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Communications of the Association for Information Systems

IS RELEVANCE: ARE WE ASKING THE RIGHT QUESTIONS?

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ABSTRACT

I argue that IS research is not relevant because much of what is published is not garnered from experience in the business world. As such, in many cases our research questions are not relevant to the interests of those who do the work. Further, I believe that status quo thinking that is reinforced by the top journals in our field stifles innovation.

I. THE RELEVANCE DEBATE

Most of the recent IS relevance postings on IS World criticize the status quo in the IS field. Prof. Samir Chatterjee suspects that the system is failing to recognize the broad talents that many professors possess, but for which they are not rewarded (IS World, 2/13/2001). I agree that the top-tier research universities pay little attention to technical, managerial, and teaching prowess. More importantly, published research is considered valuable only if it appears in the top 2 or 3 ranked IS journals. Prof. Choton Basu agrees that the reward structure needs to be reevaluated (IS World, 2/14/2001). I think that we are oversimplifying the debate when we pit theoretical research against applied research. I believe that field research constantly forces me to rethink my theories and hypotheses on how the world works. In support of this argument, Yin (1994) stresses that case study research is always superior when asking 'how' and 'why' questions. How can we then develop theory unless we know how the world we study works and why it works the way it does?

In defense of the current paradigm in the IS field, I agree that we do need to be rigorous in our research. My concern is that rigor appears to mean measurement rigor rather than rigorous study of phenomena. I believe that our major concern should be the research question. Exactly what are we trying to explore, investigate, and/or discover with our research. I genuinely believe that most published research in top-tier journals are not read by executives because they ask the wrong questions. If the questions are perceived to be valuable and interesting to executives, I believe that the readership will greatly increase. When I embark on a case study, I sometimes spend years working with managers and IS personnel. My rigor is in the data collection and emerging idea side of research, but it is definitely no less rigorous than any study published in the top IS journals. Also keep in mind that establishing contacts and trust takes time. The time devoted to getting to know executives, their needs, motivations, and the way they approach problem solving is part of the rigor involved in case/field study research. It seems that this critical part of field research tends to be overlooked by top-tier journals, if it is considered at all.

I read recent discussions about establishing relevant PhD programs, inviting practitioners into the classroom, and starting new journals. While all of these activities can definitely help, they will not change the status quo. Professors that publish in *MIS Quarterly* and *ISR* will still be considered

the best researchers in IS and those that publish in so called "lesser" journals will probably not be considered at all.

The issue at hand is whether or not the current promotion system rewards innovation and state-of-the-art thinking. I believe that the current system within which we operate does not promote innovation. The real question to the IS field is what is our purpose? What is our goal as a discipline? A debate is healthy for the future of our field.

II. LEADING EDITORIALS

Professor Detmar Straub perceives that the relevance-versus-rigor dichotomy is profoundly myopic (IS World, 2/15/01). I agree in principle. However, I believe that we construct this dichotomy artificially. In reality, a proper IS research project must be both relevant and rigorous. How can we have one without the other? If a project is rigorous but not based in reality, what real value can it have to anyone (including academia)? I argue that the best IS research over the past twenty-five years has come from practice (mainly innovations developed in research groups sponsored by organization). A recent example is business process reengineering (BPR). One origin of BPR research came from consulting work that Hammer and Champy did with several companies. These prophets of BPR didn't invent reengineering, but devised a terminology to fit their consulting work with GM, Goodyear, IBM, HP, Kodak, and many others (Hammer and Champy, 1993). In the same year, Thomas Davenport published a book on process innovation. In contrast to Hammer and Champy, Davenport (1993) focused on the enabling potential of information technology to BPR success. Although both Hammer and Davenport spent time in academia, their reengineering knowledge was developed and refined through field studies and consulting with organizations. Their field research was extremely rigorous, but also practical.

Alan Dennis argues that there is a huge danger in wanting the discipline as a whole to be relevant (IS World, 2/15/01). He believes that this is the job of consultants who get paid well to make academic research relevant. Professor Dennis makes the point that windows and the mouse interface was not relevant in 1967, but is so today. The solution offered by Professor Dennis is in the creation of a new journal, namely, *MISQ Executive*. I humbly disagree. First, I can see no danger in at least attempting to make IS research relevant. Even research that may not be relevant today should at least conceptually pose its important to the IS world at large. Second, I disagree strongly with the proposition that consultants are charged with making our work relevant. As a consultant, I rely on case studies, experience, white papers, contacts, technology "press" textbooks, practitioner conferences, and practitioner textbooks to facilitate and supplement my work. Third, windows and the mouse concepts were originally conceived by Doug Engelbart while at Stanford Research Institute, not from published research. "One day I was passing Doug's [Engelbart] office in the late 1960's and he called me in to show me the mouse and the windows concepts. It is true that Xerox commercialized them, first through the Alta (used internally) and then through the Xerox Star, and promptly blew the marketing" (Gray, 2001). Steven Jobs appropriated the mouse and windows concepts after a visit to Xerox Parc and Bill Gates stole it from Apple. Finally, the establishment of a new journal is commendable, but late. *JITCA* has been in existence for over three years. *JITCA* came into existence to address the IS relevance gap. It publishes two research articles and one teaching case every quarter. The research articles are very rigorous and the editorial board is excellent.

Allen Lee, editor of MISQ, states that, "research unfolds in this overall structure … [the structure] is sanctioned by established researchers" [Lee, 2000, p. v]. He goes on to say that most IS research adopts the natural science model. IS research also involves "not only logic and procedures, but also the norms, values, and culture of the particular scientific community" Finally, he states " … I have acted with some colleagues to establish a new journal, whose review system would explicitly recognize and reward a practitioner-oriented direction in academic research". First, I believe that the current structure is the problem because "established researchers" sanction it. How can these people objectively review the structure that made them what they are? How can they make decisions that may ultimately diminish the value of their own work? Second, I

object to using the scientific model to study the IS world. I realize that this decision was made over twenty years ago, but this model may not be pertinent to IS research. Professor Lee doesn't endorse the scientific model as an absolute he merely reports his perceptions. He states that improvement research " … is no less valuable than research following the natural science model". He also advocates the establishment of a new journal. Again, I don't think that this step is drastic enough to make any real changes to the field. Also, *JITCA* was established over three years ago for this very purpose. "A social intervention into our research structure could be required to give [improvement research] the full recognition that it deserves" [Lee, 2000, p. vi]. What kind of social intervention is Professor Lee referring to? Who will instigate this social intervention?

III. WHAT CAN BE DONE?

Steve Dickinson (BPR Director, Caterpillar) once asked me why I was researching with Caterpillar. I informed Steve that I wanted to learn from them. Steve replied, "Dave, that is NOT your job. Your job is to LEAD us!" How can we lead if we don't know anything? Our structure is inflexible, rewards status quo thinking, and is very *exclusive*. Of course, we cannot afford an "If it ain't broke, don't fix it" mentality. Our system (the MIS discipline) is broken. We are not leading industry in any way that I can see. Practice doesn't respect us because we really don't respect them. If we shut ourselves away from practice, then we are telling them that we don't respect their innovativeness, ideas, practices, and logic. It is very difficult (if not impossible) for people within a system to 'think out-of-the-box' without such an intervention (Paper, 1998). I agree with Murray Jennex when he says, " … business is a part of social structure … business is a product of our social structures and a manifestation of society … "(IS World, 2/16/2001). Similar to leading organizations, we need a cogent and "doable" vision and direction. So, what can be done?

First - we need to rethink our stance on A, B, and C journals. The current structure will never reward state-of-the-art thinking.

Second - we need to redesign our basic structure. Tsipi Heart makes an interesting comment. "Why don't we embrace the Medicine discipline model, where academia and practice MUST be integrated … " (IS World, 2/15/2001).

Third - we need to define ourselves differently than other disciplines. We are a new discipline that *can* be different. We can develop our own principles. We need to our methodologies to be grounded in valid methods and procedures as called for by W. Edward Deming (1993). According to Deming (1993), a system is made up of four principles:

- appreciation of a system,
- appreciation of variation,
- theory of knowledge, and
- an understanding of psychology.

A system is a collection of processes that collectively makes the customer happy. However, we must understand variation in the system to improve it. As we solve business problems, we develop working theory based on current knowledge of how the system works and cyclically test the theory. Finally, we have to understand how people think, as people are the ones who do the work. Mastery of these four principles is what Deming calls 'profound knowledge'. According to Deming, we cannot make good decisions before we have profound knowledge.

Fourth - we need to be innovative. What are the future technologies? What is our role in researching and consulting in these areas to make a difference? In this arena, case studies can be an excellent vehicle to explore state-of-the-art thinking because researchers can gain a better understanding of 'how' a phenomena works and 'why' it works the way it does. The state of the IS field is not fertile for case study research. " … quantitative methodologies and especially survey research [dominate] MIS research … more qualitative research and case studies need to be conducted … " (Palvia, 1999).

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David Paper is associate professor in the Business Information Systems and Education Department at Utah State University. His publications appear in *Long Range Planning, Knowledge and Process Management, JITCA, Creativity and Innovation,* and several others. He also spent time in industry working for Texas Instruments, DLS, Inc., the Phoenix Small Business Administration, and Wides Village Real Estate Company. He is currently consulting with the Utah Department of Transportation and researching with Moore BCS, Fannie Mae, and Bank of America.

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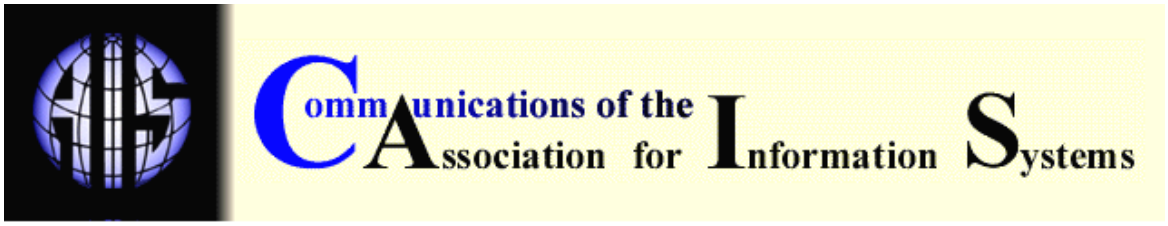

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