Susan Albers Mohrman, Edward E. Lawler, III, and Associates: Useful Research: Advancing Theory and Practice. San Francisco: Berrett-Koehler, 2011. 456 pp. $49.95, hardcover.

''Theory is when you know everything but nothing works. Practice is when everything works but no one knows why. In this room, theory and practice come together: Nothing works and no one knows why'' (p. 103). Hackman's reference in chapter 5 to Herb Kelman's door quote sums up what is often seen—especially for organizational scholars in business schools—as a fundamentally intractable dilemma between rigor and relevance. Yet our most mature theories are often the most contextualized and therefore most practical ones. Theory is useful because it is applicable beyond a particular case, while practice is a particular case. This apparent incompatibility is resolved when theory properly specifies key contextual components necessary to qualify applicability, and appropriate theories are applied to understanding particular cases. Conversely, theories can ''learn'' through feedback from particular cases. Mohrman and Lawler's 25-year sequel compilation, Useful Research, champions and demonstrates practical organizational studies research through rich examples of collaborations between researchers and practitioners, career narratives, tangible approaches, and environmental evaluations, nestled among cogent commentaries and frameworks.

Following Mohrman and Lawler's introduction, the book opens with rich descriptions of practitioner collaborations that had practical and theoretical impact. Edmondson (chapter 2) offers three ''attributes of [her] approach,'' namely, ''starting with an important problem, getting into the field (early and often), and not being afraid to collaborate across disciplinary and organizational boundaries.'' Sue and Allan Mohrman (chapter 3) talk about experiences at USC's Center for Effective Organizations (CEO)—which was responsible for the publication of Useful Research—including how to unify multiple, heterogeneous interactions with practitioners to form a coherent research stream. Chapter 4 (Gratton) covers some of the grittier details of achieving ''buy-in'' that so often discourage researchers and practitioners. Hackman (chapter 5) asserts the need to connect theory, phenomena, and empirical research—while conceding that ''it is probably asking too much to expect that any one researcher, or even any one research group'' tackle all three legs (p. 109). As many of the other contributors conclude, the field needs to increase its relevance, though each researcher's contribution will vary.

Does rigor necessarily conflict with relevance? Several researchers trace their career paths while discussing this tension. Mirvis and Lawler (chapter 6) conclude their engaging career narrative by positing that rigor and relevance are

likely two distinct domains. The late C. K. Prahalad related how he pursued ''next practices'' (versus ''best practices'') by looking for ''weak signals.'' His emphasis on ''core principles . . . independent of the data from which they were derived'' illustrates the importance of constructing ''theory'' (our term) beyond particular cases. Beer (chapter 8) emphasizes how practitioner-focused research can provide a critical complement to traditional research methods, which are in danger of accounting for a negligible amount of the variance and being trapped within a particular theoretical framework. Tushman points out in chapter 9 that ''our field runs the risk of having great answers to less and less interesting problems'' and urges ''Pasteur's quadrant'' research, motivated by both considerations of use and a quest for fundamental understanding. We note that Bohr's research (whose name denotes the quadrant high on theory, low on practice) was not immediately relevant to practitioners, though it ultimately formed the foundation that led to Einstein's path breaking Pasteur-quadrant research. Similarly, while we as individual researchers may not always be in Pasteur's quadrant, at the end of the day, our field ought to be.

Half a dozen subsequent chapters present tangible approaches to bridging research and practice: partnerships with consulting groups (Wageman), executive Ph.D. programs to train ''scholar-practitioners'' (Tenkasi), improvements in communication from scholars to practitioners (Bartunek and Schein; Rousseau and Boudreau), professional associations (Cascio), and management books (Benson). Latham (chapter 16) comments on the section by emphasizing the duty of behavioral researchers to help improve organizational life and suggests several communication tactics to enhance the impact of research on practitioners. A ''Practitioner Perspectives'' panel discussion (Nadler, Ziskin, Lawler, Beer, and Morhman) from the December 2009 book workshop concludes the section.

Cummings (chapter 18) proposes multiple forces leading business schools to become more discipline-based; Rynes (chapter 19) illustrates the counterpoint: that ''applied research'' is valued and valuable now more than ever. The section's concluding chapter (O'Toole) offers a critique of trying to understand complex, overdetermined reality through traditional positivistic research methods. O'Toole reiterates a ''modest proposal'' calling for the ''professionalization'' of business schools—along the lines of law and medical schools—by rewarding and regarding both applied and discipline-focused career tracks equally.

Van de Ven's (chapter 21) insightful commentary includes descriptions of multiple ''engaged scholarship'' categories: (1) informed basic research, (2) collaborative basic research, (3) design and evaluation research, and (4) action/intervention research. According to this framework, even researchers working on relatively abstract organizational topics can enhance relevance by (category 1) soliciting ''advice and feedback from key stakeholders.'' In contrast, extremely applied researchers (category 4) must remember that for ''N-of-1 studies . . . systematic comparative evidence can only be gained through trial-and-error experiments over time'' (p. 391).

Mohrman and Lawler's excellent compilation of frameworks, examples, and advice promises to stimulate further discussion of the challenges and opportunities endemic to research conducted within an ''applied discipline.'' Two broad

questions were especially salient during our reading. First, how scalable is what's being proposed? At an individual level, the list of requirements summarized in chapter 22 (conclusion) for producing research that has an impact on practice is daunting. Thus the dual-accomplished theoretical and practical contributors featured in this volume are particularly noteworthy. Their how-to suggestions are essential reading for those seeking to imprint management practice with their own research. But even doubling or tripling the rate at which our field produces this type of scholarship will hardly make a difference for our collective image or impact. It is difficult to imagine large-scale replication of the featured individual successes and/or ''practical scholarship'' becoming a trait of our field without significant institutional assistance. This makes us wonder what the institutional version of the Mode 2 approach (cf. introduction) would look like for academia. What can we learn from CEOs and a handful of other university-based research centers that are successfully spanning the worlds of academia and practice? What can we learn from profession-level applied research institutes in other fields (marketing) and countries (UK)?

Second, in pursuit of more practical research, is ''theory'' part of the problem or part of the solution? In light of the book's subtitle, our review hints at a surprising lack of attention to ''advancing theory,'' exemplified by the absence of ''theory'' as an entry in the index. There are any number of justifiable reasons for focusing a single volume on only one of the descriptors of ''useful research'' in the book's subtitle (i.e., ''advancing practice''); academic-focused research is likely to be much more theory-based than practice-focused research. Recent criticism of our field's excessive focus on theory is beginning to rival, in intensity and persistence, the concern highlighted in this book about lack of practicality. Though it is tempting to conclude that the key to overcoming both problems is to downplay the role of theory in our research, it is equally probable that *better* theory is at least one key to more practical management and organization scholarship. Kurt Lewin's oft-quoted dictum, ''There is nothing quite so practical as a good theory,'' invites systematic examination of what kinds of theory have proven useful to practitioners and how practice-based scholarship (e.g., grounded theory development) might generate better theory. Such an examination might warrant a subsequent compilation—an equally thoughtful and helpful treatment of useful research that emphasizes theory advancement, in addition to practice. As our field issues consistent calls for increased relevance, incentives and pressures for more rigor continue to mount. Perhaps both relevance and rigor can be achieved through improved theorizing—especially for mature theories—by making explicit the boundary conditions and limitations of theories, uncovering mediators (for practitioners to understand causal mechanisms) and identifying essential moderators—for practitioners to know when particular theories apply, or to borrow Beer's words, ''conditions that must be in place to ensure success'' (p. 148).

Useful Research provides a rich collection of inspiring examples: academically and practically successful scholars. It further offers timely observations of the competing values guiding management and organizational scholarship and the associated challenges facing those who practice this craft and try to influence practice. As colleagues plan research projects, deliberate in department and college meetings, and dream and scheme at professional gatherings, we

hope this book will stimulate further, equally thoughtful, discussion about ''who we are.''

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