

EDITOR'S COMMENTS

Research in Information Systems: What We Haven't Learned

My three-year term as editor-in-chief draws to a close on December 31. I am glad that I have had this chance to serve our information systems research community. From the time I embarked on my research career, I have met scholars (both inside and outside the IS discipline) who gave and continue to give their time, energy, and support to me not because of any material benefit that they might ever receive in return, but because, apparently, their belief has been that giving to others is simply the right thing to do. Their efforts have had the effect of integrating me into the IS research community intellectually, socially, and politically. Being editor-in-chief has provided me opportunities to follow in the footsteps of these role models.

One final effort that I would like to make as editor-in-chief is to offer some observations on good directions for future IS research. A continuing concern in my own thinking has been what we, in the IS discipline, have learned and what we haven't learned. (An inspiration for this line of thinking is Attewell and Rule's seminal article, "Computing and Organizations: What We Know and What We Don't Know," published by *Communications of the ACM* in 1984.) When I mentioned "What We've Learned and What We Haven't Learned" as a possible editorial topic to my trusted colleague Bob Zmud, he astutely observed that the locution "What We've Learned" could imply that the IS field has completed and closed its investigations on particular topics. I therefore decided to frame the matter instead as "What We Haven't Learned," where all the named topics and issues would then receive positive attention as fertile grounds for future research. Rather than limit these observations to my own, I invited some leading IS researchers—the senior editors of *MIS Quarterly*—to share their thoughts. Below, I present their thoughts in their own words, followed by my observations.

Robert Zmud (*senior editor, January 1999–December 2001; editor-in-chief, 1995–1998*)

Considerable research attention has been given over the past decade to examinations of whether or not firms are obtaining benefits from investment in IT. In general, such studies have tended to emphasize one of two views:

- a macro view in which the value obtained from the total IT investment is assessed
- a micro view in which the value obtained from a single IT investment (an application, a platform, a technology, etc.) is assessed

Far less common have been examinations of how an organization should manage, in an on-going manner, its portfolio of IT investments. Anecdotal evidence indicates that a high portion of most firm's IT (operating plus capital) investment is targeted at business domains providing, at best, small returns. Many of these are mandatory, i.e., undertaken under regulatory or "partner" mandates, "fixes" to solve technical problems with critical, operational services, etc. Others of these poor-return investments are undertaken without any systematic questioning, and still others are characterized by an initial burst of systematic questioning but

then allowed to persist without further questioning once resources have been allocated. This leads to the following research issue:

How should organizations manage their portfolio of IT expenditures and investments in order to achieve high returns from these investments?

Dan Robey (senior editor, January 1999–December 2001)

Like Allen Lee, I will be leaving my current role with *MIS Quarterly* at the end of 2001. Retirement, however, does not imply disappearance. Rather, as a former senior editor I will have “lingering influence” and will continue to handle the manuscripts that I have been assigned before my term is up. I will continue to devote my energies to seeing that the quality and interest of articles published in the *Quarterly* remain high.

I offer two impressions gained from my service as SE. My first impression is that publication in *MISQ* has become almost too valued a goal among IS scholars. I think we are justly proud of the reputation that the journal currently enjoys, but authors should probably target their work to a greater variety of publication outlets. Researchers apparently feel pressure to publish their work in a top journal, such as *MISQ* or *Information Systems Research*. Yet, our field has matured to the point where other journals have become legitimate outlets for outstanding research. Clearly, there are not enough pages in the two or three top IS journals to satisfy the aspirations of every scholar in the field. More careful targeting of work to a variety of journals should pay off not only for submitting authors, who are likely to see their work actually published, but also for the field as a whole. Collectively, we need to establish the reputations of more journals as outlets for premier IS research.

My second impression is that our review process too often generates reasons not to publish manuscripts and too seldom helps authors to develop the potential in their manuscripts. Editors and reviewers may now hold *MISQ* in such high regard that they raise the “methodological bar” too high. The standards for both quantitative and qualitative research should increase, but in practice standards are always relative rather than absolute. All papers have methodological flaws, but they can still be valuable. The objective of any journal should be to publish articles, not to reject all submitted work. As a mature field, IS needs to encourage and develop promising ideas so that they satisfy evolving standards of scholarship.

What should aspiring authors seek to contribute to future volumes of *MISQ*? Rather than identifying one or more of my favorite topics, I would like to express a more general hope that future research published in *MISQ* be more interesting. What is interesting work? Sometimes it is work that exposes new problems and challenges, but more often interesting work casts new light on familiar intellectual puzzles that have not been resolved by prior research. Fewer and fewer papers published in *MISQ* seem to be interesting in this way. Among the most interesting papers are those that the editors select as “papers of the year” in *MISQ*. These papers, and the other papers nominated for this award each year, catch the imagination of readers because they show new ways to understand complicated phenomena. Award-winning papers are also rigorously researched, but their value typically derives from their unconventional departures from accepted wisdom. Paradoxically perhaps, a mature IS field needs to reinvent itself by opening new and interesting avenues for inquiry.

Richard Watson (*senior editor, MISQ Review, January 1998–December 2001*)

A Good Theory

Introduction

Lewin (1947) observes, "Nothing is so practical as a good theory." MIS is a practical discipline, and many of us teach skills (e.g., data modeling, systems analysis and design) that are central to the daily work of MIS professionals. Frequently, when we report our research, we include a section discussing the implications for practitioners. Even though practical relevance is a desirable attribute of our work, we also strive for theoretical contributions and methodological rigor. Over the years, as a discipline we have used a variety of theoretical bases from other disciplines and have developed a number of frameworks that classify MIS research. A core set of concepts or beliefs that we collectively agree are the foundations and driving forces of MIS research are emerging in our field. We need to accelerate this materialization of a "good grand theory."

In this short opinion, I would like to present a small set of key principles that I consider underlie the great majority of MIS research.

Six Key Principles

These six principles arise from my reflection on the central purpose of our discipline. My core belief is that MIS is focused on creating and implementing information systems that serve organizational goals. Each principle presented below is preceded by a brief rationale.

The goal of an IS is to improve organizational performance, which includes improving the performance of individuals or groups within the organization. There is no other rationale for building an IS. The goal of MIS is to increase an entity's efficiency (e.g., lowering transaction cost) or effectiveness (e.g., improving customer service) by developing and implementing high quality systems.

1. A quality information systems improves organizational performance

The selection of a system's underlying technology frequently determines its present and future capabilities (e.g., the choice between object or relational DBMS). MIS scholars are concerned with identifying the consequences of technology choices upon systems success.

2. The quality of an information system is determined by its foundational technology

The systems development life cycle (SDLC) is a long established tool, and thus it is not surprising that we should expect that it implicitly encompasses some of the fundamental concepts of MIS. First, the quality of a system's design is determined by how accurately requirements are captured. The quality of the recording of requirements determines how well they can be interpreted and validated. Second, design quality is a function of the application of coupling (how cleanly the modules in a system are separated from one another) and cohesion (the strength of functional relatedness of elements within a module), which are the basis of object-orientation principles.

3. The quality of information systems design is determined by data collection and representation methods

4. The quality of an information system's design is determined by coupling and cohesion

Implementation of an information system disturbs the socio-technical system of an organization. The extent of this perturbation determines the difficulty of the change and the management skills that must be applied.

5. The success of an information system's implementation is determined by the management of the conversion from the old and new system

Particularly for systems that are voluntary, adoption is determined by the fit between the new system and the organizational tasks it is designed to improve. Even in mandatory systems, there must be a high degree of concordance between the technology and the task.

6. The extent of an information system's use is determined by the degree to which it improves task performance

Why Do We Need Theory?

A theory of MIS will make explicit the shared beliefs of our community. It will reinforce the quality of our scholarship and foster debate about the central ideas of our discipline. Our sense of distinctiveness and position in the academic community will be enhanced.

I believe that it is time to start the discussion, and put forward the preceding set of six principles as a starting point for community debate. I realize there is a danger, assuming my challenge is accepted, in that we might anchor and adjust from this set. Thus, our community needs to first consider other starting sets.

In the past decade, we have honed our skills as researchers, increased the rigor of work, and embraced a wide variety of research methods and theoretical foundations from other fields. We have not, in my opinion, sufficiently nurtured the very core of our field. To paraphrase Socrates, an unexamined field is not worth researching, to know the heart of its discipline is the primary goal of any thinking community of scholars.

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Ilze Zigurs (*senior editor, January 2000–December 2002*)

Each of us involved in conducting MIS research follows a very personal path over time and on a variety of dimensions: the goals we have for our research, the topics we are interested in studying, the approaches we take, the value we place on collaboration, the targets we choose to disseminate our work, and the personal gratification we receive from different aspects of what we do, to name just a few. The path of an individual piece of research is equally varied, as it moves from a question about the world, to a more concrete formulation of a topic, to data collection, analysis, write-up, feedback and review cycles, presentations, and the ultimate though almost anti-climactic goal of appearance in published form. All these individual goals, approaches, and aspirations evolve in the context of the communities, organizations, and collaborative influences that also guide our work. Our enthusiasm or cynicism about different aspects of this process waxes and wanes, as we learn from and reflect on our experiences and contexts.

Our ideas about what matters also shape our beliefs about what we know and don't know. One person might say that we have learned a great deal about groupware and the circumstances under which different types of tools are effective for different groups and group activities. Others might disagree with that statement, and some perhaps fervently. For instance, how easily and confidently (and briefly) can we reply to a manager who asks the question: "What groupware tools should I use to get maximum performance from my far-flung software design teams?" Our answer rightly is, "It depends," but what can we easily say about the dependencies? How rich is our contextual knowledge of collaboration technologies in use? How many levels or dimensions of context can we address? How well does understanding from one context translate to another? Do we know what the key leverage points are?

Contextual understanding is important for any type of technology, not just groupware tools. That message is not new. But how well have we translated that message into action? Collaborative technologies present a particularly salient example because they cover such a broad range of potential tools, uses, and users, mutually changing one another through on-going group processes. As with previous kinds of systems, we have attempted to categorize, classify, and create taxonomies of group systems and group tasks. We have good examples of researchers sharing instruments, replicating tasks in different contexts, and conducting programmatic research. Useful theoretical perspectives have also emerged for understanding some of the dependencies in the context of groupware use. All these efforts have been very valuable and a critical part of the evolution of knowledge, but we have much more to learn and we can go further.

An essential part of going further is a greater sense of community and an active encouragement of diversity in perspectives, techniques, and styles. Examining "dependencies" might conjure a boxes-and-arrows diagram for one person, a rich text description for another, and some entirely new form for a third. This message is not new either. But old habits not only die hard, they continue to be refreshed and reinforced in how we train each new generation of doctoral students. That training still differs markedly in different parts of the information systems world. Excellent efforts to communicate and engage those differences have been made, by developing conference colloquia, by ensuring diverse reviewer panels, and by our own global communication network. Managing and making sense of different perspectives is much more difficult than interacting with a group of people who all think the same way. But active and positive engagement with differences, with paradoxes, and with each other is the only way that we will ever achieve a truly rich contextual understanding of technology development, use, and impacts.

Kwok-Kee Wei (*senior editor, July 2000–June 2003*)

While much attention has been given to understanding the adoption, implementation, and impact of information technology in the last two decades by IS academics, very few studies have focused on a major area of system development: the delivery process of these IT applications. System development represents a fundamental responsibility of the corporate IS department, and is a distinctive area of the IS discipline that researchers from other disciplines would find difficult to penetrate. Yet, there remains the need for more research on the process of delivering IT applications. Much has changed in the practice of system development: types of users of IT applications have shifted from internal to external (e.g., suppliers, customers, individual consumers); new tools and new ways of system development (e.g., open-source development, outsourcing, application service providers) have emerged; and new management issues concerning system development have arisen. We need to gather some insights into the impact of these new tools on programmers'/analysts' productivity, into the effectiveness of these new methodologies and paradigms, and into the management of the new system development processes (arising from the involvement of new players such as graphic designers and artists, animation experts, consumers, etc., and from the use of new tools). These insights are important in three ways: guiding managerial practices; supporting and leading new system development efforts; and revising our IS curricula and educating future IS graduates.

Another area which I feel is worthy of exploration is the IS interface with marketing and economics. As IS expands from internal use to supporting key business functions such as selling and marketing and also to forming a marketplace for locating trading partners and facilitating transactions between customers and suppliers, I believe it would make sense for IS researchers to strengthen their links with their marketing and economics counterparts in order to solidify the relationship between IS and other disciplines.

Michael Myers (*senior editor, January 2001–December 2003*)

I believe one of the most important lessons that we have learned as a field is to value a diversity of research methods and approaches in IS. Given the complexity and richness of the subject matter, we have realized that both qualitative and quantitative methods are essential. I think I am on firm ground in stating that all the senior editors and associate editors of our top journals welcome positivist, interpretive, and critical research articles, as long as the research itself is of high quality. In my view, this is one of the strengths of our field, and sets us apart from many other disciplines where this is not the case.

However, if we are to maintain this happy state of affairs, I believe we need to go on to the next level. That is, IS researchers from different research perspectives and approaches need to learn to work together within the scope of a single research project or within a particular research area. This is because the nature of our subject matter demands multiple perspectives and approaches. I am aware of one or two colleagues who have started to do this already, but they are definitely in the minority. If we can learn to collaborate without compromising the quality of the research, then I am confident that the IS field will go from strength to strength and may well become an example to others.

V. Sambamurthy (*senior editor, January 2001–December 2003*)

The rise and the fall of the dot coms have sensitized most firms to how advanced information technologies could significantly transform and redefine their business ecosystems. Key transformational aspects include: (1) how relationships are handled with customers, (2) how coalitions are architected with suppliers and other external partners to create novel business models (e.g., direct to the customer), and (3) how capabilities for globalization, speed, flexibility, innovation, and cost economy are developed through IT-enabled processes, relationships, and knowledge. Furthermore, dramatic transformations in the IT industry have: (1) accelerated the pace of innovation and obsolescence in products, services, and skills, and (2) created new business models (e.g., applications services providers, web hosting partners) and technology partners. As a result, changes are occurring in organizational structures, processes, and systems associated with the management and use of information technologies.

Therefore, the following topic is a fertile area for further research:

What business and IT capabilities, structures, and processes are associated with continued success in leveraging information technologies for superior performance through innovation, globalization, speed-to-market, operational excellence, cost leadership, and customer intimacy?

I see two types of research strategies for this topic. One strategy adopts a global perspective on firm capabilities, structures, and processes. Some IS researchers have pursued this strategy by using the resource-based theories of the firm and the dynamic capabilities perspective. Although we are beginning to gain knowledge here, there is room for more research using surveys, case studies, and archival data. In particular, there is need for research that examines how and why these capabilities influence superior firm performance. Relatedly, there is a need for creative measures of firm performance, beyond self-

reported perceptual measures or pure accounting measures of revenue, profit, or ROI. Examples of desired measures of firm performance include EVA, Tobin's q, rate of product or process innovation, agility, and time-to-market.

Further, there is need for research that examines the complementarities among business and IT capabilities in influencing firm performance. For instance, large firms in particular are focusing on cross-divisional integration of customer relationships, knowledge, and business processes. How do specific business and IT capabilities facilitate cross-divisional integration and what are the consequences for firm performance?

Finally, a fundamental IT-enabled shift away from internal value chains toward value constellations has occurred. Groups of collaborative firms with complementary capabilities and knowledge operate as tight-knit networks. Firms such as Dell, Cisco, or Nortel compete through their IT-enabled business value webs of virtual integration. Theories of strategic alliances and network structures should be utilized to examine the strategic role of IT in the formation, maintenance, evolution, and competitiveness of such value constellations.

As opposed to the global approach, an alternative perspective is to examine specific capabilities, structures, and processes for leveraging IT-based innovation in key enterprise activities such as customer relationships, logistics and supply chain, product innovation, or manufacturing. Relatively much less research has occurred here. IS researchers should combine knowledge from IT management and strategy with appropriate theories from marketing, supply chain, distribution and logistics, procurement, product innovation, or manufacturing to not only identify complementary functional and IT capabilities, but also develop appropriate measures of firm performance. For example, global firms are outsourcing their supply chains and logistics for different product lines and markets to lead logistics providers. The supply chain solution space is shifting from solutions produced and marketed by a single firm toward solutions assembled across firms with complementary capabilities. Further, these supply chain solutions are transforming from a traditional focus on the physical, flow of goods toward exploitation of complementary information, physical and financial flows across the supply chain. Research is needed that examines the appropriate IT-enabled capabilities, structures, and processes in such solution architectures. Similarly, theories of consumer behavior and marketing channels should be utilized in examining the role of IT in influencing the effectiveness of customer relationships and multi-channel go-to-market strategies in firms.

Finally, a few comments about the publishability of research on this topic. My belief is that publishable research must combine theory with field-based emerging insights. For example, relevant ideas about business or IT capabilities are not likely to emerge simply from a literature review. Researchers must develop their insights by examining the trade press, working with a few companies, talking to senior executives, and then blending these emerging insights with theory and prior literature. Second, although researchers should remain alert to the psychometric and statistical criteria of rigor (for example, norms for Cronbach's alpha or goodness of fit), some flexibility in how we view research is also imperative. Not always will it be possible to define constructs with precision or meet all the social science criteria of rigor. Authors and reviewers must balance attention between descriptive relevance ("Did the research capture an elusive phenomenon reasonably well?") and empirical rigor ("Did the research meet all the thresholds of statistical and psychometric rigor?"). While I am not advocating the loosening of empirical rigor, my belief is that this research will be challenging when it comes to construct definition, operationalization, and testing. We can only accumulate knowledge through initial moves aimed at descriptive relevance and subsequent progressive moves to empirical rigor. Finally, as more researchers devote their attention toward the topic of IT-enabled business strategies and value ecosystems, it is important to nurture the cumulative tradition through the sharing of instruments, data sets, and methodological approaches.

Jane Webster (*senior editor of MISQ Review, January 2001–December 2003*)¹

We haven't learned how to take stock of the MIS field by reviewing where we are and where we need to go next. The good news is that the field has come a long way in terms of conducting empirical research. In a comparison of MIS "paradigm consensus," or the extent to which researchers share beliefs, values, and techniques (Kuhn 1970) with established scientific fields like Chemistry and with other older management fields like Organizational Behavior (OB), Webster and Starbuck (1988) concluded that MIS was probably pre-paradigmatic in the 1970s.

This has changed greatly over the past 20 years. For instance, the following graphs compare several measures of paradigm consensus in 1990 and 2000 for research articles in *MIS Quarterly* (*MISQ*: a leading journal in MIS) with those in the *Academy of Management Journal* (*AMJ*: a leading empirical journal in OB). Specifically, only "Theory and Research" articles in *MISQ* and full articles in *AMJ* (not research notes) were included for the consensus measures. These measures represent the number of references per article (where a larger number indicates higher consensus), the percentage of references to the same journal (where a higher percentage indicates higher consensus), and the citing half-life of references or the median age of references to a journal (where higher half-lives indicate higher consensus).

As can be seen, MIS research exhibited similar referencing characteristics to OB research in both 1990 and 2000. The graphs also demonstrate that the volume of research articles is growing in MIS. For instance, the first issue of *MISQ* in 2000 contained six research articles. This is the same number as all of 1990 in which *MISQ* contained many other types of articles, such as "Application" papers. This represents another indication that research papers were valued more in 2000. However, we continue to observe a smaller volume of research articles overall in *MISQ* as compared with *AMJ*, probably due to the smaller number of researchers in MIS and its relative youth as a field. Nevertheless, we can conclude from these analyses that we are seeing a greater focus on empirical research articles in MIS and, from this perspective, we can deduce that the field appears to be coalescing.

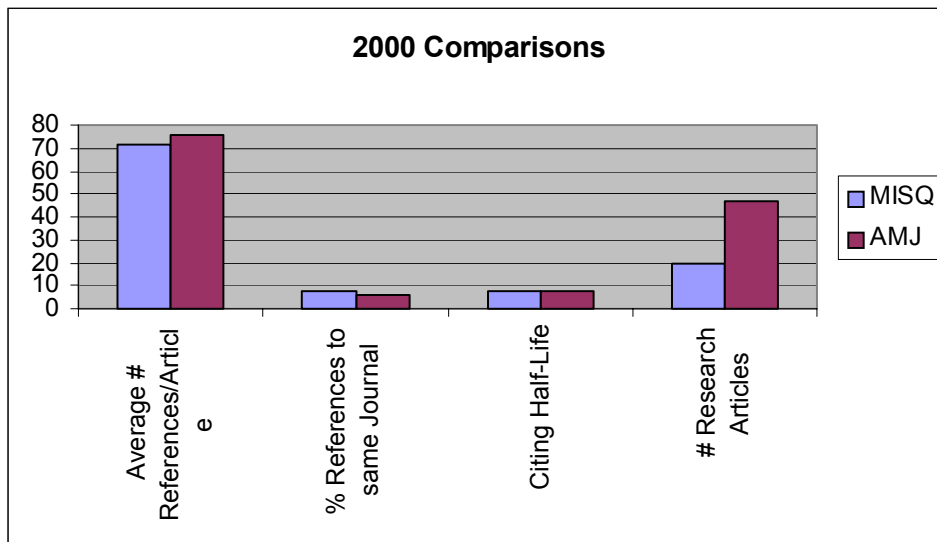
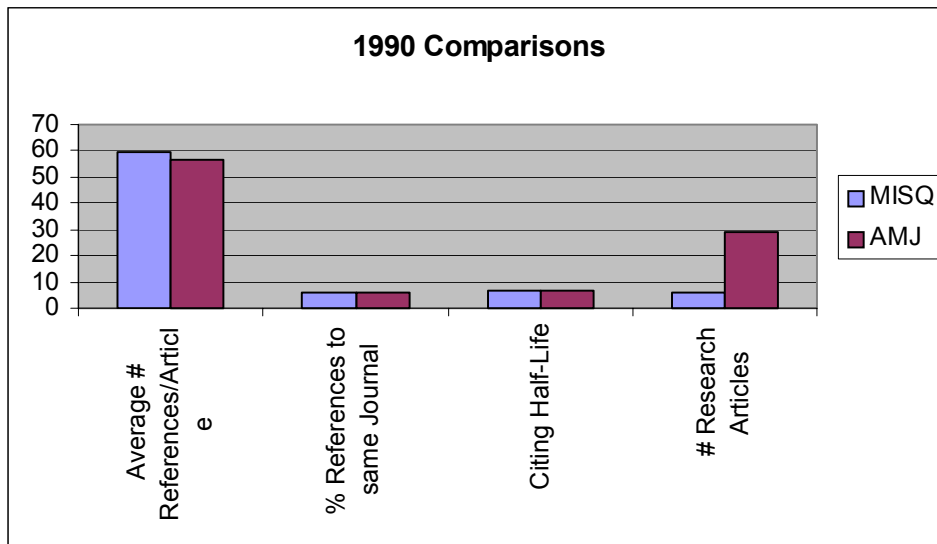
The bad news is that MIS focuses little on meta-theory. That is, what we see less of in MIS (as compared with older fields such as OB) is an emphasis on review and theoretical articles—that is, on articles that summarize and move the field forward. There have been few MIS review papers, notable exceptions being articles such as Griffith (1999), Malone and Crowston (1994), and Robey et al. (2000). However, at its present stage of theory development, MIS scholars should be focusing more on the development of better conceptual frameworks, including well-reasoned propositions. Thus, we suggest that an "ideal" article would critically review the past literature in the area and related areas (e.g., Malone and Crowston 1994) and develop a model and propositions to guide future research (e.g., Griffith 1999).

Because of its relative youth, MIS does not have an outlet dedicated to review articles as other management fields do, such as the *Academy of Management Review* (*AMR*). However, *MISQ* does have a mandate to encourage such theoretical articles through a new department, appropriately called *MISQ Review*. MIS researchers can learn more about this new department at

<http://www.misq.org/misreview/announce.html>

We encourage researchers to explore review articles in other outlets as possible models, such as those in *AMR*. Now is time for researchers to begin to take stock of the MIS field and identify where we can add to the development of shared theory and research.

¹Ann Frances Cameron, a graduate student at Queen's School of Business, provided much-needed assistance in compiling the comparative paradigm consensus statistics for *Academy of Management Journal* and *MIS Quarterly*.



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Ritu Agarwal (*senior editor, July 2001–June 2004*)

There is little doubt that we are poised at a critical and exciting juncture in the development of our discipline. Never before has the role of IT in organizations been more central and fundamental to organizational processes, strategy, and, indeed, success. And never before have organizations struggled as hard as they do today in an attempt to derive value from information technologies. As a consequence, “new” IT management challenges and concerns present themselves each day, and “old” IT challenges take on a renewed significance. I would like to focus on two areas that could plausibly be considered both “old” and “new” simultaneously.

In my opinion, an enduring question for IS researchers and a fertile area for continued attention and research emphasis is the phenomenon of IT innovation. In particular, I believe we need to better understand how organizations facilitate and promote innovation and creativity in the use and application of IT to achieve strategic success as well as operational excellence. This is especially true in today’s digital economy where, arguably, what drives organizational transformation is the frequency and persistence of IT-based innovation. Such innovation could manifest itself at many levels, ranging from a fundamental shift in an organization’s business model fueled and enabled by IT, to the introduction of a new ERP system that transforms the supply chain, to the diffusion of a new communication technology that enables new forms of organization and work.

IT-based innovation encompasses several interesting and relevant research questions. We might ask “what managerial behaviors, internal processes, and incentive structures foster creative use of IT among organizational members?” How precisely do we define and measure creativity in IT? Should creativity be a “process” construct or an outcome? Or we might explore how alternative structural arrangements facilitate or inhibit IT-based innovation among an organization’s executive core and other members. Are team-based organizations more innovative in the use of IT than functionally based organizations? What structures promote the initiation of IT-based innovation and how are these different from those that promote its effective assimilation? How do creative ideas about the application of IT get enriched and adapted as they flow through interpersonal communication networks throughout the organization, and what impedes this flow? We could ponder whether successful IT innovation is market driven (i.e., in response to competitive maneuvers), if it arises as a result of a fortuitous confluence of factors that are outside managerial action and control, or if it is best conceptualized as an all-pervasive grassroots activity that permeates all levels of an organization. As questions arise about the boundaries of a firm in a netcentric environment, we could study if IT innovation occurs within a firm or within a value network of multiple firms. Theories and concepts from sociology, economics, and organization theory can assist IS researchers in the formulation of conceptual models that help us gain insights into these questions.

A second related area that I foresee as becoming increasingly relevant is the management of IT human capital. I define IT human capital as the accumulated stock of tacit and explicit knowledge about IT that is resident not only within individuals who might typically be considered IT professionals, but also in other organizational members whose primary roles are outside the IT function. Contemporary theories of the firm such as the resource-based view underscore the notion that sustainable competitive advantage is attainable only through rare, non-imitable, and imperfectly distributed resources. Clearly IT skills and competencies, as well as the business acumen to creatively combine IT knowledge with business opportunities are representative of such critical assets and need to be acquired, developed, and nurtured appropriately. Against a backdrop of rapidly changing technologies that render existing competencies obsolete, and emerging business opportunities that have to be seized within a very short window, organizations face a considerable challenge in ensuring that they possess IT human capital that is current, relevant, and responsive.

The strategic management of IT human capital poses multiple managerial dilemmas that are deserving of rigorous research. Several examples follow. Managers struggle to identify the components of an appropriate competency IT bundle for their organizations. They grapple with issues related to the sourcing of IT competencies and ask what components of IT knowledge are best developed in-house and what components can be more effectively sourced from other partners external to the firm. They wonder how to avoid the high costs associated with persistent turnover among IT staff and what human resource and work practices might entice valuable IT human capital to remain with the firm. They question whether IT professionals have distinct characteristics that demand new ways of management. Here, theories from economics and organizational behavior could provide fruitful insights for researchers interested in examining these phenomena.

I do not wish to suggest that IS researchers have not addressed these two areas in their investigations. What I do wish to emphasize is that richer, more rigorous, and field-based research in these areas is likely to have a substantial impact both on the advancement of theory development in our discipline as well as on the practice of IT management. Research at the organizational level of analysis would be particularly valuable here, as these phenomena have been less studied in this context. While I recognize the pragmatic difficulties of executing organizational level studies, I believe such research is likely to be well received by editors, reviewers, and practitioners alike.

I affirm the observations that the senior editors offer and I wish to emphasize a point on which some of them have touched—namely, the collaborative, social, and even political dimensions of our research community. I also believe that there remains the need for the IS research mainstream to embrace action research, design science research, and the systems approach. These are dimensions and research genres on which I have commented, at length, in keynote addresses at conferences, previous editorials, and other writings.

A theme that recurs in my thinking whenever I am doing my work as editor-in-chief relates to the “big picture.” Often, when I am reading a manuscript, the reviewers’ reports, and the associate editor’s report, what I see at work is the operation of a very special and unique organization: the organization of IS researchers. I believe that we, who are members of this organization, are people just like those whom we study in business organizations and that, therefore, the brilliant research imaginations that we take to our research settings can, and should, apply no less in such matters as how we come to understand ourselves as researchers, what makes a research paper significant, and how we should proceed to move our IS research field forward. Such an understanding would go beyond the standard, but necessary, aspects of what good theory, good method, and good topics are, and deliver us to the realm of what it is about our research community’s own organizational and technological infrastructure that has been good, and bad, for the IS research that we have done and that we still need to do.

It is with these thoughts that I pause, step back, and reflect on how it is that I managed to meet certain people who so selflessly helped me when I was a young scholar, how it is that our research community happened to be organized so as to allow me to follow my heart despite extensive resistance to qualitative research, and how it is that I came to be chosen editor-in-chief. What remains for me to learn are, among other things, lessons from my own personal case study as editor-in-chief—lessons that I hope to be able to use in continuing the work of those who have helped me, especially the work of building and strengthening the organization of IS researchers.

Allen S. Lee
Editor-in-Chief

