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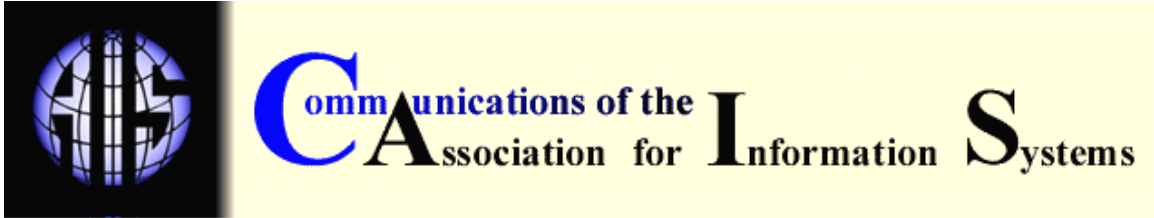
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RIGOR VS. RELEVANCE: A PRACTITIONER'S EYE VIEW OF AN EXPLOSION OF IS OPINIONS

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ABSTRACT

The Information Systems field has long been concerned with the issue of research rigor vs. research relevance. A recent IS World electronic discussion group opened up the subject once again; what follows is a collection of pithy quotes from that discussion, selected from an IS practitioner point of view.

I. INTRODUCTION

A classic dilemma in any academic field with practical implications is the issue of rigor vs. relevance - that is, is it more important to do impeccably sound research (rigor), or research that has the potential of being useful to practice (relevance)?

To the naive, that may seem like a tempest in a teapot. Isn't it possible, after all, to be both rigorous and relevant? The answer to that question, perhaps surprisingly to those not in the research field, is a resounding "no." Rigorous experimental research, for example, demands a highly controlled, limited-scope environment. But for research to be useful to the world of practice, it should be conducted in an environment as close to that real world as possible. And the real world is hardly highly controlled and of limited scope.

Perhaps nowhere is the rigor vs. relevance dilemma more difficult than in the field of Information Systems (IS). IS is, after all, the computing academic field devoted to the application of computing solutions to business problems, a field with about as much potential relevance as one could imagine.

The issue has troubled the IS field almost since its inception, several decades ago. Periodically, discussions arise in IS circles focused on facets of the dilemma. The issue assumes several guises: What, precisely, is the scope of the Information Systems field? What are its primary foci? What is its relationship with Computer Science, and what should it be? What is its relationship with other reference disciplines? Does IS research lead practice, or trail it? If it is trailing, is that a proper role for research? These are healthy questions, and yet the failure to resolve them over the years signals some confusion in the field.

What brings this issue to the fore now is the recent explosion of email discussion over the relative roles of rigor and relevance in IS research. The discussion was lively, and in some cases barbed. This article is a summary, from a practitioner point of view, of that discussion.

II. A SUMMARY OF THE DISCUSSION

To bring some order to what was essentially a collection of very personal beliefs and opinions, I abstracted several pithy quotations from the discussion, and (more importantly, at this point) a collection of topics that cover the ground of those quotations. Those topics, and some of the quotations, follow. (Note that I do not attribute the comments below to the individuals who expressed them. In this article, I am more interested in the viewpoints, than in who presented those viewpoints).

WHY IS IS ACADEMIC RESEARCH THE WAY IT IS?

We do research primarily to survive in academia. The attainment of tenure at most schools requires some level of research productivity ... It is a game played by academicians who wish to prosper...

A few (fortunately not many) of my instructors had no relevant work experience, and little understanding of how things work in the real world.

The real yardstick for IS research should be "Have we learned anything about information technology since our field was created? I think the answer is yes."

I think we should broadly classify research contributions in the IS/IT areas as follows:

1. Visionary/thought-provoking
2. Analytical models (e.g., database, supply chain management)
3. Empirical studies
4. Systems and technology innovations

The strengths of academic as opposed to industry research:

1. Issues contrary to commercial interests
2. Unsolved problems
3. Issues economically unattractive to industry
4. Issues where management aspects are more important than technical ones
5. Research on teaching IS

In 1967, Doug Engelbart produced some research at Stanford Research Institute that practitioners thought was not relevant. He had a hard time getting anyone to listen until eventually, five years later, Xerox became interested. The research was the windows and mouse interface. It was not relevant in 1967; it is very relevant today.

The issue of relevance to business practice is ... culture dependent. Such an issue would not have been given this weight if the cultural environment were not the western "free enterprise" (capitalistic) system.

Why should the relevance of our research to current business practice be used as the metric for judging IS research?

The views of anyone speaking for the interests of the business community should not be a basis for determining the relevance of the work. What's good for General Motors is not necessary good for the country.

WHY IS PRACTICE THE WAY IT IS?

I never read a journal when I worked in industry, and I honestly don't think I know anyone who did.

There are even two sets of journals, that attend to the needs of each group - Information Systems Research, MIS Quarterly, etc., for basic research, and Datamation, CIO, etc. for applied research.

I don't know why my staff avoids academic conferences. There are a number of PhDs on the team, and I do pass along to them all the interesting conference calls.

Much of what I know about practice gets obsolete very rapidly.

The biggest thing the work experience can provide is an appreciation of the need for accuracy, testing, and quality.

THE GAP BETWEEN ACADEME/RESEARCH/THEORY, AND PRACTICE

There is a chasm between academia and industry. Part of the problem is caused by a lack of understanding of the nature and purpose of IS research.

There are probably no academic findings of any importance in IT and few, if any, from business schools in general. The evidence is few, if any, business people bother to waste their time with academic journals.

We do not have any evidence of any potential impact of IS research on practice.

Research is often not timely or relevant to the applied world. Conferences are behind by a couple of years, and journals have review times of 1-3 years.

We are definitely behind practice!!! The MIS academic discipline is broken. We are not leading industry in any way that I can see. Practice doesn't respect us because we don't respect them.

In other fields, theories are proposed, then they are tested over and over again, as everyone tries to tear them down. What emerges is a theory that works in the real world. We don't do that, to my knowledge.

Using the expectations of business people to evaluate academic research will likely be as frustrating as using a hammer to plant a tree, or a shovel to drive a nail.

Academics and practitioners have a different worldview. Academics favor the creation of general concepts and ideas, and the refinement of theories and concepts.

There is no reason to make an "either-or" distinction in rigor/relevance, theory/practice; we have the option of "both-and."

Much of IS research is based on empirical evidence from practice. In many instances of IT applications, practice leads theory. But there are other examples.

Many in academia are unable to relate to practitioners who are concerned with the specific application of theory to a specific problem.

The academic world does not appreciate "old" people who try to return after 10 or so years, and literally makes it quite impossible to survive and integrate.

There's an old saying - theory without practice is sterile, practice without theory is blind.

Consultants get paid very well to make academic research relevant to today's practitioners.

TYPES OF ACADEMIC RESEARCH

There are two fundamental types of research: basic and applied. Basic feeds back into our books/articles/curricula, while applied extends basic research to solving problems of immediate interest.

TOP IS RESEARCH FINDINGS AND TOPICS

Decision support systems. Strategic alignment. IS/user partnerships. The importance of social context. Behavioral considerations. The productivity paradox. IS/IT as a strategic resource. Measures of IS effectiveness. Business process reengineering as a form of organizational change. Information systems architectures. Group systems.

RESEARCH/TEXTBOOKS VS. TEACHING

My time for reading is severely limited, so I have to select the articles carefully. Frankly, I do not find many that enhance my teaching. Rarely do I find an IS article that is as useful as the books I consult.

Folks don't hang your articles on their walls, but they do put your books on their shelves. Yet it seems that provosts and deans don't value books. I just don't understand this.

COMPUTER SCIENCE VS. IS

Basic research is absolutely essential, but such developments come out of Computer Science, not IS. When I want to learn more about emerging technologies, I consult Communications of the ACM or other computer science publications, but rarely our own.

The incumbent territoriality of Computer Science and Engineering has tended to insure that IS never got technical enough to threaten their turf.

I never hear from my Computer Science colleagues about issues like relevance or impact. Maybe it has something to do with creation. When you create (as computer scientists tend to do), you get an identity. The alternative is a kind of "parasitic" discipline, which merely surveys, analyzes, and proposes some bizarre hypothesis about how practitioners work.

My (practitioner) teammates seem less interested in IEEE and ACM meetings than ever before.

WHAT SHOULD BE DONE?

Maybe junior faculty should have to jump through tenure hoops, but it would be nice if senior faculty were tasked with making a reputation for themselves and their institutions out beyond the ivory towers.

Remove the journal rankings. Make all journal hits worth the same, and many of us would jump at the chance to do research that is publishable in an applied publication.

Rethink our A, B, and C journals. The current structure will never reward state-of-the-art thinking.

Reward academics who have critical skills.

Redesign our basic structure to move away from "who you know" and "what school you went to."

We are not a hard science; we do not have to embrace hard science principles to feel more comfortable with our own discipline. We can find our own way.

Why don't we embrace the Medical discipline model, where academia and practice must be integrated. Medical schools are located nearside and inside hospitals, and most of the researchers are also practitioners, or at least active consultants. We should try to persuade the giant companies, such as IBM, HP, Oracle, Microsoft, etc., to act as "hospitals" for us.

We are starting the new journal MISQ Executive, which will attempt to publish relevant articles.

I went to a conference recently where the keynoters were all academics, and the paper presenters were mostly practitioners. The thing that intrigued me was the questions practitioners had for the academics: Are we doing the right thing? How do we measure success? Are we having an impact?

Isn't it time to start thinking about a strategic alliance with Computer Science?

Does it not behoove us to have required coursework for Ph.D. candidates and graduate students that includes 3-6 credit hours of "applied work"? We ask our undergraduate students to do this kind of work, but many graduate students escape it completely.

Doctoral candidates need some practical work experience. All of us need periodic updating (to the state of the practice, not just the state of the art).

Is there a place for a new kind of doctoral program - call it a "practitioner scholar" program - that brings highly skilled, mid-career folk from industry and educates them at a doctoral level? If our IT research and teaching efforts are to be "relevant," wouldn't IT programs benefit from having a complement of such "converts"? Wouldn't industry benefit from educating their brightest and best at a doctoral level so that they can bring the latest "relevant" academic research to the work world?

Exchange programs with industry would benefit all of us, including tenured, experienced professors, and practitioners getting classroom experience.

One way to increase relevance is to have larger groups of researchers throughout the world working on the same experiment, "repeating" it. I think many people working on similar interventions can lead to greater understanding and relevance.

ABOUT THE AUTHOR

Robert L. Glass is president of Computing Trends, publishers of *The Software Practitioner*. He has been active in the field of computing and software for over 45 years, largely in industry (1954-1982 and 1988-present), but also as an academic (1982-1988). He is the author of over 20 books and 70 papers on computing subjects, editor of Elsevier's Journal of Systems and Software, and a columnist for several periodicals including Communications of the ACM (the "Practical Programmer" column) and IEEE Software ("The Loyal Opposition").

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