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ABSTRACT

To be truly relevant, research must be oriented toward the future. After the research is completed, it must ultimately provide some value to society, whether near term or long term. Information systems researchers, concerned as we are with the uses of technology, operate under a severe handicap because we do not have opportunities to study new technologies while they are under development. Our challenge is to find ways to work together on the most relevant questions and to perform research at earlier stages of the technology life cycle.

I. INTRODUCTION

Is relevance in the eye of the beholder? Are we all talking about the same thing when we speak of relevance? In looking through the fascinating, spontaneous dialogue that recently erupted on ISWORLD, it is not clear that we are. Some participants seem to be saying that the only studies that can be called "relevant" are those that are currently useful. Therefore, the argument goes, we should not be concerned about relevance but only about future applicability, or "basic research".

That view of relevance may conform to the dictionary definition ("bearing on the matter at hand; pertinent"), but I doubt that most of us who argue that a higher degree of relevance is needed are thinking only of current applications. Certainly CIO's and other practitioners have to make decisions about the future, and would benefit from studies that might lead to better decisions. Benbasat and Zmud (1998) maintain that information systems scholars should be performing research that is relevant to practitioners and, in their view, we are "chasing after practice rather than leading practice" (p. 6). Several other prominent researchers not only agree with their criticisms but argue that the recommendations should be even stronger (Davenport and Markus, 1998; Lyytinen, 1998; Lee, 1998).

II. "BASIC" RESEARCH ORIENTED TO THE PAST

I believe that basic research is highly relevant if there is some likelihood that it might lead to solutions of future problems, and that we should engage in it. Unfortunately, we are NOT engaging in it to any significant extent. Several dialogue participants talked about the need for basic research, and cited such developments as Arpanet, TCP/IP, the Mosaic browser, the World Wide Web, and all of the important products that came out of Stanford Research Institute and Xerox Parc from Douglas Engelbart and others. Information systems cannot take credit for such inventions; these researchers were all computer scientists, physicists, or mathematicians. The relational model, based on mathematical principles, was developed by E. F. Codd, and the Entity Relationship model by Peter Chen. Neither were MIS researchers.

In today's world, the emerging technologies are mostly under the control of computer scientists and software engineers. Information systems researchers do not have access to them during design or development; we don't see them or even know much about them until public announcement. This situation is rather like having both hands tied behind our backs; how can we possibly lead practice if we cannot study these technologies at an earlier stage of development? In a recent Business Week article (Wildstrom, 2001) about a forthcoming product called Bluetooth, the author says that this new technology, "just hitting the market, is supposed to be cheap and simple enough to let just about anything connect wirelessly to anything else. Your laptop or handheld computer will wirelessly connect to the Internet." In a radio interview, Michael Dertouzos of MIT was discussing computers of the future, and the research being performed in his laboratory on such technologies as language translation and virtual reality. The products of the future will emerge from laboratories such as this one, and we are not participating in design or development. We should be. We should be helping to determine how such products will be used, how the user interfaces should be designed, how the new technology should be introduced in an organization, and what the benefits and costs are likely to be.

Instead, we wait until the product is announced and on the market. After Bluetooth comes out, we will start using it along with everyone else, and perhaps make authoritative comments in the news media about how the interface could have been more intelligently designed. We will design surveys and address them to practitioners, asking them to evaluate the new technology and explain how they are using it. It will be a "hot" topic, so many of us will be doing this work at the same time, though of course we will not coordinate with one another. We will collect the survey data, analyze it with great rigor, and write sophisticated, erudite articles larded with obscure references, masses of statistics, and detailed discussions of every analytical step. At the end of each article we will write a few paragraphs to explain what it all means. The articles will be published in various journals, including some of our best, after the data is four years old. Practitioners will not read them, because this technology will already be aging, and they will have to make decisions about all the *new* technologies that will be coming out in the next few years. Besides, the data came from them in the first place. How can we lead practice if we are asking them questions instead of providing answers?

About five years ago I attended a conference in which one of the sessions was focused on user interfaces. I was the discussant for three papers, all of which were based on studies of e-mail systems. All of the data had been collected at least three years before, from surveys of practitioners. All three papers were well written, their analyses of the data were thorough, and their conclusions were insightful. All three could have been useful had they been published several years earlier, but at the time of this conference *all* of the e-mail systems they were writing about were already obsolete; virtually no one was using them. Their well-reasoned recommendations had already been adopted. It seems to me that, if we are to play a more useful role in society, if we are to have any influence on how new technologies are designed, we will have to get into the laboratories and work with those who are creating them. Information systems include people, but they also include the technologies people are using. Physicists, chemists, and biologists work together. Artificial intelligence researchers come from a multitude of disciplines. Why do we insist on isolating ourselves, not only from practitioners, but also from the other business disciplines, computer science, psychology, and others with whom we should be working?

III. RESEARCHER ISOLATION

We are also, to a great extent, isolated from one another. We say we are a new discipline, building theories piece by piece, each researcher following their interests, doing basic research. Someday, somehow, someone will pull the pieces together and build a cohesive theory. But how much longer can we claim to be new? Nuclear physics was a new discipline in the 1930's; look what they accomplished in ten years. Computer science and artificial intelligence are older than MIS, but not by much. Building a meaningful structure the way we are attempting to do it is like 2000 carpenters building a house without an overall plan and without communicating with one

another. The walls won't fit together and the roof will surely leak. In the scientific disciplines, researchers work on tiny pieces of the puzzle, but with overriding goals that lead them in the same direction: finding a cure for cancer or Aids or malaria; interpreting the genetic code; breaking down the atom. They collaborate through elaborate networks and read each other's publications. According to Cech (2001), papers in biology "now often give long lists of authors, evidence that scientists working on multidimensional problems are teaming up. Those collaborations have been greatly facilitated by the Internet" (p. B24).

I am overgeneralizing, perhaps outrageously, to make a point. Certainly we have our fair share of outstanding scholars, gifted writers, and illuminating research articles, especially when compared to some of our business school counterparts (notably "Management"). But, as noted above, several of our most productive and influential scholars are calling for closer ties to industry, attention to research questions that are vitally important to practitioners, and simpler, more straightforward writing styles. We have been making similar criticisms to ourselves for years, over and over, yet there is little or no change in the way we do things.

IV. BEING OUR OWN CHANGE AGENTS

Many MIS textbooks describe the term "change agent". Systems analysts are often called change agents, and we frequently call ourselves that, because that is what information systems is. It changes things. Every computer program, every new system, every redesigned system represents a change from the previous environment. Users will have to change the way they do things to accommodate the new system, and we can help them make that adaptation. But we don't know how to change ourselves, and there is great resistance to change. Much of the reason for that is the extremely healthy job market, with employers snatching up our students as rapidly as we can turn them out, and classrooms bursting at the seams. High employer demand leads to a shortage of qualified academics, so our PhD graduates are enjoying a great sellers' market. It would be an illusion, in my opinion, to conclude that this high demand is based on our student's knowledge gained from information systems research. Most students are getting jobs as programmers, systems analysts, network specialists, help desk technicians, or database developers. They need marketable skills, and they need theories but most of these theories, such as normalization or structured programming. derive from disciplines other than information systems, The overwhelming majority of our students are NOT doctoral candidates; the body of knowledge needed for successful practitioner careers may be quite different from that needed for a faculty member.

V. RESEARCH QUESTIONS

If we are to make fundamental changes in our approach to research, we should start with a set of basic research questions. Not everyone will be able to agree on what the most important questions are, but if it is a broad question and a substantial number of researchers can agree on its significance it would help to guide us toward an eventual solution (and toward other questions further down the road). Studies focused on such questions should yield findings that lead to much more cohesive and integrated theories than those currently being produced. Some topics that might lead to useful research findings are:

E-COMMERCE.

This topic is of course very hot, but much of the research focuses on how e-commerce is practiced today. It would be much more useful to ask: what hardware and software is needed to make it better? What emerging technologies might enhance user friendliness or improve security? For products currently being developed, what is the best user interface? How should the product be introduced to ensure a high degree of acceptance?

OUR STUDENTS

What are the characteristics of a successful analyst? Of a programmer or system developer? What are their most important skills? What theories and facts do they need to know? How can

new employees be more intelligently guided into the careers for which they are best suited? Can we make student internships more valuable and meaningful? Can we establish distance learning collaborations with industry to enable their employees to acquire new skills?

VIRTUAL REALITY

Within the next five years, videoconferencing environments will be so real that it will seem we are in the same room with others who may be thousands of miles away. Can we simulate these environments and test the alternatives before they emerge? What will this mean for group decision support systems? For distance education? Can we start studying these questions now, or must we wait until the products are available to the public?

MIS EDUCATION.

In some ways, we academics ARE practitioners. We practice education; that is our real expertise (and our primary responsibility). Carefully planned and rigorously analyzed educational research could have great value for society. After all, if our objective is to determine how technologies can best be used, we have an ideal laboratory.

VI. CONCLUSIONS

Our discipline includes many talented researchers. Information systems research is good, but it could be better; therefore it SHOULD be better. If we don't constantly strive to improve, we are cheating our students, their employers, and ultimately ourselves. We are operating under severe constraints: lack of access to emerging technologies, weak ties to the organizational world, an academic environment that disdains relevance, and technological change occurring so rapidly as to be virtually impossible to keep up with. This has focused much of our research on the past rather than the future. Most of these limitations could be overcome if we have the will to do so. It will require strong leadership to pull us together, get us moving in the same direction with a high degree of collaboration, and encourage us to use the technologies ourselves in more imaginative ways.

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