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 knowledge for science and practice, but it may dissolve the theory–practice gap.