Foreword

I've spent 40 years conducting information systems research and publishing in academic journals. During this time, I've seen many changes:

- There are more publication outlets for IS research. In particular, there are a large number of specialty journals, such as *Decision Support Systems*. But while this is the case, research-oriented universities still expect faculty to publish in the top-tier journals. This often translates into publishing in *MISQ*, *ISR*, and *JMIS*.
- The number of IS academics has grown. As a result, more scholars are trying to get their research published in a limited number of top-tier journals.
- Research is more specialized. Most of the articles in the top journals are of interest only to the people doing research in that area. There are fewer articles that appeal to a broad audience.
- Academic journals have higher expectations about the rigor of the research methods and data analysis methods used. Case studies are now rare and research studies with extensive multivariate data analyses are common.
- The "packaging" of research is more structured and important. Journals expect manuscripts to be organized and written in specific ways.
- Reviewers are more demanding. Manuscript reviews may be as long as the manuscript itself. And even after revisions are made, there is no guarantee that the manuscript will be accepted for publication.

Theses changes have consequences. It is more difficult and time consuming to conduct and publish research, especially in the leading journals. Junior faculty find it challenging to publish a sufficient quantity of high-quality research to beat the tenure clock. Some people worry that the value of more rigorous, specialized research comes at the cost of decreased relevance to practitioners.

There is another significant change that has taken place: the emphasis on theory-based research. Doctoral programs now require significantly more theory-oriented coursework. The leading journals expect research to either make a significant contribution to theory building or theory testing. This is a significant departure from the past when the only research requirement was to use the literature to show that the research question was important and that the research study and methods were an appropriate way to study the phenomenon under investigation.

There are many excellent reasons for making IS research theory based:

- The use of theory helps build a cumulative body of knowledge. New theories can be developed. Existing theories can be tested or extended. Without theory, there is greater risk that studies are fragmented and provide inconsistent findings. This was a frequent lament before IS research utilized theory more.

- Theory provides researchers with a better starting point. With theory, there is already a model or framework that identifies relevant constructs, variables, and casual relationships. As one of my colleagues, Dale Goodhue, describes it, "a theory is 'a stool' that you stand on to get a better view."
- Theory building and testing are the "gold standard" for research in the social sciences and other business disciplines. Scholars in other fields view IS research as more credible when it is consistent with the research norms in their disciplines. This is important for IS faculty when promotion and tenure decisions are made.

This book provides an excellent coverage of theory, both for learning about theories and how to apply them and as a reference book for conducting theory-based research. Many of the leading theories used in IS research are discussed and illustrated, and there are many excellent chapters that explore how to conduct theory-based research.

Though theory offers many benefits, it should be recognized that there are potential downsides to its use in IS research. Most specifically, it can negatively affect the relevancy of the research. Let's explore this concern.

Any theory, whether it is transaction cost theory or the resource-based theory of the firm, provides a particular "lens" though which a phenomenon is viewed. The problem occurs when the lens is inappropriate for the interests of practice. How the research question is framed and the questions asked are not aligned with practitioners' interests. It sometimes seems that the use of theory is driving the research questions rather than allowing the researcher to investigate the questions that are really of interest to practitioners.

Appropriate theory may not exist for some new, interesting topics. Several years ago when I was conducting research on executive information systems, I became convinced that the inclusion of "soft" information (e.g., interpretations of data) is positively associated with EIS success. I had even collected and analyzed data that supported the contention. When I presented the research at practitioner conferences, I drew large audiences that reacted well to the findings. But when I tried to publish the research in a leading journal, I encountered problems. There wasn't any theory that provided a satisfactory framework for the research. The research was ultimately published in a lesser journal that was less concerned about the lack of theory.

The requirements for doing high-quality, theory-based research results in long cycle times, which limits the timeliness of the research. It takes time to identify appropriate theory, apply the constructs, refine the items for the constructs, test the instrument(s), collect and analyze the data, and write up the study findings. Add to this long review and revise cycles and it is easy to understand why IS research typically lags the needs of practice. In fact, it isn't usually wise to investigate temporal issues because the research isn't likely to be published before less rigorous, practitioner research becomes the conventional wisdom.

Though there are downsides to the use of theory, I'm not suggesting that theory should not be used. The potential benefits are too great. Rather, I believe that the academy should be more flexible in terms of the kinds of theory that are deemed acceptable. Interesting, important research should not be rejected just because the theory base is not deemed to be strong enough.

I believe that a way to increase the relevancy of IS research is for the academy to be more supportive of "Little t" research. Let me discuss the concept of "Big T" versus "Little t" theory, which Alan Dennis and Joe Valacich first introduced to me.

A Big T is a well-recognized theory, such as the theory of reasoned action. It contributes to our ability to understand and predict a phenomenon and can be applied to multiple settings. It provides a solid foundation for building a research model and other research can build on the theory. It adds to the power of specific research findings when the theory is substantiated.

By way of contrast, a Little t is often a new and less developed theory. Some people may not even view it as a theory. It may be as simple as a list of steps, such as Kotter's organizational transformation model or a 2X2 matrix like the strategic grid. A Little t is the type of theory or model that is often taught in classes. A Little t is often phenomenon specific and may be used to either frame or be the output of research.

If the academy was more accepting of Little t research, it would make IS research more approachable and relevant to the business community. Studies could be framed in ways that are more relevant to practice. The time required to conduct the research would be reduced. The research methodologies and statistical methods used might be less difficult for practitioners to understand.

I'm not arguing against Big T research. We need research that transcends the temporal issues of the day and develops understandings that can be applied to new but related phenomenon. However, what I would like to see is more of a balance. Let's also support research that can be readily used by the business community and taught in our classes.

Whether theories are big T or little t, they guide our thinking as they guide our research. This book is a comprehensive and valuable resource for anyone interested in the theoretical underpinnings of information systems research. It includes papers that explore the formation, development, testing, and extension of theory within the field. The authors come from 49 universities in 13 countries, and thus represent a cross-section of contemporary thinking about theory. I believe that theory should be relevant to all our stakeholders – researchers, students, and practising managers – and this book provides value to each of these groups.

Professor Hugh J. Watson
The University of Georgia, USA

Hugh J. Watson *is a professor of MIS and a holder of a C. Herman and Mary Virginia Terry Chair of Business Administration in the Terry College of Business at the University of Georgia. Watson is a leading scholar and authority on decision support, having authored 22 books and over 100 scholarly journal articles. He is a fellow of The Data Warehousing Institute and the Association for Information Systems and is the senior editor of the Business Intelligence Journal. For the past 20 years, Watson has been the consulting editor for John Wiley & Sons' MIS series.*