EDITOR'S COMMENTS

The Problem of the Problem

Much of what we do as researchers can be characterized in terms of three activities: (1) describing some phenomena that we perceive in the world; (2) articulating a theory to account for the phenomena; and (3) testing how well the theory accounts for the phenomena. I realize and accept that not all researchers would agree that this characterization of their activities is "valid." Nonetheless, for many, if not most of us, I believe it rings true.

As a young researcher, I focused primarily on research method. I struggled mightily to acquire and master the arcane skills that made for good research design, high-quality data collection, and appropriate data analysis. As a reviewer for journal submissions, I wrought substantial satisfaction from exercising these skills to evaluate the work of my colleagues. Often, I regret to indicate, my methodological evaluations of a paper overshadowed all else. Methodology was concrete. It enabled me to evaluate relatively easily whether any flaws or weaknesses existed in a paper. Moreover, by using methodology as my primary basis for evaluating papers, I avoided having to justify to editors any concerns I had about other aspects of a paper—for instance, the extent of the contribution I believed the paper was making to the information systems discipline.

Subsequently, I came to understand that the ability to build good theory was an equally important but difficult skill to acquire. When I was a graduate student in the early days of the information systems discipline, little was said or written about theory building. Instead, methodological concerns were dominant. Regrettably, I believe, little has changed. While we now pay some attention to how one builds good theory, I suspect that most graduate programs remain impoverished in terms of developing theory-building skills among students. Apparently, we must acquire our theory-building skills by osmosis. Little wonder, therefore, that as a discipline we have a reputation for using and adapting theories developed in other disciplines (although this situation is hardly different from a number of other disciplines). Little wonder, also, that we see few high-quality standalone theory papers in our discipline, in spite of the significant insights that such papers can provide about information systems-related phenomena.

Somewhat late in my career as a researcher, I began to realize the importance of the first phase of research—describing those phenomena that we perceive in the world that we wish to explain or predict. As a graduate student, I had an inkling that this phase was important. For instance, like many graduate students, I had great difficulty finding a topic for my thesis. Unfortunately, I did not attend to the lesson I should have been learning via this experience about the importance of and difficulties associated with choosing a "good" research problem. Instead, I paid more attention to the pangs of inadequacy I felt as I saw some of my fellow students identify thesis topics with ease. Moreover, I was chastened when some experienced researchers with whom I spoke about my difficulties in choosing a thesis topic remonstrated that interesting research problems existed everywhere!

Today I believe that the choice of research problem—choosing the phenomena we wish to explain or predict—is the most important decision we make as a researcher. We can learn research method. Albeit with greater difficulty, we can also learn theory-building skills. With some tutoring and experience, we can also learn to carve out large numbers of problems that we might research. Unfortunately, teasing out deep, substantive research problems is another matter. It remains a dark art.

Why Is Problem Articulation the Most-Important Activity?

Albert Einstein is reputed to have said that great scholars do not *solve* problems. Instead, they *create* them. I will not presume to understand what Einstein meant. I can, however, give my own interpretation of what such a statement might mean.

As researchers, we understand that we ought to devote our scarce resources to solving the most-important problems within our discipline. But what are the most-important problems? Often we are driven by practical considerations. For instance, some new information technology artifact is created (such as enterprise resource planning systems). An immediate need arises to understand how the difficulties associated with developing and implementing the artifact can be overcome. Through our research, we might be able to provide answers to the immediate, practically important problems that confront users of the artifact.

As scholars, however, hopefully we are seeking answers to problems that have longevity. Ideally, we could look at the plethora of practical problems that confront us and characterize these problems in terms of a much smaller set of generic, prototypical problems. By solving these generic, prototypical problems, our expectation is that we could quickly garner important insights into the specific, somewhat transitory problems that arise with each new wave of information technology.

What are the deep, substantive, generic, prototypical problems that confront our discipline? From time to time, I have pondered this question with colleagues, some of whom have been senior, experienced researchers of high repute within our discipline. Regrettably, we have been unable to answer the question satisfactorily. Are there no deep, substantive, generic, prototypical problems in the information systems discipline? If we continue to believe that such problems exist and someone manages to articulate them clearly, this colleague will have made a major contribution to our discipline. She/he will have provided the critical foci for our research efforts. They will have manifested a characteristic of "greatness" as a researcher in the sense that I believe Einstein meant.

Aside from indicating where we should devote our research efforts, articulating the deep, substantive, generic, prototypical problems in a discipline has yet another benefit. We ascribe *identity* to disciplines based on the problems that their members seek to solve. To the extent these problems are not pursued elsewhere in other disciplines, the discipline's self-identity is likely to be strong. Similarly, to the extent these problems simply mirror problems studied in other disciplines, the discipline's self-identity is weakened.

Besides articulating the deep, substantive, generic, prototypical problems in a discipline, great researchers create problems in two other ways. First, they "see" problems that nobody else sees. They observe phenomena that escape the attention of others and recognize that these phenomena have deep significance. As a member of a discipline, often we are shackled in our ability to observe and make sense of the world. We see the world through the theoretical lenses or philosophical or methodological perspectives that are dominant in our discipline (and with which we feel comfortable or with which we have accumulated a research portfolio in which we are invested). To the extent our lens or perspective does not "pick up" certain phenomena, we will not see them. At the appropriate moment, great researchers have an ability to set aside hegemony and comfort—to eschew traditional lenses and perspectives and their own predilections and to adopt a fresh view of the world. Conversely, they have an ability to recognize "false" problems—to see some problems that command the attention of the discipline as being shallow and ephemeral.

Second, great researchers have an ability to reframe problems. They see a particular problem as an instance of another deeper problem, or they characterize a problem in a way that its deep structure becomes apparent. Somehow they turn the apparently trivial, mundane problem into a substantive problem—one that occupies researchers in the discipline for a long period. Often they bring to bear a new theoretical lens or perspective that is not primarily an incarnation of a theory or perspective that dominates another discipline.

Status of Problems in the Information Systems Discipline

In his editorial notes in the December 2002 issue of *Information Systems Research*, Chris Kemerer commented (p. iv): "As a community we need to put more focus on our choice of problems and our efforts to explain our choice of problems to others in the broad community, not just to those in some familiar niche." Chris also observed that his two predecessors as Editor-in-Chief of *Information Systems Research*, Izak Benbasat and John King, had indicated the need for us, as researchers in the information systems discipline, to think more carefully about the nature of the contribution to knowledge we were likely to make when we undertook some piece of research.

My own experience with the *MIS Quarterly* echoes Chris's, Izak's, and John's concerns. Many colleagues have commented to me that often we achieve high levels of rigor in the research we now publish in the information systems discipline. They feel the problems we address, however, are relatively uninteresting and unimportant (at least from their perspective). Recently I have also had some Reviewers and Associate Editors ask authors why their research ought to be published in an information systems journal. Members of the review team have argued that the primary contributions of the paper are to organizational science, or to management, or to economics, or to marketing, or to some other discipline, but they see no substantive information systems issue that the paper has addressed.

I have long held concerns about the nature of the topics we often study as information systems researchers. In 1983, for example, I presented a paper to the Doctoral Consortium of the International Conference on Information Systems where I expressed concerns about our over-reliance on (1) the so-called reference disciplines to provide the theoretical bases for our research, and (2) the "surface" issues associated with new technology as the bases used to motivate our research. The paper was subsequently published in the Spring 1987 issue of the American Accounting Association's *Journal of Information Systems* under the title, "Toward a Theory of Artifacts: A Paradigmatic Basis for Information Systems Research." I know and accept that many colleagues do not share the concerns I expressed in this paper. For some colleagues, however, I know they remain enduring concerns (as they do for me).

Unfortunately, the institutional environment in which we operate as scholars contains many incentives that steer us away from thinking deeply about the problems we research. For example, as young scholars facing a promotion-and-tenure time clock, we need to achieve publications quickly in the most-prestigious journals. We are more likely to achieve this goal if we follow well-established streams of research and use research models that lend themselves to relatively easy empirical testing. Similarly, our status as scholars and our compensation often depend on how frequently we publish. If we take time to think hard about what we are doing, our publication productivity is likely to be undermined. It takes courage to pause, to stop, and to reflect. It takes even more courage to change our existing patterns of behavior as researchers, especially if we have made large investments in acquiring knowledge and experience that no longer seem relevant to addressing the deep problems that emerge as we reflect on the nature of our discipline. In this regard, the senior scholars in our field have a special responsibility. They must take the lead in breaking the molds of conventional research and confronting directly the problem of the problem.

How Do We Find Deep, Substantive Problems?

As you have no doubt surmised already, I have no easy answer to this question. Many researchers on creativity now argue that creativity can be taught. Perhaps in our graduate programs we should spend some time teaching students ways to enhance their creativity. We might then see a cohort of researchers emerge who are comfortable with devoting more of their energies to choosing and characterizing the problems they will research.

If we simply begin to attend to the problem of the problem in a concerted way, however, I believe we will eventually see an improvement in the quality of the research work we undertake. As I indicated in my March 2002 editorial comments, I have asked my colleagues on the Editorial Board of the *MIS Quarterly* to pay more attention to the contributions to knowledge a paper is making. I have urged them not to make an acceptance recommendation based primarily on the author's use of a rigorous, high-quality research method or the paper's fit with a well-developed stream of research. The research problem addressed by the paper must also be substantive.

I believe, also, that we need to change some deeply ingrained mindsets in our discipline that are damaging to our identifying and articulating high-quality research problems. One such mindset is reflected in the way many of us characterize the research work we do within the information systems discipline. We see ourselves as qualitative researchers, or experimentalists, or phenomenologists, or survey researchers, or case-study researchers, or whatever. In short, our identity as researchers is a function of the extent to which we can flex our muscles using a particular research approach or method. It is not rooted in the sorts of fundamental problems we should be striving to address as scholars. I suspect this situation is a vestige of the old "methodology wars" that occurred long ago in our discipline. If I am right, it is time to forget and to move on.

Another mindset that I believe is problematic is the "have-theory-will-travel" mindset. Sometimes I have heard senior researchers urge graduate students to take a theory they have learned from a reference discipline, perhaps modify it in some way, and then apply it in an information systems context. Alternatively, I have heard students urged to take a theory that has been used widely in the information systems discipline, perhaps extend it in some way, and then apply it in another information systems context. I agree that these are useful strategies for identifying research topics, especially when one is trying to gain experience as a researcher; indeed, some would classify such research under Thomas Kuhn's heading of "normal science." To the extent we continue to use them as strategies for identifying research topics throughout our careers, however, we will avoid confronting the problem of the problem. The theories we use eventually will constrain our ability to see the world rather than enrich the ways in which we see the world.

At least among graduate students, however, the greatest concern I have relates to the "subservience" mindset that many seem to hold. Once, when I acted as a faculty member for a doctoral consortium, I asked the students in my group how they had selected the problem they were researching for their thesis. We had just spent some time talking about the problem of the problem. The colleague who was working with me as a faculty member noted that most, if not all, of the students were shifting uncomfortably in their seats. Moreover, their eyes were downcast; they would not look at us. He then proceeded to go around the group, asking each one to answer my question. Almost every student in the group indicated they had selected their thesis topic to try to fit in with the research interests of their supervisor or the faculty within the department where they were undertaking their Ph.D. They perceived that the pressure to conform, either overt or covert, was substantial. If this situation is symptomatic of the state of graduate education in our discipline, future generations of information systems researchers will be ill-prepared to address the problem of the problem.

In summary, I believe we need to devote more attention to our choice of research problems in the information systems discipline. In addition, I believe we need to debate openly our choices of research problems. In the same ways we critique theory, research designs, research methods, and data analysis approaches, we ought to critique the choice of research problems. Moreover, as with any critique of colleagues' work, we should be forthright but constructive. Unless we are open, forthright, and constructive, we run the risk that some group of researchers will appoint themselves arbiters of what constitute the important problems within our discipline, use their power to discriminate against other researchers, and act covertly to mask the basis for their decision making. We need to accept, also, that at present the criteria for evaluating the choice of research problem are much less clear than those we use to evaluate theory, research designs, research methods, and data analysis methods. Thus, there is more scope for genuine disagreement among researchers about the choices made. Hopefully, however, these differences might motivate some scholars in our discipline to attempt to articulate what these criteria might be.

Abolition of Executive Overviews and Increased Emphasis Now on Quality of Abstracts and Keyword Choice

For many years, the *MIS Quarterly* has published Executive Overviews of the articles contained in each of our issues. Warren McFarlan first introduced them when he was Editor-in-Chief of the *MIS Quarterly* as a means of increasing the accessibility of our articles to practitioner members of the Society for Information Management (SIM). At that time, members of SIM received the *MIS Quarterly* automatically as part of the membership services associated with their annual dues.

Over the last few years, a number of senior colleagues have questioned the wisdom of our continuing to publish Executive Overviews in the *MIS Quarterly*. For a start, they have pointed out that SIM members no longer receive the *MIS Quarterly* as part of their membership services. This outcome occurred because SIM members increasingly experienced difficulties in bridging the gap between the research focus of our authors and their needs as practitioners. Thus, the clientele for whom the Executive Overviews were first devised no longer existed.

More salient, however, was the concern among some senior colleagues that the existence of Executive Overviews in the *MIS Quarterly* might send a wrong signal to important academic stakeholders about the nature of the journal. For instance, they surmised that some deans and members of promotion and tenure committees might mistakenly doubt the academic status of the *MIS Quarterly* when it contained content that was oriented primarily to practitioner readers.

Recently, the Senior Editors and I decided to address the question of whether we should continue to publish Executive Overviews in the *MIS Quarterly*. We also sought the advice of Jack Rockart, Editor-in-Chief of *MIS Quarterly Executive*. Aside from taking advantage of Jack's substantial wisdom and experience on matters associated with the interface between academe and practice, *MIS Quarterly Executive* now plays an important role in our discipline as a means of communicating our research results to practitioner colleagues. Jack, in turn, consulted with the Senior Editors of *MIS Quarterly Executive*. The Senior Editors of the *MIS Quarterly* and I received excellent counsel from Jack and his colleagues. We gratefully acknowledge their assistance during our discernment process.

In the interests of brevity, I will not canvass all of the issues we discussed in trying to reach a decision about whether we should continue to publish Executive Overviews in the *MIS Quarterly*. Suffice to say, however, that at the outset many of us held equivocal views. As a result, we reached a decision neither quickly nor easily. For instance, my own view was that Executive Overviews were valuable. Indeed, often

I found them to more useful than the abstracts prepared by the authors of articles. In the end, however, I became convinced that we should no longer continue with them.

For me, the turning point came when I was examining the editorial practices used and articles published by other journals to obtain information to help me with my own decision-making about whether to continue publishing Executive Overviews. Rather than reference hard-copy versions of the journals, I simply accessed them via the online journal database facilities offered through my library. If I am a typical user of electronic databases wanting to gain access to journal articles, the exercise reminded me of something I ought to have realized from the start. Specifically, I do not want to have to access multiple files (e.g., the article file plus an executive overview file or the editor's comments file) to obtain a copy of and information about an article. This is especially the case if each access requires a separate payment (either by myself or my library). Moreover, some electronic databases (e.g., ABI-Informs) provide access to articles on the basis of a topic area (e.g., they have a pointer from a topical index to articles on enterprise resource planning systems). The likelihood of these indexes pointing to any kind of summary that is not part of the article itself is small (if not zero). I suspect that scholars are increasingly using these indexes to find articles on particular topics that interest them. Thus, the usefulness of article summaries that are not part of the article itself will decline (perhaps rapidly) over time.

In short, the idea of having some kind of summary of an article that is physically separate from the article itself might make sense when the primary means of accessing and reading articles is by obtaining a physical copy of the issue of the journal in which the article appears. When articles are accessed electronically, however, most likely they will be retrieved separately from other articles that appear in the issue of the journal in which the article is published. Indeed, with electronic access to articles, the particular issue of the journal in which articles appear has decreased importance. In this regard, some electronic journals no longer use issues as their publication unit. Instead, they publish an article immediately after it has been accepted for publication. Given that in the near future the primary means of accessing *MIS Quarterly* articles is likely be electronic (if this is not the case already), the Senior Editors and I concluded that we should no longer publish Executive Overviews. This decision has been implemented with this issue of the *MIS Quarterly*.

In light of our ceasing publication of Executive Overviews, the abstracts associated with the articles we publish now become more important. Whereas in the past, readers of the *MIS Quarterly* might have relied on the Executive Overviews to determine whether they should read an article, they now must rely only on the abstract to make this decision. Abstracts have become more important, however, for still another reason. In an electronic environment, abstracts often form the basis for indexing. Similarly, the choice of keywords for an article becomes more important because they too often form the basis for indexing.

For these reasons, the Senior Editors and I are committed to working with authors to ensure we have high-quality abstracts for all our articles (in all departments of the *MIS Quarterly)*. Abstracts ought to be concise, but we see no reason why authors should not use slightly longer abstracts if, as a result, they are better able to communicate the contributions of their article. Similarly, authors should strive to ensure they make astute choices in terms of the keywords they select for their article. The *MIS Quarterly* will accommodate slightly longer lists of keywords if this will result in electronic services better indexing the articles we publish. A key issue here is that the quality of abstracts and keyword choices is likely to impact significantly on the extent to which articles published in the *MIS Quarterly* are indexed properly by the electronic services. This in turn will affect how frequently our authors' articles will be accessed, read, and cited.

Thus, the Senior Editors and I believe it is imperative that authors work hard to ensure their abstracts are well written and their keywords are well chosen. Abstracts should not be an afterthought—something written hurriedly after the first draft of a paper has been completed. Rather, they should be crafted carefully to highlight the research and practical contributions of the paper and the special "twist" that the paper brings to our understanding of or approach to studying a particular topic. Similarly, keywords should not be selected cursorily. Instead, they should be chosen to ensure the author's paper is not overlooked when scholars search electronically for papers published on a particular topic.

Changes in the Editorial Board

The following colleagues have completed their terms as Associate Editors of the *MIS Quarterly*: Anitesh Barua (University of Texas, Austin), Deborah Compeau (University of Western Ontario), Dale Goodhue (University of Georgia), Terri Griffith (Santa Clara University), Suzanne Iacona (U.S. National Science Foundation), Barbara Marcolin (University of Calgary), Poppy McLeod (Case-Western Reserve University), Rajiv Sabherwal (University of Missouri, St. Louis), Chris Sauer (Templeton College, Oxford), Carol Saunders (University of Central Florida), Peter Seddon (University of Melbourne), Sandra Slaughter (Carnegie-Mellon University), Veda Storey (Georgia State University), and Ron Thompson (Wake Forrest University). On behalf of the *MIS Quarterly*, I thank them for the outstanding service that they have provided, and I wish them well in their future endeavours.

I am pleased to welcome Michelle Kaarst-Brown (Syracuse University), Sue Brown (Indiana University), Wendy Currie (Brunel University), Gert-Jan de Vreede (University of Nebraska at Omaha), Debabrata Dey (University of Washington), Varun Grover (Clemson University), Prabhudev Konana (University of Texas, Austin), Dorothy Leidner (Baylor University), Shaila Miranda (Oklahoma University), Guy Pare (HEC Montreal), Jeff Smith (Wake Forrest University), Christina Soh (Nanyang Technological University), Sandy Staples (Queen's University), and James Thong (Hong Kong University of Science and Technology) as new Associate Editors of the *MIS Quarterly*. Each of these colleagues has been invited to be Associate Editors because of the outstanding review work that they have undertaken for the *MIS Quarterly* in the past and their distinguished record of publications. I congratulate them on their appointment to the Editorial Board, and I look forward to working with them.

It is with much regret that I bid farewell to Ilze Zigurs (University of Nebraska, Omaha) as a Senior Editor of the *MIS Quarterly*. Ilze has completed her term as Senior Editor, and she has earned a well-deserved rest from major editorial responsibilities. On behalf of the *MIS Quarterly*, I thank Ilze for her major contributions to our activities. I will greatly miss her wise counsel, support, and friendly disposition.

It is my pleasure to welcome Deborah Compeau (University of Western Ontario) and Carol Saunders (University of Central Florida) as Senior Editors of the *MIS Quarterly*. Debbie and Carol have been outstanding Associate Editors and Reviewers for the *MIS Quarterly*. Their appointment as Senior Editors is thoroughly deserved. I congratulate them, wish them well in their new role, and look forward to working with them.

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