MIS Quarterly
ISSUES AND OPINIONS

EMPIRICAL RESEARCH IN INFORMATION SYSTEMS: THE PRACTICE OF RELEVANCE[1]

By: **Izak Benbasat**
University of British Columbia
Faculty of Commerce and Business Administration
#452–2053 Main Mall
Vancouver, BC V6T 1Z2
CANADA
izak@unixg.ubc.ca

Robert W. Zmud
Michael F. Price College of Business
University of Oklahoma
Norman, OK 73019
U.S.A.
rzmud@ou.edu

Abstract

This commentary discusses why most IS academic research today lacks relevance to practice and suggests tactics, procedures, and guidelines that the IS academic community might follow in their research efforts and articles to introduce relevance to practitioners. The commentary begins by defining what is meant by relevancy in the context of academic research. It then explains why there is a lack of attention to relevance within the IS scholarly literature. Next, actions that can be taken to make relevance a more central aspect of IS research and to communicate implications of IS research more effectively to IS professionals are suggested.

[1]Lynda Applegate was the accepting senior editor for this paper.

Keywords: Relevance, rigor, academic research, applied research

ISRL Categories: AI0104, AI03, AI05

Introduction

"Is research in the Ivory Tower 'Fuzzy, Irrelevant, Pretentious?'" (*Business Week* 1990). The pointed question raised in the title of this *Business Week* article is not an isolated, off-hand observation. Instead, it represents the views of many of the stakeholders collectively holding the largess of business school faculty: students; recruiters; funding, grant, contract, and gift sources; contacts enabling access to resource sites; and business school deans. Scott Cowen, then dean of Case Western Reserve University's Weatherhead School of Management, stated "As much as 80% of management research may be irrelevant" (*Business Week* 1990, p. 62) and Richard West, New York University's business school dean at the time, was even more critical in his assessment of academic articles in scholarly journals, "[Business academics] say nothing in these articles and they say it in a pretentious way" (*Business Week* 1990, p. 62). While these remarks are somewhat dated, they most likely would be upheld, or perhaps even exaggerated, today.

The criticisms expressed above have also been directed to published information systems (IS) research (Galliers 1994; Saunders 1998; Zmud 1996a, 1996b). That IS research has a credibility gap within the business community is certainly

evidenced by the 1995 decision of the Society for Information Management (SIM) International to terminate their long-standing practice of including a subscription to the *MIS Quarterly* as part of their members' dues. And, when provided with the opportunity on their dues statement to receive (at additional charge) the *Quarterly* at a discounted price, few members have chosen to do so—even though the journal has as a stated editorial mission of publishing research targeted at information system managers.

It certainly is not a surprise to most IS academics that the business community would question the practical relevance of IS research published in the leading journals of our field. Does IS research produce the knowledge that today's IS professionals can apply in their daily work? Does it address the problems or challenges that are of concern to IS professionals? Does it focus on current technological and business issues? Are IS research articles accessible to IS professionals? It is our view that the answers to these questions do not shed a favorable light on IS academic work. The aforementioned fact that the *MIS Quarterly,* which has made a conscientious attempt over the years to cater to practitioners, has lost the majority of its SIM readership tangibly supports such a view. For much "softer" evidence, how often have IS practitioners commented to you about an article published in an IS scholarly journal? One of this article's authors regularly interacts with an elite group of information systems executives in his role as research director of SIM's Advanced Practices Council. In the rare instance when a reference to published academic work is made by these executives, it invariably was published in *Harvard Business Review* or *Sloan Management Review.*

We do not wish to imply through this essay that all IS research should strive to meet the (often capricious) needs of practicing IS professionals. We value the role theory development and basic research play in the advancement of the field. This paper is targeted to IS academics who are committed to both applying rigorously the methodology best suited to their research goals and *better accommodating* practical relevance within their research endeavors. We consider IS professionals (including analysts, user representatives, and IS managers) as well as managers with an interest in information technology (IT) deployment and utilization to be the consumers of IS research that is relevant.

The intent of this article is two-fold: to explain why one tends today to observe a lack of **relevance to practice** in IS research and to suggest tactics, procedures, and guidelines that the IS academic community might follow to introduce relevance into their research efforts and articles. We begin by defining what is meant by relevancy in the context of academic research. Then, we provide an explanation for the lack of attention to relevance within the IS scholarly literature. Finally, we recommend actions to be taken to make relevance a more vibrant aspect of IS research efforts and to communicate the fruits of IS research more effectively to IS professionals.

It is important to mention at the start that the views expressed in this paper are those of two North American IS academics who have mainly espoused a positivist research tradition. Colleagues who prefer to see the world through other lenses, e.g., advocates of "action research," are likely to offer their own approaches to attain relevance. In addition, we focus here on the contributions of empirical research to relevance; "design" is outside the scope of our discussion. Our answers here are obviously not the only ones feasible, but we hope that they will initiate a discussion on how to obtain the same goals through multiple means.

The Nature of Relevant Research

Just what exactly is research that is "relevant to practice"? It is possible to gain insights regarding what managers find to be interesting and important (Price 1995; Sears and Pickler 1996). The **topics** being addressed are clearly very influential in determining an article's relevance in the eyes of practitioners. Not surprisingly, articles that address enduring (or current) organizational problems, challenges, and dilemmas as well as articles that address timely business issues tend to be well received by practice.

Relevancy is not assured, however, through the selection of a "relevant" topic. Unless an article's implications are **implementable,** i.e., prescribed in a manner that could be put to use (to some extent) in practice to exploit an opportunity or to resolve a problem, practitioners are unlikely to characterize it as being of interest. Not all (perhaps, even, quite few) scholarly articles, however, manage to produce such outcomes. Does this mean that few scholarly articles have the potential to be relevant? It does not, as two other types of articles are also valued, though not as universally, by practicing managers. They are articles that **synthesize** an existing body of research (i.e., which classify, categorize, and summarize major themes and findings), and articles that **stimulate critical thinking** by challenging the reader's causal assumptions (Davis 1971) or by identifying emerging trends, structural changes, or paradigms. The former provide a fairly "painless" way for managers to acquaint themselves with the "state of knowledge" regarding specific domains, while the latter have the potential to enhance or restructure the mental models managers apply in their practice.

Just as important as, if not more important than, an article's content is its **style and tone.** Stated simply, articles that are not read, regardless of their content, are not relevant. Articles that tend to be read by IS professionals are those that

- are shorter
- use more exhibits
- use everyday language, rather than esoteric or stilted language
- have less discussion of related literature
- have less discussion of a study's methods
- have more contextual description
- have more prescriptions

Note that the above attributes omit commentary on an article's rigor, i.e., the correct use of methods and analyses appropriate to the tasks-at-hand. Relevancy does not imply that research needs to be carried out in a less rigorous fashion. In fact, managers respect and value rigor (as it often proves to be a key discriminator between academic and consultancy contributions). What should be refrained from are the "trappings" of rigor: lengthy homages to the "literature," too "scholarly" a tone, and elongated descriptions of a study's methodological and analytical procedures that could be kept in the background in appendices.

To summarize, we suggest that authors who strive to craft relevant articles for practitioners must, at a minimum, focus on the concerns of practice, provide real value to IS professionals, and apply a pragmatic rather than academic tone. Ideally, they would also describe how the ideas discussed or actions suggested would be implemented in practice, allowing for contextual differences that are important to individual readers.

Why Much IS Research Lacks Relevance

In this section, we discuss the reasons that much of the current IS literature lacks sufficient relevance. If these are structural (i.e., reflective of institutional or environmental forces), then resolutions of such structural elements must be embedded within the tactics we propose for increasing the relevancy of IS research.

We have identified five such explanations. Two are associated with the nature of IS scholarship: an emphasis on rigor over practical relevance and a lack of a cumulative research tradition. A third reflects the dominant attribute of the domain in which our research takes place: the rapid and continuous rate of change associated with information technologies. The fourth reflects the limited extent to which IS academicians are exposed to the business and technological contexts in which IS phenomena transpire. The fifth is associated with institutional and environmental constraints that influence the freedom of action within academia.

An Emphasis on Rigor Over Relevance

In order to establish IS as an academic discipline and to gain the respect of more established academic disciplines in business schools, IS researchers and the editors of top IS academic journals have tended to emphasize rigor over relevance in their journals and in their views of appropriate promotion and tenure criteria. Such a stance was taken as a reaction to the criticism leveled at the field since its inception. (See Benbasat and Weber [1996] for a historical perspective.)

Throughout the 1970s, it became quite evident that the quality of IS research, in comparison to that of other business disciplines, was found wanting.

In response, North American IS academics began laying the foundations of a research infrastructure with new journals (*MIS Quarterly*), conferences (International Conference on Information Systems), and workshops on research methods (Harvard Business School colloquia on qualitative, experimental, and survey research). By the 1990s, this continuing emphasis on performing rigorous research had finally paid off. Today, the quality of IS research matches that of its sister business school disciplines. But the price we have paid is that the practical relevance of our research has been relegated to a secondary role.

Lack of a Cumulative Tradition

Generally, IS researchers have been less successful than their colleagues in other business school disciplines in developing a cumulative research tradition (Keen 1980). Without such cumulative results, it becomes difficult, if not impossible, to develop and assess strong theoretical models such that *prescriptive actions can confidently be suggested for practice.*

At least three explanations arise to explain this lack of a cumulative tradition. First, in IS, a multiplicity of theoretical frames exists for most phenomena being studied. For example, an examination of interorganizational linkages could very well be approached from a number of quite distinct perspectives (I/O economics, computer network or data architectures, sociological networks, organization design, interpersonal relationships, population ecology, etc.), with resultant articles potentially publishable in the same IS journal. IS scholars reflecting each of these disparate perspectives often have difficulty understanding, let alone incorporating, work performed within others. Second, as a community, IS scholars have, until recently, been reluctant to value the existence of a well-defined collection of research constructs and instruments. Being technophiles at heart, IS researchers would much rather invent than adopt! But without common tools and a shared language, it becomes difficult to evolve streams of research that build rich conceptualizations and understandings of the phenomena populating the IS domain. Third, there has been a significant proliferation of IS journals, as with many other disciplines, over the last two decades. As a consequence, it is becoming harder and harder to locate and access, let alone build upon, the work of others.

The Dynamism of Information Technology

Many IS academics (as well as practitioners) find the dynamic technological environment of the IS field to be one of the more compelling motives for working in this field. But this dynamism also (1) adds much complexity and uncertainty to our research endeavors, (2) results in our chasing after practice rather than leading practice, and (3) typically leads to reporting results from (rigorous) studies involving new technologies years after the technology's acceptance (and, occasionally, its rejection) by practice. As a result, many of the articles published in current issues of IS journals read—in terms of the technical and business contexts being described—as if they could have been written years ago (in many instances, the first drafts probably were!).

As a community, IS scholars just have not dealt well with the dynamism of our field. Too often, studies that focus on new technologies are published far past the date when the technology could be considered "new." Needless to say, pronouncements in the future about today's technological and associated business challenges are just not going to be considered relevant by most practitioners. This structural problem has clearly not been handled well and will be addressed later in this article in the recommendations to authors on topic selection.

Limited Exposure to Relevant Contexts

In order that IS research be relevant, IS researchers must in some form or another be exposed to the practical contexts where IT-related usage and management behaviors unfold. For many IS academicians, such exposure tends to occur infrequently and, when it does occur, tends to be insufficiently targeted, insufficiently rich, or both.

We believe there are three reasons for this. First, academics are very busy with teaching, research, and administrative service leaving very little time to gain exposure, on a regular basis, to the contexts with which practitioners must contend. Second, when such exposure does occur, it often entails either recruiting or consulting (including continuing education) relationships. With recruiting contexts, the majority of the discussion usually focuses on curriculum issues, skill set development, and assessments of specific students, and emphasizes technical, rather than managerial, issues. With consulting relationships, IS academicians often find themselves involved in delivering predefined content (i.e., continuing education) or a predefined output (a design, a program, etc.) In both cases, what tends to be absent are rich, loosely-structured dialogues of the opportunities and problems being experienced in practice and discussions of how these might be examined through academic research. Third, all IS academics lack a sufficient exposure to current and future technological environments. Many, if not most, academic IS departments are laggards regarding their capabilities to maintain a current, let alone leading-edge, hands-on technological environment due to a lack of financial and human resources.

Institutional and Political Factors

The patronage and power structures within academic institutions wield significant influence on the degrees of freedom academics have in pursuing relevance. To be "legitimate," IS researchers have to adhere to the rules and conventions imposed upon them by the academic patronage system (King and Applegate 1997). The majority of the leading North American universities and business schools are research-oriented institutions. Many of the top business schools stress theory-based empirical research, economics, or mathematical modeling based work in their promotion and tenure decisions. In the past, internal (university) and external (federal) granting agencies have emphasized the pursuit of rigor as the standard for awarding research grants. Therefore, our institutional environment has not encouraged the pursuit of relevance. Interestingly, King and Applegate comment that the increasing shift from government to corporate funding may be fueling interest in the relevance of research.

Summary

All of the five reasons discussed above have affected the topics studied in academic IS research, whereas the emphasis on rigor, institutional factors, and limited exposure to relevant contexts have been mainly responsible for influencing the language and argumentation employed in academic IS articles. The IS discipline has made significant progress in two areas. Today, the quality of most IS research work is comparable with that of other business school disciplines. Since we have managed to achieve a high standard of rigor, we can afford to shift some of our attention toward relevance without undue concern about being criticized by others, although the forces of institutional patronage still limit the scope of such a shift. And, over the last decade, we have begun to observe cumulative traditions develop in a number of the theoretical streams that comprise the IS discipline. The remaining three areas—the fast changing IT environment, limited exposure to relevant contexts, and the institutional context—continue to pose significant challenges for the IS community.

Suggestions for Increasing the Relevance of IS Research

The primary responsibilities for publishing more relevant IS research falls jointly on the authors who produce manuscripts and the journal editors who decide which of the submitted manuscripts will be published. Authors need to rethink their behaviors regarding the topics being studied and written about, the purpose of the research projects, and the readability of their manuscripts. Journal editors need to rethink their behaviors regarding acceptance/rejection criteria and related signals (reviewer feedback, editorial comments, and published articles) sent to current and future authors. Here, we develop each of these pathways for increasing the relevance of IS research and offer specific recommendations.

Authors: Topic Selection

Too often, an individual's or group's interest regarding a particular issue, and not the intended outputs of the project, becomes the sole basis for

initiating research. While interest and an ability to perform are required, they should be necessary rather than sufficient determinants of a research project. The key criterion is the potential value and interest that stakeholders (journals, colleagues, practitioners) are likely to derive from the outcomes of the research project, outcomes most likely to appear in the literature three to five years in the *future*.

Recommendation 1: The foremost criterion to be applied in selecting research topics should be directly related to the future interest that key stakeholders (journals, colleagues, and practitioners) are likely to hold in a topic.

We suggest a *two-pronged* approach to the selection of relevant IS topics. The first is to be more attuned to the needs of practitioners by paying more careful attention to the expressed areas of interest to practitioners. The second is for the IS academic community to take a more proactive role in discussing the key research areas of the discipline by taking into account the benefits that will accrue to practitioners from research in these areas.

All too often, however, IS academics discover their research topics from the IS academic literature. Using the IS academic literature as the primary source for research topics is problematic for at least two reasons: the articles are dated when published, and few of these articles were likely motivated explicitly by the concerns of practice.

Recommendation 2: IS researchers should look to practice to identify research topics and look to the IS literature only after a commitment has been made to a specific topic.

There are many ways to find out what is of interest to practitioners: going to practitioner conferences, talking to practitioners, reading practitioner and general management journals, teaching executive education courses, engaging in consulting activities, etc. Another effective vehicle is "key issues" surveys, where practitioners identify those issues of most importance to them. Admittedly, time delays are present between the conduct of these surveys and their publication. For example, the latest MISRC/SIM key issues survey published in *MIS Quarterly* (Brancheau et al. 1996) presents key issues identified by practitioners in 1994—even with an expedited review process. Nevertheless, while "key issues" are expected to vary in their importance over time, they historically have exhibited a reasonable degree of stability. Still, the best strategy for any IS researcher is to develop personal relationships with individual practitioners, both as a source of ideas for future projects and as a source of feedback on current projects.

Even with the identification of what is of interest to practitioners, we obviously still face the problem of reaction time. If, on average, it takes three or more years from the start of a study to eventual publication, then the "sense and react" cycle is much too long. Therefore, rather than "intelligence" regarding what was important two years ago (from "key issues" surveys) or what is important today (from the current IT press), what we need is to be focusing on those fundamental issues likely to be important three years from now! This is where we think the IS academic community should and could play a more active role.

Consider, for example, a concern of great importance to IS practitioners and a topic for which considerable academic research has been performed: the adoption and adaptation of new information technologies. Over time, IS researchers (along with colleagues from related disciplines) have had success in defining the nature of this phenomenon and developing a rich understanding of the fundamental concerns associated with it. But for how many other phenomena germane to IS contexts could a similar picture be painted? We think surprisingly few. While individual intellectual creativity and serendipity in identifying research topics will (and must) always exist, we can not shirk our collective responsibility to bring more focus to the IS discipline. Without an understanding of the fundamental phenomena that comprise our field, how can we be proactive and have, even partially, answers to assist practitioners when new problems arise? We need to reach some degree of consensus as a community regarding the core, enduring phenomena associated with information systems planning, design, development, implementation, and use. And such a consensus must be biased toward the future while remaining cognizant of the past. We realize that a call for consensus has both proponents (Benbasat and Weber 1996) and opponents (Robey 1996). What we are suggesting here is agreement on the

nature of these core phenomena, not conformity on how to investigate them.

Recommendation 3: More discussion on the core research issues of the IS field that have the potential to influence practice is needed. Members of the IS research community must prepare manuscripts that define the important phenomena for the various segments of the IS discipline, and editors of the leading IS journals, as well as other major outlets such as ISWorld Net, must work with these authors to ensure that such information is disseminated.

Authors: Article's Purpose

Research projects are undertaken for many different reasons, and once they have been completed, articles describing these projects can again be written with quite distinct purposes in mind. Often IS research projects are initiated because authors wish to communicate how they have conceptualized and dealt with a challenging problem rather than communicate a compelling and implementable resolution for the problem. Articles based on the former projects are written primarily to describe the completed work for publication purposes rather than to communicate a well-honed message to a well-defined readership. Generally, IS researchers tend to focus on the "inputs" (e.g., conceptualizations and methods) of their research projects rather than the "outputs" of these efforts.

Recommendation 4: When deciding whether or not to begin a new research project or a new manuscript, IS researchers should focus on the likely outcomes, rather than the inputs, of such efforts.

Let's consider these "outputs," focusing on the three basic ways in which organizational researchers could influence practice (Astley and Zammuto 1992).[2]

Direct Utilization

Academic work could impact practice through the development of tools, techniques, and practices (Astley and Zammuto 1992). As such an output holds immediate and real value to practice, it tends to be the primary criteria applied by those who criticize the relevance of academic research. Such research contributions are infrequently observed since "researchers and users belong to separate communities with very different values and ideologies and these differences impede utilization" (Beyer and Trice 1982, p. 608). Academics doing theory-based empirical research normally strive to develop parsimonious theories with a limited number of variables that explain phenomena across a wide range of organizations, settings, and contexts (Daft and Lewin 1990). However, what practitioners desire are rich prescriptions to be applied in (their) specific situations that capture the uniqueness and complexity of their own organizational settings. Therefore, the potential for direct applicability is typically quite low.

Still, it is possible for some academic research to contribute to practice in a direct, implementable mode. Once a sizable body of literature exists regarding a phenomenon, it does become possible to synthesize this literature, e.g., as a state of the art review, to develop usable prescriptions. And, as this body of literature evolves, authors can place the insights gained from their specific studies within this broader body of knowledge to again produce useable prescriptions.

Recommendation 5: In order for IS researchers to be more proactive in a direct sense, it is imperative that the IS research community produce cumulative, theory-based, context-rich bodies of research.

What are these theories, if any, which form the basis for these cumulative bodies of research? While the IS discipline has had little success in creating its own theories (the on-going debate about whether or not this is desirable for the IS discipline lies outside the scope of this essay), it has been fairly successful in applying theories developed in related disciplines to important IS phenomena. One exemplar of such practices involves the efforts of a number of IS researches to adopt and adapt the Theory of Reasoned Action (Fishbein and Ajzen 1975) and its extensions, such as the Theory of Planned Behavior (TPB) (Ajzen 1991), to the study of IT adoption, implementation, and use.

[2]Astley and Zammuto label them as the instrumental, conceptual, and symbolic modes.

TPB posits that an individual's use of a specific IT will be influenced by: (1) *attitudes* formed by the person's beliefs about the expectations of outcomes associated with IT use, (2) *subjective norms* formed by the person's beliefs about how important others expect the person to behave regarding IT use, and, (3) *perceived behavioral control* formed by the person's beliefs about the extent to which the person is capable of actually using the IT. While TPB does not describe the process of implementation in a specific context, it has experienced a high degree of predictive validity and can be used to identify areas of concern for a specific context (see Taylor and Todd 1995 for an overview). As such, it can serve as an effective diagnostic tool.

Now let's examine how the body of published IS research applying TPB fares in terms of its "utilizability" (Cheng and McKinley 1983), i.e., its practical relevance, the applicability of its findings, and its specificity. First, TPB can certainly be conceived as possessing *practical relevance* as it focuses indirectly on improving organizational performance through its predictions regarding IT usage. Second, prior to implementing a particular IT, the potential users of this IT could be surveyed to determine their likelihood of using the IT, their attitudes toward using the IT, their subjective norms and behavioral control. Based on the feedback, system implementers could take appropriate corrective action (e.g., improving attitudes by making the new system more compatible with current practices.) As TPB contain variables that can be manipulated by practitioners, its findings are *applicable*. Third, one of the strengths of the TPB is that while it is a general theory, it could be applied to a wide variety of contexts for predicting the adoption of different types of IT. Its major constructs reflect the key variables that have been identified as influential in previous implementation research, such as top management support, and are flexible enough to subsume situation-specific factors; thus it has the characteristic of *specificity*. Considering all three criteria together, TPB does seem to be a theory very likely to produce *utilizable* outcomes.

In addition to a theory's usability, there is another major way academic researchers in IS can instrumentally contribute to practice. These are contributions in the area of methodology and measurement. Practitioners are very often involved in measurement. Consider, for example, the needs of an individual assigned to undertake a post-implementation audit of an information system that involves assessing users' satisfaction with the quality of the implemented system. Another common measurement need involves user perceptions of the usability and usefulness of an application's interface, especially during a prototyping development effort.

To answer these questions correctly requires proper measures. However, measurement is a complex endeavor, and poor measures are worse than no measures as they provide incorrect data for prediction, monitoring, and evaluation. The aforementioned emphasis on rigor has influenced IS academics to develop a large number of high-quality measurement tools. Recently, for example, the May 1997 issue of *MIS Quarterly* published three research notes on the utilization of the SERVQUAL instrument to measure IS service quality. Many such instruments, and evidence of their quality, are available from a web site for IS measurement instruments (http://www.ucalgary.ca/~newsted/surveys.html).

Frames of Reference

Based on their research findings, academicians supply new concepts, which then alter the perceptions and mental models that practitioners apply in their work lives. What are some of the IS-related conceptual developments that have had an impact on practice? End-user computing, critical success factors, business process reengineering, Nolan's model of stages of growth, Porter's value chain as well as his competitive advantage framework, and transaction cost theories are just a few that immediately come to mind. What kind of conceptual development is most useful for practice? Generally, those that reorganize phenomena such that they seem less complex (Davis 1971) or that enable contingency-based action. Descriptive case studies, together with the author's interpretations of events taking place in a specific organization, often prove to be effective means for communicating such contributions to practice.

> By imputing understandings to managerial events, [academics] perform a quasi-

> journalistic role by communicating and reporting interpretations that disseminate throughout the managerial community (Astley and Zammuto 1992, p. 456.)

An excellent example of this conceptual propagation is provided by the Strategic Grid (Applegate et al. 1996). Here, a 2-by-2 matrix represents the strategic relevance and impact of an organization's IT portfolio, along with intuitively understood labels like factory, support, strategic, and turnaround. Such labeling is critical as it becomes the vehicle (i.e., the "hooks") for contingency models that allow managers to then interpret these concepts from the context of their own organizations. Similarly, Nolan's (1973) "Stages Hypothesis," through its initiation, contagion, control, and integration stages, crisply and clearly describes the phases that essentially all organizations follow as they increase their investment in and dependence on IT. Why is it that the "Stages Hypothesis" continues to be received well by practitioners, in spite of having been criticized by some academic researchers on theoretical and empirical grounds? It is because it provides a conceptual language enabling an IS manager to identify where his/her firm is positioned in a "stages" sense. This allows the manager to better grasp the current challenges facing the firm and the appropriate tactics for overcoming them, to predict what is likely to happen as the firm transcends to successive stages, and, most importantly, to communicate these notions to other executives.

Recommendation 6: In order for IS research to be more relevant, it is important that authors develop frames of reference which are intuitively meaningful to practitioners to organize complex phenomena and to provide contingency approaches to action.

Justification

Theories, concepts, and findings from IS research could be used by practitioners to legitimate, rationalize, and justify courses of action taken (Astley and Zammuto 1992). Over the last decade, numerous practitioners have undoubtedly used the "bible" of academic research (along with the presence of academic consultants) to justify IT-based initiatives on the basis of "reengineering" or gaining "competitive advantage." For example, IT expenditures may be defended by claiming that they are needed to reengineer the firm for better customer service and improved employee morale through empowerment, as claimed in the academic literature.

A particular form of IS research often used in justifying action is the *benchmark* study. Because such studies (e.g., the percentage of IT budget that goes toward new development, the composition of firms' application portfolios) enable firms to compare themselves with others, they tend to be of great interest to practice. There is understandably a desire to evaluate yourself against your peers, for budgeting purposes as well as for use in justifying why particular actions were taken. Typically, such information is actually a by-product rather than the primary outcome of many academic research studies. However, by including such material within an article, the article's relevance can be greatly enhanced.

Recommendation 7: In order for IS research to be more relevant, IS academics should portray the outputs of their research in ways such that it might be utilized by practitioners to justify and rationalize IT-related initiatives.

Authors: Article's Readability

A principal means (and perhaps the least difficult) for enhancing the relevance of IS articles lies with improving the language of communication. A practitioner member of the Academy of Management makes this point strongly:

> *Journal* and *Review* seem written by academics looking to impress their fellow academics with their ability to use polysyllabic jargon to confuse and obfuscate. I sometimes show articles to colleagues as a JOKE. Most can't make it through the abstract (Sears and Pickler 1996, p. 25).

A number of years ago, the *MIS Quarterly* responded to such criticism by its managerial readership (1) by incorporating reader-friendly "executive overviews" for published articles as a means of increasing practitioners' motivation to actually read the articles and (2) by proactively working with authors to improve the readability of their manuscripts.

The tendency of academics to communicate their ideas through a dense and dry style rests

partly on the belief that reviewers expect to see evidence of academic rigor. But how exactly does one demonstrate academic rigor:

- Does scientific rigor demand complex, detailed, and often redundant explanation?
- Would scientific rigor benefit from clear, simple, and concise explanation?

Too often, while the former view prevails, the latter view may actually communicate an article's rigor—to both practitioners and academics—much more effectively. As our academic subdisciplines become more and more specialized, even IS academicians are having difficulty communicating with one another!

Recommendation 8: The vast majority of IS research articles should be crafted in a clear, simple, and concise manner such that they are accessible by *all* the potential readership of a journal.

The exceptions to this recommendation might involve manuscripts whose primary contribution to the IS field is based on technical/analytical sophistication and elegance, articles dealing with research methods/methodologies, and philosophical discussions of the foundations of the IS field.

Journals: Reviewing Processes

We have made a number of suggestions to enable and encourage IS academicians to undertake research projects on topics of interest to practitioners and to then prepare articles relevant for practice based on this research. We do recognize, however, that the primary force influencing IS academicians regarding their research, manuscript preparation, and journal selection behaviors are the signals provided them by leading IS journals: mission statements, review comments, and editorial decisions. If the IS discipline is to become more relevant, this change process must be fostered by our leading journals.

A much better balance among rigor and relevance is needed in the editorial review process. Our own experiences in a variety of editorial roles suggest that the most difficult entity to influence regarding the reviewing processes is the behavior of reviewers. Thus, journal editors and board members must adopt proactive stances in their support of relevance. Conscious effort should be spent to (1) select referees likely to value both rigor and relevance and (2) ensure that review commentaries and decisions address a manuscript's relevance as well as its rigor.

Recommendation 9: Editors and editorial boards of all IS journals need to critically examine their current postures, reviewing procedures, and editorial decisions concerning the balance between rigor and relevance with the goal of publishing manuscripts that are characterized by *both*.

Conclusions

In our commentary, we have attempted to offer some specific recommendations to increase the relevance of research published in IS journals. In Table 1, we summarize the key dimensions of relevance and in Table 2, recommendations as to how to achieve them. Note that in Table 1 we do not have a dimension labeled "useful." In our view, as outlined in the section entitled "The Nature of Relevant Research," an article describing research that is *interesting, applicable,* and *current* has the potential to be *useful* for practitioners. This potential is fulfilled when the article is accessible, i.e., understood by practitioners. Therefore, our definition of relevant research is one that is potentially useful for, as well as accessible by, its intended audience.

Our view accommodates the need for rigor while clearly distinguishing the "relevant academic" from the consultant. While both academic researchers and consultants value the discovery and application of new ideas and solutions, it is the academic researcher who is more concerned with issues of justification (i.e., insuring that what is being discovered and applied is in fact "correct"). While practitioners seem to feign little direct interest in justification, most do recognize that justification allows them, even if indirectly, to better assess whether or not the prescriptions they receive are based on solid foundations.

The world of practice certainly has much to offer the IS academic researcher. Practice provides strong signals regarding what we should be studying. Invariably, it is practitioners, as they strive to

Table 1. Dimensions of Relevance

Category	Dimensions of Relevance	Description
Article's Content	*Interesting*	Does IS research address the problems or challenges that are of concern to IS professionals?
	Applicable	Does IS research produce the knowledge and offer prescriptions that can be utilized by practitioners?
	Current	Does IS research focus on current, *at the time of publication,* technologies and business issues?
Article's Style	*Accessible*	Are IS research articles able to be understood (in terms of tone, style, structure, and semantics) by IS professionals? Are they written in a style that professionals would enjoy reading?

stay ahead of their competitors, who discover through trial and error (if only within a single context) the value of emerging IT-related innovations. Clearly, a strong symbiotic relationship exists between practice and research. Theories and models are judged on their predictive power for guiding practitioners, the quality of theories are enhanced by testing and revising them with data from the world of practice, and the cues and insights offered by practitioners are assessed through rigorous examination across a variety of contexts.

Academic research is time consuming, due in part to the necessity to adhere to rigorous standards. We have no doubt that information systems practitioners understand this well, since they too face the necessity of devising technically correct, yet resilient, resolutions to complex issues for which there are no simple solutions. And, like IS practitioners, academics face more interesting and important (research) challenges than they can tackle given their limited time, effort, and monetary resources. So, how do we proceed? We, as a community, must engage in a dialogue with practitioners about: (1) their "critical success factors"; (2) the important challenges they confront on a regular or periodic basis; (3) the important questions they have been unable to find answers for; and (4) the issues that will be important to them three to five years from now. Similarly, we as academics, with insights from practitioners, must define the "fundamental issues" of our discipline. We are just not convinced that a laissez-faire climate of "let a thousand flowers bloom" is in the best long-term interest of the IS academic community.

We must make a concerted effort to communicate to practitioners how our research would be relevant to them. As well, we need to demonstrate, or at least describe, the extent to which the outcomes of IS research have been and are used by practitioners. Unfortunately, the IS field does not possess the evidence with which to illustrate the impact of its research. While other administrative science disciplines have investigated how academic research has diffused into the world of practice and influenced its inhabitants, we know of no work in IS that has done so. This is an important question that IS academics should investigate.

In closing, we wish to end on a positive note. To reiterate, we are committed to and interested in doing good research. We strongly believe that the contributions of IS academics to practice can and

Table 2. Recommendations to Attain Relevance

	Recommendations	Dimensions of Relevance Addressed
Topic Selection	1. Focus on future interests of key stakeholders.	Interesting
	2. Identify topics from IS practice.	Interesting
	3. Identify, as an academic community, the core research issues that can influence practice in the future.	Current
Article's Purpose	4. Focus on the likely outcome (that can influence practice) rather than on inputs (academic and intellectual challenges) when choosing a research project.	Interesting Applicable
	5. Develop cumulative, theory-based, context-rich bodies of research to be able to make prescriptions and be proactive.	Applicable
	6. Develop frames of references to organize phenomena and provide contingency approaches to managerial action.	Applicable
	7. Portray research outputs in ways practitioners can utilize to justify and rationalize IT related decisions.	Applicable
Article's Readability	8. Use clear, simple, and concise style in the write-up.	Accessible
Editorial Process	9. Set the goal of publishing manuscript as being *both* rigorous and relevant.	Interesting Applicable Current Accessible

should rise. The IS academic community has a strong research infrastructure, highly respected journals, a growing body of good measurement instruments, and, as more cumulative work is done, an expanding portfolio of versatile theories to enable us to *react* more quickly and provide prescriptions to practice. Furthermore, in the last decade, North American IS journals have been less dogmatic about their emphasis on positivist (or "scientific") research. They are now more welcoming of qualitative and case-oriented studies that are more likely to produce the contextual and linguistic "hooks" so valuable in conveying the outcomes of rigorous IS research such that its value to practice is more easily recognized. For all these reasons, and with some extra effort and attention by IS academicians, we foresee in the future even stronger cooperation between the two solitudes of practice and academia.

Acknowledgements

We thank Geneviève Basselier for her valuable comments on this manuscript and Lynda Applegate for her excellent editorial guidance.

References

Ajzen, I. "The Theory of Planned Behavior," *Organizational Performance and Human Decision Processes* (50), 1991, pp. 79–211.

Applegate, L. M., McFarlan, F. W., and McKenney, J. L. *Corporate Information Systems Management,* Irwin, Chicago, 1996.

Astley, W. G., and Zammuto, R. F. "Organization Science, Managers, and Language Games," *Organization Science* (3:4), November 1992, pp. 443–460.

Benbasat, I., and Weber, R. "Rethinking Diversity in Information Systems Research," *Information Systems Research* (7:4) , December 1996, pp. 389–399.

Beyer, J. M., and Trice, H. M. "The Utilization Process: A Conceptual Framework and Synthesis of Empirical Findings," *Administrative Science Quarterly* (27), 1982, pp. 591–622.

Brancheau, J. C., Janz, B. D., and Wetherbe, J. C. "Key Issues in Information Systems Management: 1994–95 SIM Delphi Results," *MIS Quarterly* (20:2), June 1996, pp. 225–242.

Business Week. "Is Research in the Ivory Tower 'Fuzzy, Irrelevant, Pretentious?'" October 29, 1990, pp. 62–66.

Cheng, J. L. C., and McKinley, W. "Toward an Integration of Organization Research and Practice: A Contingency Study of Bureaucratic Control and Performance in Scientific Settings," *Administrative Science Quarterly* (28), 1983, pp. 85–100.

Daft, R. L., and Lewin A. Y. "Can Organization Studies Begin to Break out of the Normal Science Straightjacket? An Editorial Essay," *Organization Science* (1:1), 1990, pp. 1–9.

Davis, M. F. "That's Interesting: Towards a Phenomenology of Sociology and a Sociology of Phenomenology," *Philosophy of Social Science* (1), 1971, pp. 309–344.

Dearden, J. "MIS is a Mirage," *Harvard Business Review,* January-February 1972, pp. 90–99.

Fishbein, M., and Ajzen, I. *Belief, Attitude, Intention and Behavior: An Introduction to Theory and Research,* Addison-Wesley, Reading, MA, 1975.

Galliers, R. D. "Relevance and Rigour in Information Systems Research: Some Personal Reflections on Issues Facing the Information Systems Research Community," in *Business Process Re-Engineering, Proceedings of the IFIP TC8 Open Conference on Business Process Re-Engineering,* B. C. Glasson, I. T. Hawryszkiewycz, B. A. Underwood, R. A. Weber (eds.), Queensland, Australia, 1994, pp. 93–101.

Keen, P. W. "MIS Research: Reference Disciplines and a Cumulative Tradition," *Proceedings of the First International Conference on Information Systems,* E. McLean (ed.), Philadelphia, PA, 1980, pp. 9–18.

King, J. L., and Applegate, L. M. "Crisis in the Case Study Crisis: Marginal Diminishing Returns to Scale in the Quantitative-Qualitative Research Debate," *Information Systems and Qualitative Research,* A. S. Lee, J. Liebenau and J. I. DeGross (eds.), Chapman & Hall, London, 1997, pp. 28–30 (accessible from http://www.hbs.edu/applegate/cases/research/paper.html#patronage).

Nolan, R. L. "Managing the Computer Resource: A Stage Hypothesis," *Communications of the ACM* (16:7), July 1973, pp. 399–405.

Price, R. L. "A Customer's View of Organizational Literature," in *Publishing in the Organizational Sciences,* 2nd ed. L. L. Cummings and P. J. Frost (eds.), Sage Publications, Thousand Oaks, CA, 1995, pp. 98–107.

Robey, D. " Diversity in Information Systems Research: Threat, Promise and Responsibility," *Information Systems Research* (7:4), December 1996, pp. 400–408.

Saunders, C. "The Role of Business in IS Research," *Information Resources Management Journal,* Winter 1998, pp. 4–6.

Sears, P., and Pickler, L. "A Preliminary Study of the Practitioner Members of the Academy of Management," working paper, Baldwin Wallace College, Summer 1996.

Taylor, S., and Todd, P. "Understanding Information Technology Usage: A Test of Competing Models, *Information Systems Research* (6:2), June 1995, pp. 144–176.

Zmud, R. "Editor's Comments," *MIS Quarterly* (20:2), June 1996a, pp. xxi–xxiii.

Zmud, R. "Editor's Comments," *MIS Quarterly* (20:3), September 1996b, pp. xxxvii–xxxviii.

About the Authors

Izak Benbasat is CANFOR Professor of Management Information Systems and associate dean at the Faculty of Commerce and Business Administration, the University of British

Columbia, Vancouver, Canada. He holds a Ph.D. in management information systems from the University of Minnesota. His research interests are in understanding the managerial policies instrumental in the successful adoption of new information technologies, measuring information systems competence in line managers, evaluating human-computer interfaces, measuring the effectiveness of decision support tools, investigating the role of explanations in expert systems, and comparing methods for conducting information systems research.

Robert W. Zmud is Michael F. Price Professor of Management in the College of Business Administration at the University of Oklahoma. His research interests focus on the impact of information technology in facilitating a variety of organizational behaviors and on organizational efforts involved with planning, managing, and diffusing information technology. He holds a Ph.D. (management) from the University of Arizona, an M.S. (management) from M.I.T., and a B.A.E. (aerospace engineering) from the University of Virginia.