

Editors' Comments

Guest Editorial: Practice-based Research: We've Only Just Begun¹

In 1982, I enrolled in the University of Wisconsin-Milwaukee's new Ph.D. program in Management Information Systems. I enrolled not because I had a passion for the topic, but because I was an accounting professor who had been asked to teach a course in Information Systems. After reading the recommended textbook for the course I would be teaching, I could not figure out what we hoped students would learn or why. This was distressing!

I learned many interesting things in the Ph.D. program, but I did not get an answer to the questions that motivated my enrollment: why do all business students need a course in information systems? What should they learn? How would such a course help them and the companies they would someday lead?

What I now understand is that the drive to teach information systems in business curricula emanated from concerns that companies' growing investments in information systems were not generating a positive financial return. Business leaders recognized that the data and reports generated by their systems had become essential to doing business, but financial analyses suggested that additional expenditures did not result in corresponding value. Understandably, practitioners wanted to know how they might spend less or benefit more from information systems.

Research examined a variety of possibilities: (1) improvements to the systems themselves (e.g. better project management or requirements analysis); (2) alternative approaches to costing and assessing value (e.g. business case design and chargeback); and (3) dedicated change management processes (e.g., new governance, processes and roles to drive benefits). As researchers studied company efforts and their outcomes, they developed recommendations that almost certainly led to better management

and use of information systems. But did they substantially address the "high cost/low value" problem? It is questionable.

Good practitioners constantly work to improve outcomes. To do so, they seek out expertise. That creates a demand for practice-based research. But be aware that practitioners gain useful insights not only from academic researchers but also from their own experiments, their communities of practice, professional research and consulting organizations, and even professional magazines, where reporters describe successful companies' efforts. Because practitioners can rely on so many sources, it is important for academic researchers to offer a unique value proposition.

What is the academic researcher's unique value proposition? I would argue it is the rigorous training, research methodology, and dedicated time that academics can pour into developing deep insights. In other words, academics can look beyond the questions practitioners are asking to provide understanding of the underlying issues and solutions.

The Need for Deep Insights

It is tempting to suggest that an academic's training in theory is a critical asset in the search for deep insights. Indeed, theory can help a researcher understand a phenomenon and factors that reinforce or change circumstances. However, in my 30 years of conducting practice-based research, I have not found theory to be particularly helpful to practitioners. In other words, theory does not directly help practitioners make sense of their world.

Practitioners make decisions in a complex world that is constantly changing. They take actions to address the issues they perceive. From their ivory tower, academics will never grasp all the complexities that make any given practitioner's problems so difficult to solve – and it is not necessary to understand each individual's unique concerns. On the other hand, because they are not immersed in the pressures and daily chaos of any specific business, academics can gain visibility of general issues that affect many companies. In doing so, they can distinguish real

¹ I would like to thank Cynthia Beath for her insightful comments on an earlier draft of this editorial—and for many years of fun and inspiring collaboration.

issues from noise and help practitioners focus on what matters most.

To accomplish this worthwhile but challenging feat, academics must look beyond the questions that practitioners ask (and that professional research organizations and magazines happily address). The academic can help practitioners frame the questions they *should* be asking. By providing a deeper understanding of the issues, academic researchers avoid the allure of “silver bullets,” and instead pursue lasting changes.

For example, the early IS research on how to generate more value from information systems typically focused on new development. In doing so, the research ignored the reason so many business leaders were discouraged by the cost of information systems: operations and maintenance costs. In short, researchers heard business leaders asking how to design and implement better systems. But the answer to that question was never going to fix companies' very expensive legacy environment. In fact, the focus on new systems often exacerbated the problem. Some savvy CIOs were cutting their budgets so that business leaders would be forced to make tough decisions around systems priorities. Forced prioritization simultaneously increased the payback on new systems *and* addressed long-term maintenance and operations cost.

As Gabe Piccoli mentioned in an earlier editorial², practice-based research must be guided by practitioners' concerns. At the same time, researchers should avoid narrow definitions of those concerns. For example, in the late nineties and early 2000s, many business and technology leaders were tackling their legacy issues by implementing ERPs. Most were failing. Understandably, business and technology leaders asked for insights into how to succeed with their ERP implementations. Dutifully, academic researchers (and many other experts) studied ERP implementations and identified critical success factors. Nonetheless, failures remain common.

The reason that identifying critical success factors had limited impact was that the research had overlooked some flawed assumptions underlying many ERP implementations.

ERPs, by definition, standardize systems and processes across an enterprise. Business leaders were assuming standardization was a good idea, because research had demonstrated the potential for cost savings from standardization. However, standardization eliminates bad *and* good variability in processes. Consequently, the question that business and technology leaders needed to ask was: where does variability add value? Until they protected the value adding diversity, they could not successfully implement an ERP.

Today, many practitioners are anxious about the business changes that digital technologies are introducing. There is no shortage of research—and advice—on digital transformations. But again, the questions most practitioners are asking may not be the questions that lead to essential insights on digital business success. In our research at MIT, we found that different practitioners meant different things when they talked about digital transformations: one definition we refer to as *digitizing*, the other as *becoming digital*. It turns out that digitizing enhances operational excellence, while becoming digital enhances innovation and agility. Both are inspired by digital technologies, but the two opportunities are essentially opposites and they impose different business requirements. It is hard to make progress on a digital transformation until business leaders address questions about which digital opportunities are most salient for their business.

I should note that as researchers and practitioners take a deeper look at the toughest business challenges, there is an inclination to point a finger at organizational culture. My feeling is that blaming culture for anything is pointless. Identify the habits that impede progress and you're developing actionable insights; blame culture and you're merely stating the obvious.

How to Develop Deep Insights

I am guessing most academics who read and publish in *MISQE* will agree that deep insights about practice are a desirable objective for practice-based academic researchers. But Ph.D. training, and the unique style of academic writing, do not provide a template for developing and communicating deep insights for practice.

² See Gabriele Piccoli, “Editors' Comments,” *MISQE* (18:2), June 2019, pp. iii-v.

In fact, there is no template. Deep insights are born of passionate curiosity, and a belief that an answer is never fully known. Here is what I believe is required for deep insights:

1. Talk with practitioners to learn what issues they are facing. But rather than look for solutions, look for causes. Why is this issue not easily solved?
2. Based on interviews and cases at multiple companies, model your understanding of the issues.
3. Discuss your model with practitioners and academics and revise it until it becomes actionable—not just descriptive (i.e., it is provocative and answers the question: so what?).
4. Identify the new questions your findings raise and start exploring those.

Recognize that deep insights accumulate over time. Unlike traditional academic research, which maps a research study that will lead to a paper reporting the findings of that study, deep insights result from a series of studies, usually involving multiple research sites and multiple methods. Intermediate data—in the form of case studies or interview summaries—still provide useful findings (and great *MISQE* articles). Cross-case studies or perhaps quantitative analyses of larger samples provide the evidence for deep insights.

Ph.D. training should help researchers apply rigorous methodologies to generate insights and find theories that will help with sense-making. Ultimately, however, the practice-based researcher should pursue research more like a lawyer pursuing a verdict. The goal is to learn some element of truth and present evidence supporting that truth. *MISQE* was founded as an outlet for exactly that kind of publication.

The Growing Impact of MISQE

Over the years, *MISQE* has helped many practicing managers. It is extremely gratifying to see the impact the journal has had. Going forward, I believe its impact will be even greater.

I urge you to consider how you might develop deep insights into the management challenges that most interest you. In doing so, you will make *MISQE* essential reading for every academic and researcher, and have fun doing it!

About the Special Issue Editors

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Jeanne is the principal research scientist at the MIT center for information systems research. She spent 27 years at MIT's center for information systems research, conducting research that targeted the challenges of senior level executives at CISR's nearly 100 global sponsor companies. She is best known for her work around enterprise architecture, which was highlighted in her book *Enterprise Architecture as Strategy: Creating a Foundation for Business Execution* (*Harvard Business School Press*, 2006; coauthored by Peter Weill and David Robertson). In 2019, she published *Designed for Digital: How to Architect your Business for Sustained Success* through MIT Press with coauthors Cynthia Beath and Martin Mocker. Jeanne was often a featured speaker at global IT and enterprise architecture events. She was a regular contributor to *Sloan Management Review*. She also published in *MIS Quarterly*, *MIS Quarterly Executive*, *Harvard Business Review*, *The Wall Street Journal*, and a variety of other academic and practitioner publications. She is co-founder and former editor in chief of *MIS Quarterly Executive*.