EDITOR'S COMMENTS ▮▮▮▮▮▮▮▮▮▮

Still Desperately Seeking the IT Artifact ▮▮▮▮▮▮▮

Since the early 1970s, scholars within the information systems discipline have been concerned about the nature of and the future of the discipline. For instance, in 1972, John Dearden from the Harvard Business School wrote an article in the *Harvard Business Review* entitled, "MIS Is a Mirage."[1] Dearden argued (p. 90) that management information systems as a "conceptual entity" were "embedded in a mish mash of fuzzy thinking and incomprehensible jargon." Dearden's comments sparked substantial controversy, and among some scholars they were roundly condemned as destructive and unhelpful to a nascent discipline. Nonetheless, they were a harbinger of concerns that have persisted to this day.

As a young scholar in the 1980s, I confronted my own personal and professional crisis with the information systems discipline. My secondary school education had been heavily oriented to physics, chemistry, and mathematics, and my initial tertiary education had been in economics. In the information systems discipline, I searched in vain for the powerful, general theories that underpinned research in disciplines like physics and economics. As I indicated in my March 2003 editorial, I first voiced my concerns publicly in a presentation I gave to attendees at the 1983 Doctoral Consortium of the International Conference on Information Systems.[2] I learned subsequently that other colleagues harbored similar concerns to mine about the discipline. For instance, Phillip Ein-Dor[3] and Ali Farhoomand[4] had written papers that articulated various problems they perceived were undermining progress within the discipline.

In the ensuing years, concerns about the information systems discipline have waxed and waned. Recently, however, they have emerged again in various forms. In a research commentary published in the June 2001 issue of *Information Systems Research* entitled "Desperately Seeking the 'IT' in IT Research–A Call to Theorizing the IT Artifact," Wanda Orlikowski and Suzanne Iacono expressed concerns about the lack of centrality of the information technology artifact in much of the information systems research that has been conducted to date. In 2002, the Association to Advance Collegiate Schools of Business (AACSB) for a time contemplated removing information systems as a core course in its undergraduate and graduate accreditation standards. Only intense lobbying by the Association for Information Systems averted this outcome. At the 2002 International Conference on Information Systems, the then-President of the Association for Information Systems, Phillip Ein-Dor, convened a meeting to discuss concerns about the state of the discipline. At the conference, two lively panel sessions were held that addressed various aspects of the state of the information systems discipline—one organized by Wanda Orlikowski and

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[1]J. Dearden, "MIS Is a Mirage," *Harvard Business Review* (50:1), 1972, pp. 90-99.

[2]R. Weber, "Toward a Theory of Artifacts: A Paradigmatic Basis for Information Systems Research," *The Journal of Information Systems* (1:2), 1987, pp. 3-19.

[3]P. Ein-Dor, "An Epistemological Approach to the Theory of Information Systems," *Proceedings of the 18th Annual Meeting of the Decision Sciences Institute*, Honolulu, Hawaii, November 1986, pp. 563-565.

[4]A. Farhoomand, "Scientific Progress in Management Information Systems," *Data Base* (18:4), 1987, pp. 48-56.

Suzanne Iacono in response to the interest generated among colleagues by their research commentary, and another organized by Rick Watson and Elena Karahanna to canvass the need for developing a theory of information systems. Earlier this year, Phillip Ein-Dor, as President of the Association for Information Systems, also convened a committee on information systems disciplinary matters to advise and prepare a report for the Council of the Association for Information Systems on the state of the information systems discipline.

In this issue of the *MIS Quarterly*, we have a paper in the Issues and Opinions section by two senior colleagues within the information systems discipline, Izak Benbasat and Bob Zmud, which provides their perspective on the "identity crisis" they believe faces our discipline. In their paper, Izak and Bob propose a "core set of properties" for the information systems discipline that they believe provides a basis for establishing an identity for our discipline. From conversations I have had with colleagues who have already read earlier versions of Izak and Bob's paper, I am aware that some of the views expressed in the paper are controversial. I hope, therefore, that Izak and Bob's paper will provide a stimulus for further debate about the nature and meaningfulness of the research in which we engage within the information systems discipline. As I indicated in my March 2003 editorial statement in the *MIS Quarterly*, I believe we need to focus more on "the problem of the problem" in the conduct of our research. In other words, we need to be more circumspect about our choice of research topics and their implications for the long-term future of our discipline.

Is There a Crisis in the Information Systems Discipline?

I doubt we will ever achieve unanimity within the information systems discipline about whether we have a serious identity problem within the discipline. In 1989, for example, Claude Banville and Maurice Landry characterized the discipline as a "fragmented adhocracy"—a discipline characterized by a low level of political independence among its members, a low level of conceptual coherence, and a low level of coherence in terms of standardization of research methods and interpretation of results.[5] Rudy Hirschheim, Heinz Klein, and Kalle Lyytinen have also argued that a "call for a unifying paradigm is not desirable."[6] Similarly, Dan Robey would prefer to embrace diversity within the discipline than attempt to unify it.[7] From the discourse that has occurred at number of panel discussions and meetings that I have attended recently, it is clear that many colleagues, some of whom are very senior members of our discipline, believe the problems pertaining to identity that others have articulated are vacuous. Some colleagues are also quick to point out that the sorts of concerns we have about identity within the information systems discipline are typical concerns in other disciplines (not that this stance provides sufficient justification, in my view, for ignoring our concerns).

In any event, achieving unanimity about whether we have a problem of disciplinary identity is not important in my view. In this regard, I have a Darwinian perspective on the matter: let the different "species" coexist for the moment, and over time only the fittest will survive. As scholars we need to argue our viewpoints in forthright, constructive, and respectful ways. In due course, we will either convince colleagues (especi-

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[5]C. Banville and M. Landry, "Can the Field of MIS Be Disciplined?," *Communications of the ACM* (32:1), 1989, pp. 48-60.

[6]R. Hirschheim, H. K. Klein, and K. Lyytinen, "Exploring the Intellectual Structures of Information Systems Development: A Social Action Theoretic Perspective," *Accounting, Management and Information Technologies* (6:1/2), 1996, pp. 1-64.

[7]D. Robey, "Research Commentary: Diversity in Information Systems Research: Threat, Opportunity, and Responsibility," *Information Systems Research* (7:4), 1996, pp. 400-408.

ally younger colleagues) of our views, or they will be attracted to other views that they find more compelling. We need to accept that colleagues (and ourselves for that matter) may change their views and not denigrate them if they do so. While the debate over disciplinary identity is now longstanding and somewhat hackneyed, nonetheless I believe it remains a healthy aspect of our discipline. When we engage with it, we are reminded that we ought always to reflect carefully on what we do as scholars.

How Might the Information Systems Discipline Establish an "Identity"?

Benbasat and Zmud argue that a *collective*, such as a discipline, establishes its identity via "a set of important, essential core properties that distinguish the collective from others in its environment." They define the *core* of a discipline as the "central character, that connotes, in a distinctive manner, the essence" of the discipline. For Izak and Bob the key to establishing an *identity* for the information discipline is to articulate its core.

Of course, their basis for establishing the identity of a discipline provokes the question: *How* do we identify the core of a discipline? Benbasat and Zmud answer this question in two ways. First, they "conceptualize the IT artifact" in terms of its application to a task embedded within a structure that in turn is embedded within a context. Second, they look at the phenomena studied by information systems scholars and, in a sense, *induce* the core properties of the information systems discipline based on the phenomena that have been and are being studied.

I am somewhat at odds with Izak and Bob in terms of how we identify the core of a discipline and the way that the discipline establishes its identity. By looking at what information systems scholars do as a basis for identifying the core, we simply perpetuate our current problems with disciplinary identity. I doubt an *inductive* approach to identifying the core of our discipline will work successfully.

Instead, I believe we need to follow a two-step process. First, I agree with Izak and Bob that we need to identify and to classify the types of phenomena that are the focus of researchers who call themselves members of the information systems discipline. In some sense, presumably these researchers must believe the phenomena they are studying are special in some way. In the search for the core of the information systems discipline I agree that these phenomena are a good place to start.

Identifying and classifying these phenomena, however, is only half the story in my opinion. The second step involves a crucial test. Specifically, we need to ask whether other disciplines have provided theories (or theories that might be adapted in a relatively straightforward way) to account satisfactorily for the phenomena we have identified. If such theories exist, in my view the information systems discipline will not be capable of establishing a separate identity by claiming these phenomena as a component of its core. If we include them in our core, we will forever be characterized as a reference-theory discipline—one that simply borrows theories from other disciplines to explain or predict phenomena that interest us. As a result, the problems that have occurred because we have failed to establish a disciplinary identity (described by Izak and Bob in their paper) will become even more endemic.

For instance, assume that the things that are our focus are "information systems" and "users of information systems." Furthermore, assume the properties on which we focus are the various characteristics of human-computer interfaces that might be used with information systems and various characteristics of users that manifest their performance in their use of human-computer interfaces. At first glance, these phenomena might seem like reasonable phenomena for an information systems scholar to address, especially relationships among them. If we conclude that psychological theories can be used or readily

adapted to account for user performance with different types of human-computer interfaces, however, we will have done little to contribute to the core of the information systems discipline. Instead, at a basic-research level, we will be contributing to the discipline of psychology rather than the discipline of information systems.

Clearly, we are off to a promising start when seeking to articulate the core of a discipline if we can identify things or properties of things that are not the focus of other disciplines. Nevertheless, I believe that such an outcome is neither a necessary nor sufficient condition for identifying the core of a discipline. If we find that we can use theories from other disciplines to account for the behavior of these new things or new properties of things, we have no basis for the core of the information systems discipline. For me, the key to identifying a core is finding phenomena where existing theories are non-existent or deficient. The key to *creating* the core is then building theory that is novel—theory that colleagues in other disciplines will acknowledge as belonging to the information systems discipline. Conceivably, it might be a completely new theory—one that has no genesis in other disciplines. I suspect that the more-likely outcome is that the theory is a marked adaptation of or extension of a theory that has its roots in another discipline. It will be sufficiently different from its ancestors, however, that ownership will be ascribed to the information systems discipline.

To establish a core for the information systems discipline, I believe we need two creative acts. First, we need to "see" things or phenomena that are not the focus of other disciplines. Alternatively, we need to see things or phenomena that are the focus of other disciplines in new, rich, insightful ways (see the world through a dramatically different lens). Second, we need to build powerful, generic theories to account for these phenomena that are not applications of theories from other disciplines or straightforward extensions of these theories. The "value-add" associated with the theories we propose must be sufficient for other disciplines to ascribe *ownership* of these theories to the information systems discipline. In other words, we cannot establish our identity as a discipline by fiat. Instead, our identity will emerge only as the outcome of a social process—one in which members of other disciplines acknowledge that the theoretical contributions we have made are important to their own understanding and prediction of some phenomena.

In short, I believe the identity of a discipline is established through the contributions it makes to theory. The core phenomena of the discipline are circumscribed via the theories "owned" by the discipline that account for these phenomena. Disciplinary identity and ownership of theories that other disciplines deem important are linked inextricably. Likewise, the theories owned by a discipline and its core phenomena are linked inextricably.

Information Technology-Related Phenomena: A Misplaced Focus?

For those of us who seek the core of the information systems discipline, there is one line of enquiry that I believe will be unproductive and thus should be avoided. Specifically, I believe the core, if one exists, will *not* lie in theories that account for *information technology*-related phenomena. Rather, it will lie in theories that account for *information systems*-related phenomena. The two sets of phenomena are *not* the same. They are fundamentally different. Moreover, for the moment I believe our understanding the likely different implications of our researching the two sets of phenomena is important to our chances of success in creating theories that differentiate us from other disciplines and thus finding the core of our discipline.

If we believe a theory of the core lies in information technology-related phenomena, presumably we believe theories of the core also exist for other sorts of technology-related phenomena—for instance, a theory of the core of automobile-related phenomena, or space shuttle-related phenomena, or electric toothbrush-

related phenomena. I have canvassed senior colleagues in the disciplines of sociology and anthropology about whether theories of the core have been developed for these sorts of technology-related phenomena. They were somewhat bemused by my questions, but nonetheless they indicated they knew of no such theories of the type I was describing. Clearly, much research has been undertaken on the impacts of various types of technologies on humans. Where this research is informed by theory apparently existing social science theories (or some adaptation thereof) have sufficed to explain or predict the phenomena of interest.

It is true that in some cases the emergence of new technologies has spawned new disciplines. For example, the discipline of computer science owes its existence to the invention of computers. Based on conversations I have had with colleagues in computer science, however, it seems the nature of the core of the computer science discipline remains elusive (although I believe good progress has been made). Ultimately, the knowledge created by computer scientists that has the most impact may be important applications of theory from other disciplines or engineering-based knowledge rather than theory-based knowledge. If this outcome occurs, a theory of the core of the computer science discipline might not emerge.

I hoped to be proved wrong about my belief that a theory of the core of the information systems discipline does not lie in information technology-related phenomena. If information technology-related phenomena formed part of the core of our discipline, we would have a richer, larger core and potentially a more interesting and exciting discipline. Perhaps one way forward is to try to identify a set of generic characteristics of information technology that cause existing theories about technology to fail. In this way, the basis for building new theory and the identity of the information systems discipline would be established. In some ways, the somewhat tentative status of computer science as a discipline with its own identity has been founded on generic characteristics of information technology that differentiate it from other technologies. For example, compiler theory has been developed because of special needs associated with computing machines as opposed to other sorts of machines.

Again, I am not hopeful of this path leading to productive outcomes for the information systems discipline. Our discipline is called the information systems discipline, *not* the information technology discipline. Surely our focus ought to be information systems, therefore, and not information technology. Information technology is simply the platform or resource on which we build information systems.

What Is the Core of the Information Systems Discipline?

In the mid-1980s, Yair Wand and I wrestled at length with the question of what constitutes the core of the information systems discipline.[8] As an initial step, we followed the course of action proposed by Benbasat and Zmud: we classified the phenomena that information systems researchers had studied and were studying at the time. For the different classes of phenomena we then applied the second, critical test I described above. We asked whether theories from other disciplines were likely to account for the different classes of phenomena we had identified. After a long period of discernment, we found we could identify only one class of phenomena for which theories sourced from other disciplines seemed deficient— namely, phenomena associated with building conceptual models and designing databases. This was an "unhappy" conclusion for us for several reasons. First, through our undergraduate and postgraduate studies, neither of us had built the knowledge base that allowed us to research these sorts of phenomena with confidence.

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[8]See, for example, Y. Wand and R. Weber, "On the Deep Structure of Information Systems," *Information Systems Journal* (5:3), 1995, pp. 203-223.

Second, research on these phenomena had fallen somewhat into disrepute. Third, the discipline of computer science had already staked substantive claims on these phenomena via extensive work that had been carried out on the topics of semantic modeling and database normalization.

As we reflected on these phenomena further, however, we reached two additional conclusions. First, we became convinced that "representation" was the *essence* of all information systems. The *raison d'être* for information systems was that they tracked states of and state changes in other systems. By observing the behavior of an information system, we obviate the need to observe the behavior of the system it represents. We thereby avoid having to incur the costs associated with observing the represented system. For example, with an order-entry information system, we track states of and state changes in customers, which means that we do not have to consult with each customer individually to determine the goods or services they wish to purchase. Moreover, in some cases an information system provides us with the only means we have available to *observe* the behavior of the represented system. For example, in a simulation, the represented system may not exist, except in our minds. The simulation is an information system that allows us to gain insights into the behavior of the represented system.

Second, even though a substantial amount of work had been done on building conceptual models and designing databases, some of it was atheoretical. In particular, while research on data normalization was grounded in solid theory, research on conceptual modeling was virtually devoid of theory. This situation seemed curious, given we had concluded that *representation* was the essence of an information system. Moreover, as we searched in other disciplines for theories that might account for the sorts of representational phenomena that were relevant in an information systems context, we were unable to find any that were compelling. Ultimately, we concluded that theories of ontology potentially provided us with the best base for building theories about information systems representational phenomena. Nonetheless, it was also clear that these theories had to be adapted and extended substantially if they were to provide us with powerful accounts of information systems representational phenomena.

Like Wand, I continue to believe that we will establish a theory of the core of the information systems discipline (and potentially a separate identity for the information systems discipline) if we can articulate powerful, general theories to account for the characteristics of representations that enable "faithful" tracking of other systems. Whereas natural information systems evolve to be faithful representations of the systems they track, artifactual information systems have to be *designed* to achieve this objective. Moreover, the sorts of representations built by, say, artists and engineers provide us with limited insights in terms of how we might accomplish this objective. For the most part, artists are not concerned with building representations that undertake one-to-one tracking of the "worlds" they are representing. Similarly, engineers are concerned with building artifacts that accomplish some type of work, but the artifacts that are their focus are not intended to *represent* another system (unless, of course, the engineers are also building information systems). For these reasons, how we design good or faithful representations of other systems has remained the focus of Wand's and my work.

I fully accept that other types of information systems-related phenomena might exist for which theories borrowed from or adapted from other disciplines provide an inadequate account. If so, such phenomena potentially provide the basis for theories of the core of the information systems discipline. For the moment, however, I am unable to identify such phenomena. Nonetheless, for two reasons I hope such phenomena exist. First, having a core that includes more than representational phenomena would likely make for a richer, more-interesting discipline. Second, in due course I may have to resile from my belief that representational phenomena form part of the core of the information systems discipline (e.g., a colleague may convince me that I hold mistaken views). If this outcome occurs, my consolation would be that we have other foundations on which to build an identity for our discipline.

Publishing With and Without a Theory of the Core

If we subscribe to the notion that we need to develop a theory of the core of the information systems discipline if our discipline is ever to develop its own clear identity, then the type of research we undertake ought to reflect this belief. If called to account by our colleagues, we ought to be able to explain why we believe our research is contributing to a theory of the core of our discipline. In short, we ought to practice what we preach; otherwise, with some justification, we might be accused of hypocrisy.

Moreover, if we are faithful to our belief, some important implications arise. We ought not to seek publications for the sake of increasing the length of our vitae. We ought to be forthright with our Ph.D. students about our belief. We should also advise them about their choice of topic and the conduct of their research in ways that are consistent with our belief. We ought to be especially circumspect about continuing to pursue a line of research if we conclude it is not contributing to a theory of the core of our discipline. We ought to accept that citations to our work will fall, at least in the short run. If we are senior scholars within the discipline, as I argued in my March 2003 editorial, we have a special responsibility to bring about change. Relative to our more-junior colleagues, we are better placed to withstand the setbacks that often accompany fundamental changes in the direction of a discipline.

If we do *not* subscribe to the notion that we need a theory of the core of the information systems discipline if our discipline is ever to develop its own clear identity, then we face a "boundaries" problem. We need criteria that we can use to judge what types of research fall within the boundaries of our discipline and what types of research fall outside the boundaries of our discipline. Subsequent to the publication of Orlikowski and Iacono's paper, I have noted that members of the *MIS Quarterly's* Editorial Board and reviewers for the *MIS Quarterly* increasingly are asking how papers contribute to knowledge within the information systems discipline as opposed to other disciplines. They also now appear to be more unwilling to deal with papers that engage in only superficial ways with information technology and information systems.

In this regard, Benbasat and Zmud's paper in this issue of the *MIS Quarterly* provides putative criteria we might use to evaluate whether research should reasonably be deemed to be information systems research. First, they provide a "nomological net" of constructs that they believe captures the major phenomena *immediately* associated with the information technology artifact. Second, they caution information systems researchers against "errors of exclusion"—undertaking research that includes "neither the IT artifact nor at least one of the elements associated with its immediate nomological net." Third, they caution information systems researchers against "errors of inclusion"—undertaking research that examines constructs "best left to scholars in other disciplines" because of their "significant causal distance" from the IT artifact. In the absence of a theory (or theories) of the core of the information systems discipline, I suspect we must resort to criteria like those Izak and Bob suggest to evaluate whether a piece of research constitutes information systems research. I anticipate, however, that Izak and Bob's criteria will be scrutinized and debated.

Disciplinary Identity, the Core, and Some Editorial Pointers to Publishing in the *MIS Quarterly*

My purpose in articulating my views in this editorial in a fairly forceful and forthright way is to provoke discussion and debate on enduring concerns that I have about our discipline. I accept fully that many colleagues believe strongly that the concerns I hold about the identity of and nature of the core of our discipline are spurious. In short, they see *no* problems that ought to concern us and believe instead that my concerns are badly misplaced. Moreover, some colleagues who share my concerns about our

discipline disagree strongly with my diagnosis of the problems and my views on appropriate ways to try to address them. Whatever our views, I believe we will benefit from open, constructive, and respectful debate. In this regard, as I have indicated above, I hope to be proved wrong in some of my views. It would make my life as a scholar easier!

Let me also underscore that my personal views will *not* be the standard by which manuscripts are deemed suitable or unsuitable for the *MIS Quarterly*. I believe authors, reviewers, and members of the Editorial Board who have dealt with me will vouch that I am willing to work with and support a wide range of topics and approaches when I have editorial responsibilities to discharge.

I fully understand, also, that we need work done outside the core of our discipline, however we might define the core. Information systems practitioners face a wide range of problems. The application of theories from other disciplines to better understand the nature of these problems clearly is an important contribution to our overall portfolio of knowledge about information systems- and information technology-related phenomena. Insofar as this work is the only sort of research done within our discipline, however, I believe it exacerbates rather than mitigates problems associated with our identity as a discipline. On the other hand, if it is seen as supportive of but ancillary to research on the core, I believe it will enhance rather than undermine our identity as a discipline.

At the *MIS Quarterly*, increasingly we seem to be facing the problem of receiving manuscripts that stretch the boundaries of our discipline to an extreme. At least *prima facie*, they contain little content that bears on information technology or information systems. Each Senior Editor has the prerogative to make a decision on whether a manuscript's topic is congruent with the mission of the *MIS Quarterly*. Nonetheless, I suspect that most, if not all, would find the sorts of criteria discussed in Izak and Bob's paper to be a reasonable basis for evaluating whether a paper falls within the boundaries of our discipline and thus potentially is a suitable publication in the *MIS Quarterly*. Of course, a Senior Editor will also use other criteria to evaluate whether a manuscript is congruent with the *MIS Quarterly's* mission—for example, whether the overall contribution to knowledge made by the research reported in the paper is sufficient. Nevertheless, for authors who intend to submit their papers to the *MIS Quarterly*, I believe it will be helpful if they reflect on whether they have made "errors of exclusion" or "errors of inclusion." If so, their papers might best be submitted elsewhere.

One More Time: Hot-Off-the Press Submissions to the *MIS Quarterly*

In my March 2002 and June 2002 editorials, I urged authors not to submit their papers to the *MIS Quarterly* without first obtaining feedback from colleagues. I recommended that authors present their papers at departmental or school workshops, for example, where they had to defend their research before colleagues. If their paper were to be distributed before the workshop, hopefully their colleagues would also take the time to read the paper and perhaps provide written comments on it.

The Senior Editors at the *MIS Quarterly* still receive too many hot-off-the-press submissions. I fully understand the desire to submit a paper to a journal once the first complete version has been prepared, especially if the time clock for a tenure or promotion decision is ticking rapidly. Unfortunately, the result is often a longer review process, costs imposed on other colleagues, and an unfavorable decision on a paper that might otherwise eventually have been published.

At the *MIS Quarterly*, the Senior Editors screen papers when they are received. The purpose is twofold: (1) to determine whether the research reported within the paper is congruent with our mission, and (2) to

evaluate whether the quality of the research reported and the quality of the manuscript itself are sufficient for the paper to have a reasonable chance of successfully traversing the review process. We do not wish to dispatch papers to reviewers that have little or no chance of eventual acceptance. In this regard, we are mindful of the fact that our reviewers perform an important service role. Their time is valuable; thus, we try to respect it. Understandably, some are quick to express displeasure when we send them poorly crafted papers for evaluation.

When papers do not quickly pass a Senior Editor's screening process, unfortunately the timeliness of the review process is undermined. If the Senior Editor is uncertain about the likely fate of the paper if it were to be sent out for review, she or he may consult with an Associate Editor. If a decision is taken to proceed no further with the paper, the Senior Editor (and perhaps an Associate Editor) then has to prepare a report explaining to the author why her or his paper has not been dispatched for review. Because Senior Editors and Associate Editors often handle many papers at one time, they may not be able to prepare a report for the author quickly. Moreover, authors of papers that *do* pass the screening process suffer time delays. Reviews of their papers are held up because Senior Editors and Associate Editors are devoting time to preparing reports for authors of papers that do *not* pass the screening process.

In short, we all benefit when we take the time to (1) understand the mission of the *MIS Quarterly*, and (2) refine our papers as best we can before we submit them for review. When contemplating a timeline for submission of a paper to a journal, we need to allow a period for scrutiny by colleagues prior to submission.

Reviewer of the Year for 2002

I am pleased to announce that the *MIS Quarterly's* Reviewer of the Year for 2002 is Mani Subramani of the University of Minnesota. Mani has been a frequent Reviewer for the *MIS Quarterly*, and Senior Editors and Associate Editors alike have regarded his reviews as outstanding. On behalf of the *MIS Quarterly*, I would like to extend to Mani our congratulations on his achievement and our thanks for the contributions he has made to our review processes.

Best-Paper Award for 2002

I am also pleased to announce that the *MIS Quarterly's* Best-Paper Award for 2002 goes to 'Jon (Sean) Jasperson, Traci A. Carte, Carol S. Saunders, Brian S. Butler, Henry J. P. Croes, and Weijun Zheng for their paper, "Review: Power and Information Technology Research: A Metatriangulation Review," which appeared in the December 2002 issue of the *MIS Quarterly*. Nominations for the best-paper award are made by the Associate Editors of the *MIS Quarterly*. A short-list is then considered by the Senior Editors of the *MIS Quarterly* and the winner chosen from this short-list. On behalf of the *MIS Quarterly*, I would like to extend to Jon, Traci, Carol, Brian, Henry, and Weijun our congratulations on their achieving this award and our thanks for their support of the *MIS Quarterly*.

Ron Weber
Editor-in-Chief
weber@business.uq.edu.au