

2018

Pragmatic Rigor: Principles and Criteria for Conducting and Evaluating Practitioner Scholarship

Daniel Robey

Georgia State University, USA, drobey@gsu.edu

Wallace T.F Taylor

Georgia State University, wallacethor.taylor@gmail.com

Louis J. Grabowski

Georgia State University, lgrabski@gmail.com

Follow this and additional works at: <https://commons.case.edu/emr>



Part of the [Business Commons](#)



This work is licensed under a [Creative Commons Attribution-Noncommercial 4.0 License](#)

Recommended Citation

Robey, Daniel; Taylor, Wallace T.F; and Grabowski, Louis J. (2018) "Pragmatic Rigor: Principles and Criteria for Conducting and Evaluating Practitioner Scholarship," *Engaged Management ReView*: Vol. 2 : Iss. 3 , Article 2.

Available at: <https://doi.org/10.28953/2375-8643.1053>

<https://commons.case.edu/emr/vol2/iss3/2>

This Essay Papers is brought to you for free and open access by CWRU Commons. It has been accepted for inclusion in Engaged Management ReView by an authorized editor of CWRU Commons.

Pragmatic Rigor: Principles and Criteria for Conducting and Evaluating Practitioner Scholarship

Cover Page Footnote

We are grateful for the comments by the senior editor and two anonymous reviewers. Comments on an earlier draft from Shahla Ghobadi, Michelle Kaarst-Brown, Christine Abdallah Mikhaeil, Alina Dulipovici, and Marie-Claude Boudreau are also acknowledged. We also appreciate the feedback received from associates of Georgia State University's Center for Engaged Business Research, especially Todd Maurer, Pam Ellen, Wes Johnston, and Lars Mathiassen. Finally, we are grateful to the executives, clinicians, and board members of The Taylor Foundation for the Arts and Sciences, especially Pornpimol Kodsup, Denisa Millette, and Ana Perez. An earlier version of this paper was presented at the 2018 Engaged Management Scholarship Conference, September 2018, Philadelphia, PA, USA.

EDITORIAL NOTE

The article presents an ideal case for an article in EMR's Essays on "*Engaged Scholarship Debate*." It addresses an important and nagging problem faced by practitioner scholars- how do we evaluate the quality and value of an intellectual contribution that aims to improve scholarship and impact practice at the same time. Most academic journals focus on theoretical or methodical rigor that addresses the concern for the validity of the inferences around evidence or towards some theory. Practitioner-scholarship asks in addition to what extent the inferences and produced knowledge has the potential to impact concrete settings and improve it given the stakeholder's goals and constraints. These requirements are in addition to those of academic or theoretical or methodological rigor. The article proposes four principles of pragmatic rigor based on a diligent review of extensive literature. These are relevance, actionability, comprehensibility, and ethical reasoning, and each is associated with a set of concrete criteria for conducting and evaluating this aspect of practice oriented research. The authors also show that these principles are relevant through the overall research process from the choice of topic to final evaluation by journal reviewers. This is to my knowledge the first article of this kind that offers in a structured manner a carefully culled set of principles to evaluate practitioner-scholarship based reporting. I do hope that faculty and students in the EDBAC programs use this article as a starting point to discuss carefully what they should do to evaluate rigorously the outputs of their programs. We at EMR will adopt these principles to our heart and seek to promote them in our future review processes. I hope that all readers of EMR who have an interest in practitioner-scholarship enjoy reading this manuscript as much as I did.

Kalle Lyytinen

Pragmatic Rigor: Principles and Criteria for Conducting and Evaluating Practitioner Scholarship

Daniel Robey

Georgia State University

Wallace T.F. Taylor

Georgia State University

Louis J. Grabowski

Georgia State University

ACKNOWLEDGEMENTS

We are grateful for the comments by the senior editor and two anonymous reviewers. Comments on an earlier draft from Shahla Ghobadi, Michelle Kaarst-Brown, Christine Abdallah Mikhaeil, Alina Dulipovici, and Marie-Claude Boudreau are also acknowledged. We also appreciate the feedback received from associates of Georgia State University's Center for Engaged Business Research, especially Todd Maurer, Pam Ellen, Wes Johnston, and Lars Mathiassen. Finally, we are grateful to the executives, clinicians, and board members of The Taylor Foundation for the Arts and Sciences, especially Pornpimol Kodsup, Denisa Millette, and Ana Perez. An earlier version of this paper was presented at the 2018 Engaged Management Scholarship Conference, September 2018, Philadelphia, PA, USA.

ABSTRACT

Practitioner scholarship is a promising avenue for addressing the gap between academic research and practice. To advance the objective of publishing the findings of practitioner scholarship, we develop the concept of pragmatic rigor, which is intended to complement but not replace scientific rigor. We propose four principles of pragmatic rigor: relevance, actionability, comprehensibility, and ethical reasoning. For each principle, we develop associated criteria for conducting and evaluating practical research. Pragmatic principles are relevant to the research process, from choice of topic to final evaluation by journal reviewers. We believe that applying these principles can advance the practical value of studies and help to bridge the gap between scholars and practitioners.

INTRODUCTION: THE RELEVANCE GAP

Academic management programs have long sought to be relevant to practice. For the first half of the twentieth century, business school curricula developed along the lines of trade schools, with a strong emphasis on learning from practical experience. By the late 1950s, this emphasis on practice prompted concerns about the academic rigor of business programs and led to commissioned reviews by the Ford Foundation and Carnegie Corporation (e.g., Pierson, 1959). These reviews called for more academically rigorous curricula, advocating an increase in the number of doctoral-qualified faculty and more demanding coursework for students. In partial response to these recommendations, business schools began placing greater emphasis on scientifically rigorous research in management, and less reliance on the experience of practicing executives (Clinebell & Clinebell, 2008). Advanced theorizing, along with increasingly sophisticated empirical methods, became the focus of established peer-reviewed journals, such as the *Academy of Management*

Journal, and new journals such as *Administrative Science Quarterly* and *Academy of Management Review*. A positive result of this shift was an elevation of the status of management scholarship in major universities, and they achieved respect equivalent to the basic sciences, humanities, and applied fields, such as engineering. On the downside, management scholarship became challenged to demonstrate that, in addition to being scientifically rigorous, it remained relevant to the actual practice of management. This so-called “relevance gap” has persisted for at least the past 60 years, without reaching a satisfactory resolution.¹

Academic leaders are discernibly self-conscious about the gap between academic research and practice. For example, at the 1993 Annual Conference of the Academy of Management, Academy president Donald Hambrick offered the following blunt self-criticism:

Each August, we come to talk with each other; during the rest of the year we read each others’ papers in our journals and write our own papers so that we may, in turn, have an audience the following August: an incestuous, closed loop (Hambrick, 1994, p. 13).

And as Academy president Tom Cummings remarked 13 years later:

...few of us truly believe that practitioners really listen to us, and, if they do, they sure don’t seem to be doing much with what they’ve heard. So, the “relevance ghost” continues to haunt us from one conference to another, from one presidential address to the next (Cummings, 2007, p. 356).

Various potential solutions for bridging the gap between academic research and practice have been proposed, including evidence-based management (Pfeffer & Sutton, 2006; Reay, Berta & Kohn, 2009; Rousseau, 2006); executive professorships

(Clinebell & Clinebell, 2008); joint academic–practitioner forums (Bartunek, 2008); improving academic–practitioner knowledge dissemination (Wolfberg & Lyytinen, 2017); executive education forums and changes in doctoral programs and faculty development (Tushman & O-Reilly, 2007); executive doctoral programs (Anderson et al., 2015) and engaged scholarship (Van de Ven, 2007; 2018); among others (Barrett & Oborn, 2018; Carton & Mouricou, 2017). Although none of these seeks to displace traditional academic research, each strives to address the persistent gap between research and practice.

An attempt to bridge the gap also is reflected in a shift by the Association to Advance Collegiate Schools of Business (AACSB) in its accreditation standards: Business schools now are required to provide evidence not only of academic quality but also of engagement, innovation, and impact. This evidence includes demonstration that faculty members fulfill a range of roles, including “scholarly academics,” “practice academics,” “scholarly practitioners,” and “instructional practitioners,” based on their academic preparation, professional experience, and sustained engagement with practice (AACSB, 2018).

Each of these proposals might help to narrow the gap; however, the chasm between academic research and practice has yet to be bridged completely. In fact, skeptics argue that bridging the different worlds of academia and practice cannot work (Kieser & Leiner, 2009; McKelvey, 2006). Bartunek and Rynes (2014) outline the dialectic forces and resulting tensions associated with the academic–practitioner gap, including logics, time dimension, communication styles, rigor and relevance, and differing interests and incentives. Indeed, at the heart of the divide might be the incentives for professors to publish in top-tier academic journals. Without premier journal publications to list on their vitae, they are unlikely to meet the minimum perfor-

¹ Van de Ven and Johnson (2006) claim that the rigor–relevance debate dates back more than 100 years.

mance requirements to advance in their careers. Moreover, top-tier journals rarely publish articles that are directly targeted to practitioners (Straub & Ang, 2008),² and articles selected to win “best paper” awards tend to emphasize theory, not practice (Ghobadi & Robey, 2017). If practice and relevance were truly valued in academia, top journals would publish more accessible articles and grant more awards for articles that inform practice.

In their own defense, scholars in academia frequently invoke the claim that “there is nothing so practical as a good theory” (Van de Ven, 1989). Not surprisingly, this glib assertion largely goes unchallenged by academics, who rarely construct theories with practice in mind. Meanwhile, even academics who advance practical solutions, such as evidence-based management, admit to its promise (Rousseau, 2006) while producing little demonstration of its actual value (Reay et al., 2009).

Our purpose in this essay is to offer a novel approach to complement existing solutions for improving the relevance of business research while maintaining its scientific rigor. We propose the application of explicit principles throughout the research process—from the initial choice of study topic through to its evaluation by editors and reviewers. We develop the concept of *pragmatic rigor*, which we define as *the adherence to principles and criteria throughout the research process that reflect the practical and social value of a research report*. We assume that business research can achieve the goal of reaching practice more successfully when researchers are guided by standards to ensure pragmatic rigor. We develop a detailed and multi-dimensional concept, including guiding principles and criteria for evaluating the pragmatic rigor of scholarly work. Our aim is to promote the cause of publishing research that meets both scientific

and pragmatic criteria, thereby helping to bridge the gap between academia and practice.

The term “pragmatic” has both a colloquial meaning and meanings rooted in the philosophy of science. Pragmatic philosophy justifies the truth and value of knowledge based on its practical usefulness and ethical consequences (Wicks & Freeman, 1988), and the notion of pragmatic rigor is consistent with such positions. Contemporary versions of pragmatism identify three principles relevant to our interest: “the rooting of habits in agency (constitution), the embedding of action in specific situations and environments (context), and the centrality of causality to inquiry (consequences)” (Lindberg, 2019, p. 4). These philosophical principles distinguish pragmatism from “purer” inquiries into the nature of being and therefore establish a strong base for conducting research that has practical value. In this paper, we do not draw directly from philosophical sources, but pragmatism clearly is the intellectual backbone of our efforts to produce practical guidance to practitioner scholars.

In addition, we emphasize that we are not arguing for rigor and relevance as polar opposites on a single continuum, which might suggest that relevance could only be pursued at the expense of rigor. Rather, we agree with Anderson, Herriot, and Hodgkinson (2001) and with Tushman and O’Reilly (2007), who draw from the work of Donald E. Stokes (1997) to ar-

gue that research is motivated by both understanding and use. Hence, rigor (understanding) and relevance (use) are not opposite poles to be balanced but rather are independent dimensions. Anderson et al. (2001) use these dimensions to identify a matrix of four types of science: puerile, popularist, pedantic, and pragmatic science (see Table 1).

Ignoring the puerile “non-science” type, Tushman and O’Reilly (2007) associate each quadrant with a famous research figure: Thomas Edison with popularist science, Niels Bohr with pedantic science, and Louis Pasteur with pragmatic science. In proposing increased attention to pragmatic science in conducting and evaluating business research, we do not suggest that studies in other quadrants are of little value. Rather, each quadrant has its own set of criteria for evaluation. Popularist science is appealing because it speaks directly to practice and presents workable solutions based on anecdotal experience. Pedantic science can be of great value as contributions to extended lines of theory development. Our advocacy of pragmatic science in business research echoes Corley and Gioia’s (2011) call for “a renewed and reframed emphasis on practice-oriented utility as a focus for future theorizing” (p. 13). As Table 1 indicates, such research should preserve the commitment to scientific rigor to produce findings that are internally valid while also addressing the goal to generate practical knowledge.

Table 1: Types of Science

(Adapted from Anderson et al. (2001), p. 394.)

	High Pragmatic Rigor:	Low Pragmatic Rigor:
Low Scientific Rigor:	Popularist Science	Puerile Science
High Scientific Rigor:	Pragmatic Science	Pedantic Science

² Straub and Ang state: “Our strongly held belief is that articles in *MISQ* [*Management Information Systems Quarterly*] and other top journals should certainly be relevant to practice by virtue of a more pragmatic thematic focus, and they can be judged by that criterion. But they should not attempt to speak directly to a practitioner audience” (2008, p. ix).

PRAGMATIC RIGOR

As a common practice, academics publish principles and criteria underlying a particular style or paradigm of research—especially one that is emergent rather than established. We draw specific examples from the field of information systems (IS) in business schools because it is the first author's primary field and because it historically has sought to be relevant and pragmatic. Principles offer specific guidance to scholars pursuing a particular type of research, while criteria establish standards for evaluating adherence to principles. A research method such as action research might be examined in depth, for example, so that later scholars might receive guidance (Davison, Martinsons & Kock, 2004). Principles are important for emerging disciplines as they seek to achieve legitimacy within the academic community. For example, Straub, Ang, and Evaristo (1994) proposed a set of normative standards for IS research to guide researchers trying to meet scientific criteria for publication. These standards and criteria reflect a primary interest in positivist, quantitative studies, which were emphasized in the early history of IS. Later, Sarkar, Xiao, and Beaulieu (2013) suggested principles for qualitative research in IS, even though such guidance was widely available and often was incorporated into doctoral training. Thus, IS scholars developed resources to guide and support different research paradigms. Klein and Myers (1999) and Myers and Klein (2011) have also articulated principles guiding emerging paradigms of interpretive and critical IS research, respectively.³

Articles that specify fundamental principles and criteria for conducting types of research are valuable in their respective fields because they allow researchers, as well as editors and reviewers, to refer to a common set of standards. Although every principle might not need to be followed by a scholar conducting a particular type of

research study, guiding principles provide a strong basis for designing and reporting studies of that type. Collectively, the principles and criteria establish norms for conducting and communicating research, which in turn improve the credibility, clarity, transferability, and understanding of the research results and their potential consequences. By adhering to such norms, researchers comply with requirements governing the quality of research and decrease the variability of quality in research outcomes. Ideally, research of lower quality can be avoided, ensuring that published findings reflect the best practices of particular disciplines.

To provide comparable value to the conduct and evaluation of practitioner scholarship in management-related fields, we introduce systematic, rigorous guides for conducting and evaluating the practical contributions of research. We propose four principles underlying the concept of pragmatic rigor: *relevance*, *actionability*, *comprehensibility*, and *ethical reasoning*. The principles were derived through a process involving the following broad stages:

- We read a set of widely cited articles about the gap between research and practice. Given the sheer number of articles about the relevance gap in management and related disciplines, we did not conduct an exhaustive search but rather focused on the main arguments set forth in key articles.
- We concluded from this review that research relevance and rigor are both achievable and do not require the sacrifice of one to achieve the other.
- We reasoned that the notion of rigor could be applied to both scientific and pragmatic aspects of a research study. However, we found guidance for achieving pragmatic rigor to be lacking.
- We critiqued the concept of relevance as overly broad without much apparent

effort to establish component dimensions of relevance. We decided to treat *relevance* as a more narrowly defined first principle of pragmatic rigor.

- We developed two additional principles of pragmatic rigor from prior literature on *actionability* (HakemZadah & Baba, 2016a) and *readability* (Straub & Ang, 2008), labeling the latter as *comprehensibility*.
- We added *ethical reasoning* as a fourth principle based on readings about ethical reasoning (Ford & Richardson, 2013), social justice (Rawls, 1999), and social responsibility (Mackey & Sisodia, 2014).
- We refined the specific criteria for each of the four principles.
- We developed a model to explain how the four principles contribute to achieving pragmatic rigor and how they are connected throughout the research process.

The process of derivation was not precisely linear because we received feedback from colleagues and engaged with the literature in greater depth as the specific principles took shape. We presented our initial ideas at a workshop comprising researchers holding executive doctorate degrees and university faculty holding traditional doctorates. A draft version of the paper was later sent to five academic colleagues, who provided detailed comments, and presented at the 2018 Engaged Management Scholarship Conference, where we received additional feedback from an audience consisting of practitioner scholars. Based on the feedback we received in these settings, we refined each principle and eventually settled on a parsimonious set that addresses both non-controversial (e.g., relevance, comprehensibility) and controversial positions (e.g., ethical reasoning).

³ Klein and Myers (1999) was selected as a best published paper by *MIS Quarterly* and by the Association of Information Systems in 2000 (Ghobadi & Robey, 2017).

As mentioned, establishing principles of pragmatic rigor should not be seen as an attempt to replace or supersede established principles guiding scientific rigor; rather, the new set of principles stands alongside the existing principles as distinct criteria relevant to scholarly efforts to reach practice. The principles and criteria for evaluating scientific rigor focus on research design, sampling, measurement validity and reliability, and data analysis. Some established criteria are directed toward practical significance, clarity of presentation to the intended audience, and ethical considerations (e.g., Straub & Ang, 2008; Myers & Klein, 2011). Unfortunately, issues of practical value often are outranked by more dominant criteria of scientific rigor, resulting in rigorous studies that fail to address problems important to business and society (cRRBM, 2017).

Table 2 summarizes the principles, as well as the criteria for judging the range of variation on each principle. Such evaluations commonly ask the rater to indicate the relative strength of agreement with a statement. We word the criteria as concise questions so that they might be more readily adapted by researchers, editors, and reviewers of journals (or other publication outlets) to evaluate the pragmatic rigor of research. We do not envision the pragmatic rigor of a research project to be either present or absent; rather, pragmatic rigor is conceived on a continuum, varying from low to high depending on the answers to the questions posed in Table 2. The principles and criteria are neither mutually exclusive nor exhaustive and might even overlap with established principles of scientific rigor (e.g., comprehensibility). As intended, our contribution serves more as a starting point for further discourse rather than a definitive conclusion about the practical rigor of research.

1. Relevance. *The principle of relevance refers to the connection between research and a problem or set of problems that is judged to be important by stakeholders.* In the literature on the relevance gap, we found many definitions of relevance. For example, Carton & Mouricou (2017) identify four basic

Table 2: Principles and Criteria for Pragmatic Rigor

1. The Principle of Relevance: The strength of the connection between research and a problem or set of problems that is judged to be important by stakeholders.	
DIMENSION	CRITERIA
Topic	To what extent is the topic rooted in an existing practical problem?
	To what extent has the researcher established the significance of the topic to business and other stakeholders?
Research Design	To what extent does the research design strengthen the relevance of the study?
	To what extent does the researcher demonstrate practical knowledge about the research context?
	To what degree are the data generated from involvement in real problem situations?
	To what extent does the research method engage directly with practitioners and other stakeholders as data sources?
Findings	To what extent do the findings relate to the problem context?
	To what extent does the explanation of the findings provide for multiple interpretations?
	To what extent are possible biases and distortions discussed?
Theoretical Basis	To what extent does the theoretical lens help to illuminate the practical aspects of the research question and context?
	To what extent does the theoretical framework fit the nature of the applied problem?
	To what extent are the boundary conditions clearly stated so as to identify the relevant context of the theory?
2. The Principle of Actionability: The extent to which research findings can be implemented in organizations through interventions.	
DIMENSION	CRITERIA
Causality	To what extent does the research indicate cause-and-effect relationships between variables that enable prediction or control of outcomes?
	To what extent does the research show sequential causal links in a process occurring over time?
	To what extent does the research explain the causal mechanisms accounting for the effects of antecedents on outcomes?
Operationality	To what extent does the research make pragmatic recommendations and give practical alternatives that can be implemented?
Usability	To what extent does the research capture the complexity and diversity of the situation and provide directions to manage it?
	To what extent does the study provide a logical set of actions linked to desired outcomes?

Table 2: Principles and Criteria for Pragmatic Rigor (continued)

3. The Principle of Comprehensibility: The extent to which research communicates findings at a level appropriate to the intended audience.	
DIMENSION	CRITERIA
Style	How easily is the study understood without excessive knowledge of technical language, jargon, and acronyms?
	Have long sentences and passive voice been minimized in the writing to ease comprehension?
Format	To what extent is the study supported with visual models, charts, and other elements to enhance comprehension?
	Is an executive summary included?
Audience Awareness	To what extent is the research written using terminology familiar to the intended audience?
	To what extent does the research report make a positive emotional connection with the intended audience?
4. The Principle of Ethical Reasoning: The degree to which the application of research findings considers the range of stakeholders affected and equitably weighs the consequences to all stakeholders.	
DIMENSION	CRITERIA
Social Benefit	To what extent does the study aim to develop knowledge that benefits both business and society more broadly?
Acknowledgement of Stakeholders	To what extent does the study acknowledge and involve a plurality of stakeholder perspectives and interests regarding the business or societal problems studied?
Stakeholder Effects	To what extent does the study consider the effects of the problem and proposed solution on all the diverse stakeholders?

definitions of knowledge relevance in the literature: Relevant knowledge is knowledge that (1) is spread to practitioners, (2) is interesting to practitioners, (3) makes sense to and responds to the major issues of practitioners, and (4) is useful to practitioners. Incorporating concepts from each of these definitions, we chose a narrower definition of relevance as a simple connection between the subject matter of the research and one or more practical and significant issues. Relevance can range from no relevance at all to high relevance. A high degree of relevance should enable stakeholders to use the knowledge to understand phenomena that they manage (HakemZadeh & Baba, 2016b) and should enable practitioners to make more informed choices when implementing

solutions to practical problems (Dodge, Ospina, & Foldy, 2005).

As Table 2 indicates, relevance can be judged using four dimensions and related criteria: topic, research design, findings, and theory. A relevant topic should address actual problems that are considered by stakeholders to be important. This dimension suggests that researchers should engage with (or be) practitioners with relatively deep experience in the problem context. Grounding research in practice is a central idea underlying Van de Ven's (2007; 2018) concept of engaged scholarship. The relevance of a research topic can be judged against specific criteria, as shown in the right-hand column of Table 2. Based on the answers to each question

posed, a research paper can be judged as high or low on relevance of topic.

We also consider *research design*, especially sampling, to be a dimension of relevance. To be more relevant, a research study should rely on data that are generated from settings that reflect the context of the practical problems being addressed by the research. The pragmatic value of research also might be enhanced by drawing comparisons across settings so that variation in outcomes can be inferred from findings. Single case studies typically ground research in a problem situation, while comparative case studies afford greater analytical leverage in explaining the sources of problems and their solutions (Mason, 1996). Surveys and archival data-mining efforts also can be grounded in real problem contexts, but researchers need to be clear about the origins of their data sources. Simply drawing from large databases to "crunch" trace data might obscure the connection between the activities that generated the data and the research problem (Johnson, Gray, & Sarkar, 2019). In addition, to enhance pragmatic rigor, researchers should consider mixed-methods research designs that capitalize on the strengths of different designs and methods. Field studies, surveys, experiments, simulations, and other research designs might be used in a single study to enhance relevance.

Relevance also can be judged by the *findings* of a research study, which should be closely tied to the topic and research design. Relevance demands that the proposed solution address a real problem that the practitioner is facing. Statistical tests of significance might be relevant from a practical standpoint because they establish relationships between causal factors and desired outcomes. Measures of effect size, explained variance (R-square), and variance partitioned to endogenous and confounding variables also strengthen claims of causality, depending on the research context.

The customary reporting of quantitative statistical findings can be made more use-

ful when they are accompanied by narrative explanations, perhaps drawn from ancillary qualitative data. Tables compiling statistical results should be regarded as the basis for a study's findings rather than seen as the findings themselves.

The final aspect of relevance is *theory*. Earlier we questioned the self-justifying maxim that there is nothing so practical as a good theory (Van de Ven, 1989). Theories bearing little connection to problem contexts are not likely to be seen as relevant, and their focus on abstractions and generalities might actually obscure relevance. For instance, a concept such as "time-space distanciation," which might be useful for social theorists' understandings of social and technical interfaces in organizations (Jin & Robey, 2008), would probably be perceived as irrelevant to practicing managers. Theory necessarily includes some level of abstraction so that it is transferable to multiple contexts, but greater abstraction does not enhance theory's immediate relevance for practicing managers.

If researchers draw from the language of practice, they might develop more relevant theory for managers. Although practicing professionals face many problems worthy of scholarly research, addressing these problems might not appeal or seem interesting to researchers steeped in abstract or arcane theory. To illustrate, a concept drawn from various therapeutic practices (e.g., clinical psychology, medicine, physical therapy) is "pain point." Physical pain points negatively affect client function and can be measured (Sullivan, Bishop & Pivik, 1995). Emotional pain points refer to thresholds of frustration that arise as part of human experience. The recognition and effective communication to others of these pain thresholds can lead either to further frustration and hopelessness, if they are unacknowledged or ignored, or to relief and resolution. The concept of pain points can be transferred to many areas of business or clinical practice where obstacles hinder managerial effectiveness and service provision. Theories that guide practitioners in eliminating or resolving

the associated pain points would have both academic and pragmatic value. By theorizing about these various expressions of limits and thresholds, researchers might develop theories more relevant to practice and enhance the pragmatic rigor of their studies.

Pragmatically rigorous theory also needs to carefully define the boundary conditions within which the theory is relevant (Busse, Kach, & Wagner, 2017). Although narrower boundary conditions necessarily limit the generalizability of research findings, the aim of pragmatic science is to produce findings that can be used—not to produce universal covering laws. In other words, "mid-range theory," which has narrow yet clear boundary conditions, should prove to be more relevant to a defined range of practice than "grand theories," which are more generalizable but less directly relevant to specific problem situations. With larger data sets, boundary conditions might be tested more thoroughly to specify the conditions under which various empirical relationships might apply (Johnson et al., 2019).

As Corley & Gioia (2011) point out, theoretical contribution has both scientific and practical dimensions. Hence, in assessing theoretical contribution, we are not arguing for ignoring the scientific utility that improves a concept and its potential to be operationalized. Rather, we advocate the use of scientifically rigorous theory that also has practical utility. The best examples of studies that are both scientifically and pragmatically rigorous are those that fit within Pasteur's pragmatic quadrant, as shown in Table 1 (Stokes, 1997; Tushman & O'Reilly, 2007).

2. Actionability. *Actionability refers to "the extent to which research findings can be implemented in organizations through interventions"* (HakemZadeh & Baba, 2016a, p. 1186). Management research is more pragmatically rigorous if it is actionable. However, substantial evidence suggests that the proportion of actionable research published in top management journals is not only low but also declining. Between

1960 and 2010, the percentage of actionable articles in *Administrative Science Quarterly* and the *Academy of Management Journal* decreased from 65 percent and 43 percent, respectively, to 19 percent and 24 percent (Pearce & Huang, 2012). Increasing the actionability of management research is essential to bridging the gap between industry and academia and therefore is a key principle of pragmatic rigor.

Actionability differs from relevance in that its focus is to help to produce and control outcomes, given a relevant context (HakemZadeh & Baba, 2016a; 2016b). Actionable research has a purpose that is useful in guiding managers toward particular actions and their associated outcomes. Management research that explores a problem or process, or that evaluates causes and effects, might contribute to an understanding of complex business problems. To effect change, however, these types of studies need to be augmented by normative or prescriptive research that demonstrates how to apply findings.

As shown in Table 2, we draw three dimensions – causality, operationality, and usability – from an index created by HakemZadeh and Baba (2016a; 2016b). These dimensions are designed to assess the actionability of evidence-based management. Differences in *causality* assumptions underlie core distinctions in theory, but as Markus and Rowe (2018) emphasize, it is "not possible to reconcile or unify the divergent definitions of causality" (p. 1258). To provide broader applicability, we adopt a more commonly used concept of causality that refers to the "ability to predict outcomes more accurately and create desired results through managerial interventions" (HakemZadeh & Baba, 2016a, p. 1187). To initiate action, managers need to predict what is likely to happen, both immediately and in the future, if they act in a particular way. In addition, executives need to know both the requirements and boundaries necessary to attain a desired outcome. In short, they must have clear knowledge of cause-effect relationships.

To meet this requirement, pragmatically rigorous research should clearly indicate cause-and-effect connections that enable better prediction and control. Statistical methods that attribute the relative contribution of causal variables to an outcome variable are helpful in producing actionable results. Qualitative data analysis might also determine causal inferences using techniques such as qualitative comparative analysis (QCA), which categorizes data across cases (Ragin, 1987). In addition, process causality allows outcomes to be predicted from an understanding of prior sequences of events; in this case, causality is explained with reference to underlying mechanisms that can be inferred from observations made by researchers engaged with a process over time (Mingers & Standing, 2017). For example, longitudinal studies of strategic change can isolate causes of organizational transformation as key events occurring over time. The three criteria for causality in Table 2 represent possibilities for establishing different types of causality, and most studies would need to demonstrate only the type of causality most relevant to their particular research designs.

Operationality is defined as the provision of “pragmatic recommendations that can be readily implemented in practice” (HakemZadeh & Baba, 2016a, p. 1187). Recommendations are operational when they identify activities and choices that a decision maker can actually control. Multiple operational choices might be presented as alternative candidates for action, depending on local situations. For example, comparative case study designs might reveal alternative strategic choices that fit different cases, such as public vs. private enterprises.

Usability. To be useful, solutions must be within executives’ ability to execute (Shrivastava, 1987). Usability testing in the field of website design determines how easily the average user interacts with a portal design. In general, usability engineering applies the principle “that a person of average (or even below average) ability and experience can use the thing

– whether it’s a Web site, a fighter jet, or a revolving door – for its intended purpose without getting hopelessly frustrated” (Krug, 2006). The criterion of usability can be adapted for practitioner scholarship to increase its actionability. To meet this condition, researchers might offer potential solutions that can feasibly be implemented, given legal, financial, and other real-world constraints. These provisional solutions would enable managers to consider what is useable and what is not.

Studies that report actions taken, along with their results, offer the best examples of the principle of actionability. These studies might include, but are not limited to, action research studies and field experiments in which interventions are designed and implemented. Longitudinal case studies also can report on actions taken over time, along with an assessment of these actions’ consequences.

3. Comprehensibility. *Comprehensibility refers to the extent to which research communicates findings at a level appropriate to the intended audience.* Although clear writing is emphasized in academic scholarship (Straub & Ang, 2008), the style and format used in communicating research findings often are incomprehensible to an audience of experienced practitioners. As a result, the pragmatic value of important research findings might never be apparent. Three dimensions of improved comprehensibility in research are proposed: style, format, and audience awareness.

Style refers to a study’s presentation using language that is likely to be understood by those who are not familiar with technical terminology, jargon, acronyms, foreign-language phrases, and other unnecessary obstacles to comprehension. Comprehensibility depends on shared language and shared frames of reference between the domains of research and practice. Because scholars in academia develop linguistic conventions that are not normally found in practice, research often is incomprehensible unless the reader is “bilingual” (Isaacs and Trofimovich, 2012) and able to translate academic concepts

into practice. Because academic meaning is conditioned by a university culture, words that are used in this context might not connote outside of it a more broadly accepted meaning. Therefore, practitioner research needs to be expressed lucidly, using linguistic conventions familiar to the world of practice.

Format refers to the design of research documents and reports. Research reports should be well organized, easily accessed, and engaging so that they can be navigated more easily by executive readers. Also, information often is conveyed more effectively when words are accompanied by visual aids, such as pictures, graphs, charts, and tables. Ironically, visual aids sometimes are perceived as detracting from scientific rigor. However, creatively and well-designed visual aids communicate essential content that can enhance comprehensibility without sacrificing pragmatic rigor.

Audience awareness refers to the general criterion of writing for the intended readership. This criterion is implicit in the principle of relevance, discussed earlier, insofar as material that is irrelevant to the intended audience would be disregarded. Much executive reading consists of best-selling books, which are attractive largely because of their narrative style. Executive readers connect with books that convey knowledge using stories that engage with and communicate a range of emotional situations that reflect their own experience. When academic writing prioritizes scientific rigor and neglects the human experiences, which popular business books convey so effectively, the writer shows an absence of audience awareness. We believe that forging a “positive emotional connection” with the reader (Bartunek, 2007, p. 1327) is an important element in increasing the pragmatic rigor of research studies.

Because comprehensibility is relative to an intended audience, the principle is mainly a caution for researchers to be mindful of their prospective readership and its expectations. Although dissemination outlets

are not expressly addressed in this principle, the choice of a publication medium is critical to reaching the intended audience. Ideally, outlets with policies intended to bridge the relevance gap would provide specific criteria to guide authors for this purpose.

4. Ethical Reasoning. *Ethical reasoning refers to the degree to which the application of research findings considers the range of stakeholders affected and equitably weighs the consequences to all stakeholders.* The principle of ethical reasoning is rooted in deeper philosophical principles of social justice (Rawls, 1999; Colquitt & Zipay, 2015) and ethical decision making (Ford & Richardson, 2013). Social justice argues for fairness in the treatment of all members of a society, so that rewards for actions are generated and distributed equitably, if not equally. Most scholarship in business focuses on developing and testing models based on economic objectives and rarely addresses broader social issues (Tsui, 2013). These studies often emphasize performance outcomes that advance individual or organizational wealth while ignoring the effect on other stakeholders, such as customers, employees, suppliers, or communities. The net value of research findings to society is rarely considered as a principle of scientific rigor. However, the neglect of broader social consequences limits the pragmatic value of management research and its relevance to executives wanting to pursue social values.

These obviously liberal views help to distinguish practitioner scholarship from narrowly defined proprietary research. Although both types of research might be designed to solve specific problems, practitioner scholarship aligns with an academic ethos of producing and sharing knowledge that has value for a broader spectrum of human activity. Scholarship is usually motivated by a need to understand human issues or problems (Laudan, 1986; Landry & Banville, 1992) and is therefore undertaken as a moral practice (Mason,

1996). Applying the methods of science to solve difficult social issues is the purpose of research on “grand challenges” (Winter & Butler, 2011; George et al., 2016; Davidson & Barrett, 2018). Such research might consider, for examples, the role of information technology in developing economies (Walsham, Robey, & Sahay, 2007) and environmental sustainability (Watson et al., 2014; Jenkin, Webster, & McShane, 2011). We suggest that the pragmatic rigor of studies that address such challenges is greater when they focus on the common good of a more diverse population of stakeholders (Carton & Mouricou, 2017).

The dimensions of the principle of ethical reasoning are based on a position paper written by a large number of management scholars in business and management schools worldwide who identify as the Community for Responsible Research in Business and Management (cRRBM, 2017). Their vision is to practice “*responsible science* [by] producing useful and credible knowledge that addresses problems important to business and society” (cRRBM, 2017, p. 1). We adapt three criteria from the organization’s seven principles: benefits to business and society, involvement of stakeholders, and effect of research on diverse stakeholders.⁴

Admittedly, *identifying problems important to business and society* requires judgments based on values. Applying this criterion therefore invites open consideration of the value basis of management research rather than advocating for specific values. Indeed, a dogmatic imposition of values might misdirect science as much as dogmatism about evidence does (Brown, 2012). Acknowledging the value base of research is a necessary consideration if one believes that science has the noble purposes of discovering truth and improving the human condition (Tsui, 2013).

The *involvement of stakeholders* is an ethical criterion for which practitioner scholarship is naturally well suited. Academics

continue to advocate for research with practitioners under the rubric of engaged scholarship (Van de Ven, 2018; Barrett & Oborn, 2018). However, practitioner scholarship addresses the need for practitioner involvement directly, with or without academic partners. Practitioner scholars should seek involvement of stakeholders in their research, rather than narrowly privileging their own interests or participation. Silent stakeholders, including the environment and members of future generations, also should be considered. Although silent stakeholders cannot act or represent their positions, other stakeholders might play critical roles at various stages to represent these interests without compromising scientific rigor.

Impact on stakeholders is a primary concern in ethical decision making (Ford & Richardson, 2013). This criterion asks whether research acknowledges its potential effects on diverse stakeholders, including its effect on the business and societal problems being studied. Considering stakeholder impact underlies most analyses of social justice (Rawls, 1999). Socially just practices in contemporary business are manifested in movements prioritizing social responsibility (Tsui, 2013), environmental sustainability (Watson et al., 2014), and conscious capitalism (Mackey & Sisodia, 2014), among others. Each of these movements advocates for managerial practices that acknowledge the rights and interests of multiple stakeholders while adhering to the mechanisms of free market capitalism to achieve higher aims than firm profit. For example, conscious capitalism includes four tenets: higher purpose, stakeholder orientation, conscious leadership, and conscious culture (Mackey & Sisodia, 2018). Mackey and Sisodia (2014) state that “...business is good because it creates value, it is ethical because it is based on voluntary exchange, it is noble because it can elevate our existence, it is heroic because it lifts people out of poverty and creates prosperity” (p. 21).

⁴ The four remaining principles are: (1) valuing both basic and applied contributions, (2) valuing plurality and multidisciplinary collaboration, (3) sound methodology, and (4) broad dissemination.

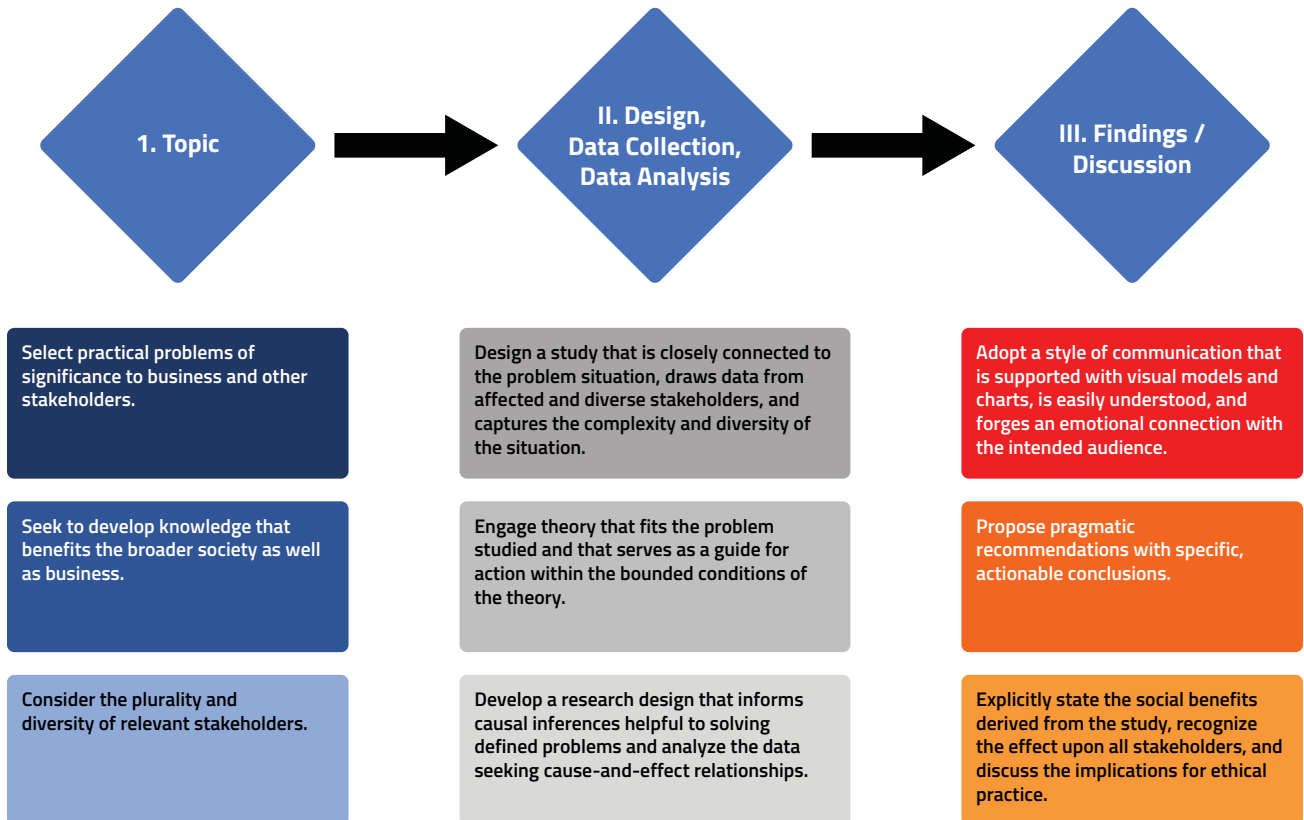
USING THE PRINCIPLES IN THE RESEARCH PROCESS

As emphasized earlier, we advocate for the application of the principles of pragmatic rigor throughout the research process, from topic selection to study execution to presenting findings and conclusions. Figure 1 presents an overview of three main stages of the typical research

process and provides guidance on how researchers might use these principles in sequence. Figure 1 is a graphic summary of the key ideas already presented in the previous sections of this article; it adds no new principles or criteria. Thus, it can be used as an overall guide that can then

be supplemented by the article to describe the specific criteria. Reviewers and editors also might use Figure 1 both to communicate expectations regarding pragmatic rigor or to prospective authors and to evaluate the pragmatic rigor of manuscripts under review.

Figure 1 - Using the Principles in the Research Process



CONCLUSION

In summary, a gap remains between research produced by academia and the practical needs of managers and industry. Although popular business books continue to sell millions of copies, few executives read scientifically rigorous academic business journals (Rynes, Gilik, & Brown, 2007). With this paper, we aimed to narrow the so-called relevance gap by offering guidance to authors, reviewers, and editors who wish to produce and evaluate practitioner scholarship. To advance this cause, we defined four principles of pragmatic rigor and explained the criteria for assessing the pragmatic rigor of research studies. Only with clear principles and criteria can evaluations of practitioner scholarship attend to its most unique characteristic – namely, the practical value it generates. Practitioner scholarship should be relevant, actionable, comprehensible, and ethically reasoned. These principles of pragmatic rigor should stand alongside standards for scientific rigor in the production of valid research findings that can affect practice.

Academic leaders and business journal editors often lament the lack of relevance in studies that they publish. However, they tend to neglect one of the more mundane, yet obvious, ways to narrow the relevance gap: the review process. Even when journals espouse the importance of pragmatic value, their review processes often tend to marginalize it. Instructions to reviewers overwhelmingly emphasize scientific criteria. In contrast to multiple criteria regarding scientific value, reviewers might be asked to respond to a single statement about a paper's practical significance – for example, "The paper is practically significant" (Straub & Ang, 2008, p. xi). We anticipate that our principles and criteria could be used by journals as a more comprehensive template to actuate editorial policies that espouse interest in the applied value of business research. Furthermore, we expect that researchers can incorporate principles of pragmatic rigor at the beginning of the research process, choosing topics and designing studies that promote

the practical utility of a study, as well as its scientific value.

We acknowledge some limitations in our analysis. First, although all questions in Table 2 suggest some form of variation from lower to higher degrees of pragmatic rigor, we have said little about the expected ranges of variation for each criterion. Our objective is not to offer refined scales of measurement for each criterion but simply to suggest that "more" pragmatic rigor is more desirable than "less." Thus, we have left unspecified the choice of scale design (e.g., 5- or 7-point Likert scales or strongly agree/disagree wording). Our emphasis has been on defining the criteria both as properties that can vary and as guides for evaluation.

Second, we have said little about the relationships among the four principles or their relationship to the global concept of pragmatic rigor. We simply have proposed the principles as components of pragmatic rigor, such that higher ratings for each principle generate a higher level of pragmatic rigor. This default assumption that the dimensions are additive could be challenged in several ways. For example, the relationships between principles perhaps could be compensatory, meaning that a higher rating on one principle (e.g., actionability) might compensate for a lower rating on another principle (e.g., comprehensibility or ethical reasoning). In addition, principles arguably differ in importance and should be weighted differently to reflect their relative importance. Some of the dimensions might be posed as prerequisite "necessary conditions" for pragmatic rigor (e.g., relevance or ethical reasoning). Alternatively, including a particular principle (e.g., ethical reasoning or comprehensibility) might be deemed unnecessary because they are seen as more general issues or universal guides for research.

At this stage, we have avoided developing a more intricate "theory" of pragmatic rigor, believing that such complications are premature and, perhaps perversely, con-

trary to the aim of promoting practitioner scholarship. We maintain the position that pragmatic rigor is a multidimensional concept and that the formula used to generate an overall score or index for pragmatic rigor would not produce much additional value. Thus, we do not go beyond positing the basic principles and criteria. In proposing this minimal (yet specific) set of principles and criteria, we encourage users to adapt them to fit their needs. Editors of particular journals might want to modify, weight, specify, combine, or eliminate criteria at their discretion. This flexibility would not be a misuse of our ideas; instead, it would be a welcome appropriation to serve specific interests and values. We hope that journal editors who wish to publish research of practical value find the four dimensions proposed to be a malleable resource, useful in formulating policies directed toward authors and reviewers.

Third, our focus is limited to the development of principles and criteria of pragmatic rigor. Practitioner scholars face other issues as well, including knowledge translation, knowledge dissemination, and adjustments to the peer review process. Knowledge translation focuses on the creation of more intelligible evidence upon which executive decisions might be based but universities typically do not educate students – whether part-time (as executives and managers) or full-time (as undergraduate or graduate students) – to understand, translate, or use scientific evidence. Research evidence might enter the classroom in lectures or case studies, along with other indirect translation mechanisms (Straub & Ang, 2008). However, absorbing research findings so as to make them useful is not the central focus of study for executives in continuing education programs, undergraduate business students, or even MBA students (Rousseau, 2006). As a result, knowledge translation is not taught or practiced, and so requires alternative channels that allow for academic and practitioner interchange (Jacobson, Butterill & Goering, 2003). Translation remains an important topic

that should receive more extended scrutiny (Smith & Nestor, 2018).

Related to knowledge translation is the issue of knowledge dissemination. Our focus on pragmatic rigor assumes that the primary means of disseminating practitioner scholarship is through peer-reviewed periodicals and journals. This assumption is a limitation because it neglects many contemporary options for dissemination enabled by advanced communication technologies. Much scientific research is relatively inaccessible for three reasons: the high cost of publishing journals, copyright protections, and the obscurity of many academic journals. Greater accessibility is achievable through blogs, digests, and industry “rags.” Unfortunately, from our perspective, more accessible channels might lack scientific rigor and promote popularist or puerile science (Anderson et al, 2001). Research in Pasteur’s (pragmatic) quadrant (Stokes, 1997) seeks to demonstrate both scientific and pragmatic rigor, which requires that the traditional review processes remain as a means of exercising quality control over the knowledge being disseminated. Consequently, more intentional focus on the creation and maintenance of high-quality channels for disseminating valid research findings by and to executives is needed.

Finally, the dissemination of practitioner scholarship through peer-reviewed journals raises issues about the qualifications of peer reviewers and the quality of their reviews. Pools of potential reviewers generally form within the ranks of academia as scholars develop their reputations and competencies in evaluating scientific research – particularly theory and method. Peer reviewers are invited to review manuscripts submitted to journals based on their specialized knowledge in the subject area of the journal or individual paper (Kelley, Sadeghieh, and Adeli, 2014). However, reviewers assigned to evaluate practitioner scholarship might have little or no experience or expertise outside of an academic setting. Their inexperience in the world of practice might therefore preclude a fair evaluation of the relevance,

actionability, audience appeal, or ethical implications of the reported study. Given the limited size of reviewer pools compared to the number of papers needing to be reviewed (Kelley et al., 2014), journal editors must find ways to diversify the types of people conducting reviews of practitioner scholarship. This shift might be achieved by training executives with professional doctorates in the practice of reviewing, by soliciting reviews directly from practitioners, and by offering more precise guidance on the criteria for evaluating the pragmatic value of an article sent for review. We hope that our efforts in this paper might serve as a guide to evaluation and be incorporated into executive doctoral programs so that students can acquire the necessary reviewing skills.

Despite these limitations, we see our efforts as a strong first step toward establishing principles for the conduct and evaluation of practitioner scholarship. We regard this contribution as a prerequisite to addressing these other issues, including translation, dissemination, and peer review, in bridging the relevance gap. Indeed, without both scientific and pragmatic rigor, studies might not be worth translating or disseminating. We wish to promote research that not only is scientifically rigorous, but also relevant, actionable, comprehensible, and ethically reasoned. Practitioner scholarship should strive to meet such standards in the service of solving real-world problems faced by executives and society.

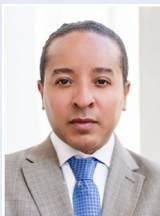
REFERENCES

- Association to Advance Collegiate Schools of Business International. 2018. *Eligibility procedures and accreditation standards for business accreditation*. revised July 1, 2018.
- <https://www.aacsb.edu/-/media/aacsb/docs/accreditation/business/standards-and-tables/2018-business-standards.ashx?la=en&hash=B9AF18F3FA0DF19B352B605CBCE17959E32445D9>
- Anderson, L., Gold, J., Stewart, J., & Thorpe, R. 2015. *A guide to professional doctorates in business and management*. Thousand Oaks, CA: Sage.
- Anderson, N., Herriot, P., & Hodgkinson, G. P. 2018. The practitioner–researcher divide in industrial, work and organizational (IWO) psychology: Where are we now, and where do we go from here? *Journal of Occupational and Organizational Psychology*, 74: 391–411.
- Barrett, M., & Eiborn, E. 2018. Bridging the research–practice divide: Harnessing expertise collaboration in making a wider set of contributions. *Information and Organization*, 28: 44–51.
- Bartunek, J.M. 2007. Academic–practitioner collaboration need not require joint or relevant research: Toward a relational scholarship of integration. *Academy of Management Journal*, 50(6): 1323–1333.
- Bartunek, J.M. 2008. You’re an organization development practitioner scholar: Can you contribute to organizational theory? *Organization Management Journal*, 5(1): 6–16.
- Bartunek, J.M., & Rynes, S.L. 2014. Academics and practitioners are alike and unlike: The paradoxes of academic–practitioner relationships. *Journal of Management*, 40(5): 1181–1201.
- Brown, M.J. 2013. The source and status of values for socially responsible science. *Philosophical Studies*, 163(1): 67–76.
- Carton, G., & Mouricou, P. 2017. Is management research relevant? A systematic analysis of the rigor–relevance debate in top-tier journals (1994–2013). *M@n@gement*, 20(2): 166–203.
- Clinebell, S. K., & Clinebell, J. M. 2008. The tension in business education between academic rigor and real-world relevance: The role of executive professors. *Academy of Management Learning & Education*, 7(1): 99–107.
- Colquitt, J. A., & Zippy, K. P. 2015. Justice, fairness, and employee reactions. *Annual Review of Organizational Psychology and Organizational Behavior*, 2(1): 75–99.
- Community for Responsible Research in Business and Management. 2017. *A vision of responsible research in business and management: Striving for useful and credible knowledge*. Position paper, November 22, 2017.
- Corley, K. G., & Gioia, D. A. 2011. Building theory about theory building: What constitutes a theoretical contribution? *Academy of Management Review*, 36(1): 12–32.
- Cummings, T. 2007. Quest for an engaged academy. *Academy of Management Review*, 32: 355–360.
- Davidson, E., & Barrett, M. 2018. Introduction to the research impact and contributions to knowledge (RICK) section. *Information and Organization*, 28(1): A1–A3.
- Davison, R., Martinsons, M. G., & Kock, N. 2004. Principles of canonical action research. *Information Systems Journal*, 14(1): 65–86.
- Dodge, J., Ospina, S. M., & Foldy, E. G. 2005. Integrating rigor and relevance in public administration scholarship: The contribution of narrative inquiry. *Public Administration Review*, 65(3): 286–300.
- Ford, R. C., & Richardson, W. D. 2013. Ethical decision making: A review of the empirical literature. In *Citation classics from the Journal of Business Ethics*: 19–44. Springer, Dordrecht.
- George, G., Howard-Grenville, J., Joshi, A., & Tihanyi, L. 2016. Understanding and tackling societal grand challenges through management research. *Academy of Management Journal*, 59(6): 1880–1895.
- Ghobadi, S., & Robey, D. 2017. Strategic signalling and awards: Investigation into the first decade of AIS best publications awards. *The Journal of Strategic Information Systems*, 26(4): 360–384.
- HakemZadeh, F., & Baba, V. V. 2016a. Measuring the actionability of evidence for evidence-based management. *Management Decision*, 54(5): 1183–1204.
- HakemZadeh, F., & Baba, V. V. 2016b. Toward a theory of collaboration for evidence-based management. *Management Decision*, 54(10): 2587–2616.
- Hambrick, D. C. 1994. What if the academy actually mattered? *Academy of Management Review*, 19(1): 11–16.
- Isaacs, T., & Trofimovich, P. 2012. Deconstructing comprehensibility: Identifying the linguistic influences on listeners’ L2 comprehensibility ratings. *Studies in Second Language Acquisition*, 34 (3): 475–505. doi. org/10.1017/S0272263112000150
- Jacobson, N., Butterill, D., & Goering, P. 2003. Development of a framework for knowledge translation: Understanding user context. *Journal of Health Services Research Policy*, 8(2): 94–99.
- Jenkin, T. A., Webster, J., & McShane, L. 2011. An agenda for “green” information technology and systems research. *Information and Organization*, 21(1): 17–40.
- Jin, L., & Robey, D. 2008. Bridging social and technical interfaces in organizations: An interpretive analysis of time–space distancing. *Information and Organization*, 18(3): 177–204.
- Johnson, S. L., Gray, P., & Sarker, S. 2019. Revisiting IS research practice in the era of big data. *Information and Organization*, 29(1): 41–56.
- Kelly, J., Sadeghieh, T., & Adeli, K. 2014. Peer review in scientific publications: Benefits, critiques, & a survival guide. *The Journal of the International Federation of Clinical Chemistry and Laboratory Medicine*, 25(3): 227–243.
- Kieser, A., & Leiner, L. 2009. Why the rigour–relevance gap in management research is unbridgeable. *Journal of Management Studies*, 46(3): 516–533.
- Klein, H.K., & Myers, M.D. 1999. A set of principles for conducting and evaluating interpretive field studies in information systems. *MIS Quarterly*, 23(1): 67–93.
- Krug, S. 2006. *Don’t make me think*. Berkeley, CA: New Riders.
- Landry, M., & Banville, C. 1992. A disciplined methodological pluralism for MIS research. *Accounting, Management & Information Technologies*, 2(2): 77–97.
- Laudan, L. 1986. *Science and values: The aims of science and their role in scientific debate*. Berkeley, CA: University of California Press.

ABOUT THE AUTHORS



Daniel Robey is Emeritus Professor of Information Systems at Georgia State University. He received his Doctorate in Business from Kent State University in Ohio, USA. His research interests include empirical examinations of the effects of a wide range of technologies on organizational structure and work practices. It also includes the development of theoretical approaches to explaining the consequences of information technology in organizations. His work has appeared many journals in information systems and management. In addition to numerous editorial positions on leading journals in management and information systems, he was Editor in Chief of *Information and Organization* from 2000-2014. He received the 2009 LEO award bestowed by the Association for Information Systems and the International Conference on Information Systems for a lifetime of distinguished contribution. He also received the Lifetime Service Award from the Organizational Communication and Information Systems Division of the Academy of Management in 2010.



Wallace Taylor is an entrepreneur and business person. He is currently the owner of Taylor Holdings International, Inc., and is also the Founder, Chairman, and CEO of The Taylor Foundation for the Arts (TFA). He received his Doctorate in Business from Georgia State University, Atlanta, GA. His entrepreneurial experience spans more than 10 years of successful initiation and implementation of private and non-profit organizations. He has worked in executive capacities in the fields of finance, technology, and real estate. His current work with participatory art in the field of public health serves a community of 2,000 students and involves contracts and collaboration between private, non-profit, and government entities. Through The Taylor Foundation for the Arts, Dr. Taylor granted funding to support public health research on children's health in Thailand. This was done in collaboration with multiple affiliated organizations including Emory University and the Provincial Public Health Office in Thailand. Dr. Taylor holds an Executive MBA from Troy State University, A Master of Internet Technology from The University of Georgia, and a BS from Georgia State University.



Louis J. Grabowski is the Director of the Executive Doctorate in Business Program in the Robinson College of Business at Georgia State University. His research interests and publications are in the areas of decision-making, organizational development, and non-profit management. In his business career of more than 25 years, Dr. Grabowski was a partner and executive in a commercial real estate business and has worked for the Kennesaw State University Foundation and in the insurance and high-tech industries. He received his Executive Doctorate in Business from Georgia State University and holds an MBA from University of California–Berkeley and a BA from Stanford University in International Relations. He and his family have lived in Atlanta for more than 30 years.