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Introduction to the New Edition

Fourteen years have passed since we at the Center for Effective Organizations (CEO) at the University of Southern California held the conference on doing research that is useful for theory and practice that resulted in this book. We assembled a number of major researchers whose work had influenced practice, to examine the many issues that surface in trying to do research that is useful for the dual purposes of advancing theory and practice. This book was intended to focus attention on and generate interest in this kind of research and, it was hoped, to start a constructive dialogue in the field.

Enough time has passed since that conference for progress to have been made in this area. This new edition provides an opportunity to see whether things have changed and where they seem to be headed.

The original preface serves well to introduce and summarize the book; therefore this new introduction first looks at how the book was originally received and briefly presents some of the issues that reviewers raised at the time. Second, it examines each of these issues in the context of what has happened in the field since the original conference and reflects on our experiences at CEO. Finally, we make some suggestions for future consideration.

A Review of the Book

Reviews of and comments about the book have been generally positive. Most people like the format at the core of the book: chapter by the researcher(s), response and commentary by a colleague, and partial transcript from the group discussion that followed. The chapters themselves are quite personal statements by the researchers in response to questions they were asked beforehand (see the Appendix) about their own values, approaches, and contributions. In fact, as one reviewer pointed out, the reader will greatly benefit by reading the questions in the Appendix before reading the chapters (Conover, 1986). The commentaries and especially the discussions consequently build on the personal as well as the methodological and conceptual nature of the research endeavor. If nothing else, it is a refreshing change of pace to consider these important issues at a level a little less abstract and academic than the usual scholarly tome. The sometimes conflicting interests and values of the participants are apparent in the discussions. Rereading them still evokes the same visceral reactions we felt during the conference. Many have commented to us that the discussions are their favorite parts of the book. Because the authors responded to a set of questions around a relatively focused topic, the book hangs together much more than the usual book of readings from a conference (Conover, 1986). There is an interplay among the chapters enhanced by the discussions. The beginning chapters by Lawler and Mitroff and the ending chapters by Goodman, Mohrman and others, and Bennis further integrate the book by pointing out common themes, emergent issues, and work to be done.

On rereading the book and the various reviews of it, we find that there are several issues around which a kind of social convergence of interest (though not necessarily agreement) exists; each of these issues has currency in academic discussions today.

The first issue relates to the *value and legitimacy* of the notion of doing research that is useful for theory and practice. We convened the conference and published this book explicitly for the purpose of legitimating the area of interest. CEO is a university-based research center created four years prior to the conference (in 1979) to carry out research that is theory based and that advances theory and practice. We feel it is the only kind of research that invites continued funding by corporations while still reaching standards of rigor that our colleagues in the organizational sciences can see as legitimate. As can be seen in Chapter Eight, our interest in dual-focused research stems from personal as well as philosophical values. The creation of CEO was also a strategic move by our business school to create a more meaningful connection between the knowledge it generates and the business community, thus contributing to that community's perceptions of the legitimacy and value of the scholarly work of the school.

Certainly the contributors to the volume had and still have the stature in the field of organizational studies to confer legitimacy to the topic, and most felt strongly that they wanted their work to impact practice. Through the years, we at CEO have benefited greatly from the insight gathered from this diverse group. The reviews agreed that the book makes a good case for the importance of doing research that is useful for theory and practice. Even with this relatively supportive cast of characters there is still a strong tension in the book reflecting an even stronger tension in the field at the time. And because, as we shall see, legitimacy issues are still with us in the field, the book is still of value.

The second issue is concerned with *definitions.* What do we mean by such terms as *research, useful, theory,* and *practice?* For that matter, what is meant by the phrase "doing research that is useful for theory and practice"? Reviewers (for example, Englehart, 1986) as well as contributors, especially Goodman (Chapter Nine), noted the general tendency among the authors not to define terms precisely; they rely on common understandings or leave the definitions to be inferred. Defining these ideas is not a trivial problem and has implications for what one has to say about them. Publications in the field still tend to use these terms without defining them. The book provides a nice springboard for discussing these definition issues.

As with most academic convocations about these topics there is an inevitable tendency for the discussion to be drawn toward the issues of *philosophy* and *worldview.* The topic demands these considerations, of course, and the book gives them their due. The contributors are extremely forthcoming about their philosophies and views of the world. Rather than polarizing, this openness greatly facilitates the discussion. When philosophies of science take center stage in this book, these issues always are dealt with in context. Personal preferences and values, organizational realities, beliefs, career interests, methods, concepts, models, institutional settings, and philosophies are all considered as pieces of the whole endeavor. Because of this approach, the book has much to say about philosophy of science without becoming fixated on it. The basic tension noted by the participants and the reviewers of the book is between whether dual usefulness to theory and practice can be achieved by traditional, positivistic science—seen to be the dominant paradigm in organizational science at the time (and seen by some participants as destructive of doing research that is useful for theory and practice)—and whether it requires an emergent more compatible paradigm. Philosophical tension still exists in the field. Because of its grounded nature, this book puts the philosophy in context and actually offers the reader progress on these issues.

Closely associated with the issue of philosophy is the issue of *method.* On the surface this is evidently the core issue of the book, and it has many facets. "Traditional" methods are seen by some as perpetuating the status quo, not offering rich enough data to be useful, not considering the context in which phenomena occur, distancing the researcher from the subject, and being biased toward control. Some see these attributes as jeopardizing all types of usefulness. Others are not so sure. Several of the participants question throwing methodological babies out with their philosophical bathwater. Both Goodman and Bennis point out that much of the dualism between "traditional" and "nontraditional" approaches portrayed throughout the book is overdrawn and that there is more common ground than acknowledged by some of the participants. Often, it seems, the authors are not so much philosophically opposed to certain methods—they simply find them inadequate to the task of developing useful theories and informing practice.

Two dimensions of method that the contributors raise to heightened awareness are (1) the nature of the relationship between the researcher and the research "subjects" and (2) the nature of the institutional settings in which both the researcher and research subjects exist. Many feel that the relationship should become collaborative, or at least that of researcher and "client," to facilitate transfer of research-based knowledge. Others are not so sure, feeling that the usefulness of results can actually be enhanced by some professional distance. Similarly, a change in the relationship between researchers and subjects would necessitate changes in their institutional settings. Academic settings would have to change their reward and career structures, and research endeavors might begin to be structured as joint ventures, for instance.

The final issue to emerge from our revisiting of the book and its reception has to do with the degree to which the book provides sufficient *grounding* of the phenomenon of doing research that is useful for theory and practice. One of our purposes for the book was not just to explore methodological and philosophical issues but to ground them in the experiences and careers of the contributors. Nevertheless, the grounding is not complete and apparently created an appetite for more. Despite the degree of personal experience that the authors drew upon and described, some reviewers were disappointed at the lack of sufficient introspection into actual experiences and specification of the logistical matters that go into doing the research described. Although they felt that the conference and its participants were on the right track, for some we did not go far enough. This is a very important point. To recognize the need for grounding is to recognize that doing research that is useful for theory and practice is subject as much to empirical scrutiny as it is to philosophical analysis.

In the next sections of this introduction we look briefly at some of the more recent developments in organizational studies on each of these five issues. As an illustrative case in point that grounds these general developments, we describe our own experiences and the institutional arrangements by which we have been doing research that has been more or less useful for theory and practice.

Value

The idea of doing research that is useful for theory and practice is explicitly value laden. Both individually held and institutionally embedded values are relevant to a discussion of whether and how this research should be accomplished. In general, practitioners may primarily value the practical side of research, academics the theoretical.

There are many facets to the values question. One is the issue of the legitimacy of doing research that is useful for theory and practice in the various relevant communities. We held the conference resulting in this book because we felt that this pursuit was not seen as academically legitimate by many in the academic community. We wanted to explore the interface between theory development, knowledge creation, and practice, and to enhance the legitimacy among academics of work that attends to both focuses. Similarly, academic research is often dismissed out of hand by practitioners as being irrelevant to them and not a legitimate way of attending to their needs; many explicitly devalue theory and especially the jargon that tends to accompany it. We hoped to shed light on what would be required for theory-enhancing research to be experienced as useful by practitioners, and for participation in dual-focused research to be valued and seen as a legitimate and practical activity.

Academic Focus

When we held the conference, the prevailing state of affairs in academia was not supportive of practically useful research. In fact, the academic community tended to devalue the activities entailed in building bridges between theory and practice, other than through traditional classroom teaching in professional schools. Since that time there seems to have been a very slow change in orientation, as organizations have realized the importance of organizational and managerial knowledge in today's competitive environment, and as more academics have increasingly built ties to the world of practice, whether because of their values or for pragmatic reasons. Practitioner organizations, such as the Society for Human Resource Management and the Human Resource Planning Society, have begun to sponsor research. Company consortia have been created to investigate issues such as how to organize for new product development; in many cases they have partnered with academics and sponsored relevant academic research. There has been an increase in participation by academics in executive education programs that stress relevance and the use of case and experiential methodology that demands a melding of practice and theory. The bridging of research and practice is also reflected in a seeming increase in the respectability of publishing academic research in practitioner-oriented journals. The establishment of *The Academy of Management Executive* is one manifestation of this change.

There has also been a proliferation of research centers created to be hospitable institutional settings for carrying out research with a dual focus. In the United States, besides our own center at USC, such schools as the University of Michigan, Boston University, Carnegie-Mellon, Cornell, University of Illinois, MIT, and Farleigh Dickinson have established or strengthened such research centers. Internationally, such schools as the University of New South Wales in Australia and the University of Warwick and the University of Sheffield in Great Britain have centers and institutes that conduct programmatic, problem-focused, academic research into issues of high import to organizations. Most of these centers establish a working relationship of some nature between interested academics and practitioners.

Other indicators of academic focus on the practical usefulness of research are periodic conferences or journal issues examining the issue. The theme for the thirty-ninth annual meeting of the Western Academy of Management in 1998 is "Turning Research into Results, Theory into Practice." A recent special issue of the *Journal of Applied Behavioral Science* (Bailey and Eastman, 1996b, p. 350) focused on science and service (the editors' term for "the practical intention of converting scientifically generated knowledge into useful managerial prescriptions"). Although most of their discourse was at a rather abstract level and more focused on philosophical issues than on what it would take actually to do this kind of work, most contributors saw value in the joint pursuit of science and service. One of the most recent handbooks in the field (Clegg, Hardy, and Nord, 1996) devotes almost one third of its chapters to the area of research, theory, and practice.

In his recent presidential address at the annual meeting of the Academy of Management, Mowday (1997) pointed out that business schools and management scholarship are under threat from two sources: competition from alternative sources of scholarship, and their own increasing irrelevance to organizations in an environment of change. He called for a broadening of the accepted notion of scholarship in the management and organizational sciences to include application and practice as well as research, insightful synthesis (theory), and teaching. He also reminded his readers that the primary objective of the Academy of Management according to its constitution is "'to foster the general advancement of research, learning, teaching, and practice in the field of management and to encourage the extension and unification of knowledge pertaining to management'" (Mowday, 1997, p. 341). Mowday was only the most recent of academy presidents to call for the academy to live up to this objective (compare with Hambrick, 1994).

Although academics involved in some of the aforementioned research centers and forums obviously value some form of doing research that is useful for theory and practice, Mowday clearly feels that this is not the preponderant value orientation among management scholars and that the academy is therefore at risk by being out of synch with its environment. Earlier, Mowday (1993) made an observation that reflects on the other part of the dual focus, that is, whether research is relevant to theory development. He noted that most articles in the *Academy of Management Journal* were never subsequently cited, and wondered, "Although we have grown accustomed to criticism that our research is largely irrelevant to managers . . . what does it tell us if our research is becoming irrelevant to our peers?"(p. 105).

Although in many ways management and organizational sciences have matured substantially in the years that have passed since the publication of *Doing Research That Is Useful for Theory and Practice*, this maturation may have resulted in an academic field in which less importance is placed on the useful side of research, whether theoretical or practical. A clear hierarchy of journals has developed within the field, and, increasingly, universities promote faculty members on the basis of the number of articles they have had published in the higher-ranked journals. Although the research in these journals places increasing emphasis on theory and method, there is very little concern with its usefulness to practitioners. What is accepted as good research is often a study that is methodologically sound and draws on theory but is merely the next in a long string of studies that tests and elaborates upon an academically interesting theoretical issue. In some cases, the issue initially had some practical implications, but as studies increasingly dissect and define the issue, they both cease to be relevant to the important practical issues raised by the theoretical point and lose the essence of the theory (see Hackman's comment, p. 162, this volume). Another phenomenon is the scholarly examination of current issues through theoretical lenses in articles that are interesting to read but lead to little follow-up testing or theory advancement, let alone to an understanding of the practical implications. Likewise, there is a proliferation of putting "old wine in new bottles"—examining the same phenomenon through different theoretical frameworks, applying new terms to the same phenomenon, and in many ways precluding the very dialogue and building that would be required to make theoretical and practical progress. This point applies not only to academics but to practitioners who tend to proliferate management "fads" under different titles, each carrying its own jargon that disrupts continuity of practice across fads.

Our journals and our tenure and promotional systems are certainly under critical attack on various fronts. These criticisms are not unheeded. Journal editors (Mowday, 1993; Hitt, 1995) and potential system reformers (for example, Bernardin, 1996) have enumerated them and offered various fixes. Criticism that our journals and tenure systems engender research that is irrelevant to managers and organizations comes from a number of sources. Hitt (1995) is relatively optimistic about the potential usefulness to practice of articles in the leading journals. He sees the problem as lying in their inaccessibility to practitioners. For him, "our research process has operated much like a closed system" (p. 55). It is closed precisely to those who could use it to practical benefit. He would involve practitioners more directly in the research process.

Ironically, Bernardin (1996) found that faculty in management and organization tend to look overwhelmingly to leading journals like the *Academy of Management Journal* as the most important sources of knowledge advancement. This is true even though, according to Mowday (1993), only a small percentage of the articles actually contribute to cumulative knowledge advancement or even get cited. In his attempt to strengthen the knowledge advancement contributions of these leading journals, Bernardin would create a performance management system for faculty that would focus their research energies on contributing only to those leading journals to which they look for knowledge advancement. Ironically, Bernardin's proposal would strengthen the very closed nature of the system that Hitt finds problematic. Bernardin uses the fact that academics already pay attention overwhelmingly to academically defined research to justify paying attention solely to academically defined research.

Practitioner Interest

While this debate proceeds, it seems clear that practitioners have become hungry for good solid knowledge about organization and management. One danger is that the tightening of methodological standards has limited the kinds of issues that can be examined, and has consequently limited the potential usefulness to practice of the knowledge that is generated. Sloppy issues sometimes can't be studied with precise methodologies and measures, and thus many interesting issues are not researched by the academic scientific community. One need only look at the best-seller list with respect to business books to understand, first, the huge market for management knowledge and, second, the kind of knowledge practitioners are seeking. Specifically, they seem to flock to treatments of the sloppy issues they are facing and to seek knowledge that offers possible ways to navigate these issues. Books on reengineering, for example, do not cite much research evidence: Hammer and Champy (1993), for instance, base their conclusions on their experiences as consultants within several organizations, but beyond that describe no data gathering and analysis. Another best-seller, *Built to Last* (Collins and Porras, 1994), is research based, uses a control group approach, and presents data, but the authors' description of their methodology would probably not pass the screen in an academic journal. In Chapter Nine, Goodman states that different kinds of research would be required to be practically useful in different ways, and points out that experienced-based books such as *In Search of Excellence* (Peters and Waterman, 1983) have had broad impact on how people organize and think.

Although these examples point out the gulf between what is academically and theoretically legitimate and what has recently been seen as useful by practitioners, they also point to the tremendous appetite for relevant research. We and our colleagues (Galbraith, Lawler, and Associates, 1993; Lawler, 1992) have argued that the way a business organizes is a key competitive advantage for that organization, but in order to take advantage of how it organizes, the organization must have valid information about what works and what doesn't work, and sound explanations for those results. Such data remain relatively hard to come by. Many of the studies assessing such practices as reengineering, the use of outsourcing and other design approaches, and a whole host of other ideas about how to organize come from consulting firms doing studies intended to develop and market their products. These studies tend to be neither theory based nor methodologically sound. Typically they consist of a quick survey of what companies are doing, and the results are published as findings of "best practice," with little attention paid to assessing the effectiveness of practice other than through the use of testimonials and managers' impressions.

In part this situation exists because practice is increasingly changing more quickly than academics can develop theory to inform it. As more and more businesses have recognized that organizational effectiveness is a key competitive advantage, and as they have confronted environments demanding quite different levels and kinds of performances, they have pushed ahead practice at a rate that has far outstripped both theory and research. The rapid movement of practice has put academic theory and research at increasing risk of irrelevance. All too often, practitioners find the issues studied in academic research "old" and irrelevant to the practices they use and the issues they face today. For example, academics started collecting systematic data and writing about quality management approaches many years after these approaches were being widely used in organizations. At the time of this writing, two documents crossed our desks on the same day: one, a *Business Week* article (Byrne, 1997) claiming Total Quality Management to be dead as a management approach; the other, a program description from the National Science Foundation (n.d.) to research transformations to Total Quality Management. This kind of gap, whether perceived or real, suggests that academics *must* get into organizations and try to understand and do research in terms of the way organizations are actually being run. It may even be that part of the perceived demise of TQM is due to a lack of timely research relevant to its application.

Thus, there seems to be a true values gulf: there is a growing appetite for timely, useful research, and still a reluctance in the academic community to engage in it. This book argues that theory and practice are not competing mistresses. We still believe it is possible to do research that is useful to both; indeed, it is more important than ever to do such research. We are a bit discouraged, however, that this challenge has not been taken up by more academic researchers and that the institutional settings in which the majority of academics work do not treat relevance to practice as a criterion for good research.

Bridging the Gap

Among the academics who do value doing research that is useful for theory and practice there are many motives. Some want people in the practitioner and academic worlds to listen to them, to pay attention to what they have to say. Some desire to carry out a "service" obligation that they believe academics have to society; others simply want to have a positive influence on society. Some see dual-focused research as the philosophically defensible approach; they want to advance knowledge about organizations and feel this is the best way to do it. Some wish to get corporate monetary support to do the research. Some believe dual-focused research is required in order to justify the existence of academic organizational research to the university, to the government, to funding agencies, to society, and maybe even to themselves. Often these motives overlap each other.

In various ways, each of the motives just mentioned are part of why we at CEO value doing research that is useful for theory and practice. In the years since the conference our center has been working to carry out the kind of research described in Chapter Eight: research that addresses both theoretically and practically important questions and that is carried out in partnership with the organizations we study. We established the center as a way of matching corporate funding and organizational research and recognized that such an endeavor required the bridging of the values in the academic and practitioner communities. From the beginning we involved corporate advisers in the design of the center. Regular interaction with our corporate sponsors has enhanced our understanding of the challenges they are experiencing and has helped us anticipate issues and cast our research in useful ways.

It quickly became clear that corporations were not going to fund specific research or sponsor our center unless they derived immediate use from the relationship. So far we have survived and grown through corporate support, although this has taken our constant attention and has required us to allocate time and effort expressly to meet organizations' needs and earn their support. We have taken pains to write up our research in ways