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IS RESEARCH RELEVANCE REVISITED: SUBTLE ACCOMPLISHMENT, UNFULFILLED PROMISE, OR SERIAL HYPOCRISY?

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ABSTRACT

The topic of "IS research relevance" is receiving increasing attention from the IS research community. In this article, based on a panel discussion at the 2001 International Conference on Information Systems, three arguments are explored in connection with IS research relevance. The first argument, called "subtle accomplishment", suggests that IS research is relevant, but in a subtle way. The second argument, called "unfulfilled promise", maintains that the promise of conducting relevant research is still unfulfilled. The third argument, called "serial hypocrisy", argues that, while "relevance to practice" is frequently proclaimed as a virtue in public, in reality practical relevance does not matter in IS research.

KEYWORDS: research methods, research relevance, rigor, external validity, industry relevance, action research

I. INTRODUCTION

"IS research relevance" is receiving increasing attention from the IS research community. While debate on this topic goes back to the inception of IS as a field, interest reached a new plateau in the last five years, with several articles published in leading IS journals addressing issues related to IS research relevance [Alter, 2001; Davenport and Markus, 1999; Dennis, 2001; Fitzgerald and Howcroft, 1998; Gray, 2001; Kock and Lau, 2001; Robey and Markus, 1997; Truex, 2001; Weinberg, 2001; Westfall, 1999; 2001]. Among the issues addressed are the possible dichotomy between IS research relevance and rigor, the contribution of IS research to practitioners, and the pros and cons of using research approaches that bridge the gap between researchers and practitioners such as action research. In addition, many discussions on the ISWorld email list address IS research relevance issues.

Publications and postings on IS research relevance suggest much diversity in the opinions held by senior IS researchers. Some seem to think that current IS research is not relevant to practitioners, and that this lack of relevance will soon lead to a negative impact on the entire field. Others disagree, pointing out that what is often referred to as relevant research is simply IS consulting conducted with little research rigor. These divergent views, while providing fuel for much constructive debate and the development of new research paradigms, result in at least one highly undesirable result: they create confusion in the minds of those who look up to senior IS researchers for guidance on how to begin their IS research careers and on what research paths to follow. Investing time and effort into a research path that is not likely to draw approval from the research community may have consequences that may range from mildly adverse (such as reduced social motivation stemming from lack of peer recognition and support) to devastating (such as tenure denial or termination of an academic appointment).

A major item is missing in the midst of the heated debate about IS research relevance: a definition of what IS research relevance means. Even though relevance is commonly equated with direct IS applicability of research results, often the theoretical foundations on which such research builds are seen as almost irrelevant when published. This perception is true for many other fields as well. An example from mathematics, where similar debates on research relevance rage, is George Boole's development of modern symbolic logic, now known as Boolean algebra. In the mid-1800s, when it was developed, it hardly qualified as a contribution to the practical needs of anyone. Nevertheless, today it provides the foundation on which virtually all digital circuits are designed and without which computers would not exist (nor indeed would the field of IS). The lesson here is that contemporaries, be they practitioners or researchers, may not be in a good position to judge the relevance of research that may find great practical applications in the future. Another example closer to home for this is the discovery of relational databases that was at first ridiculed as a theoretical toy.

II. GOAL AND FOCAL ARGUMENTS OF THE PANEL

This article is based on a panel discussion at ICIS 2001. The panel's goal was to channel the current academic debate on IS research relevance by providing a framework for discussion that is centered on a broad definition of IS research relevance. The definition takes into consideration two main facets of IS research relevance:

- audience and
- scope.

AUDIENCE

The audience for IS research includes industry practitioners and IS academics who, in turn, will produce more IS research. Even if a group of IS researchers produces research that is not of immediate use to industry practitioners, a second group of IS researchers can build on the publications of the first group and produce practitioner-relevant results. It is thus reasonable to argue that the research produced by the first group did possess indirect industry relevance in analogy to George Boole's research on symbolic logic.

SCOPE.

External validity, or generalization of findings beyond the scope of a research study, is a desirable characteristic of IS research. Yet, lack of generalizability does not indicate practical irrelevance, because research findings that are highly contingent on specific characteristics of the environment being studied are often highly relevant at a local level. A case in point is action research. The scope of relevance of IS action research findings to practitioners may vary significantly. For example, the outcomes of an action research study may be relevant to a single company, if the problems addressed through the research are specific to that company. The outcomes may be relevant to a whole industry, if the problems are faced by all (or most) companies in the industry; to a whole sector of the economy, if the problems are faced by all (or most) companies in the sector in question, and so on. But, broad or narrow, the relevance will always be there, but generalizabilty is not.

Based on the audience-scope framework, three main arguments (or points of view) regarding IS research relevance were developed to guide the panel discussion.

- IS research is actually relevant yet in a subtle way.
- The promise of conducting relevant research, which helped set the stage for the emergence of IS as an academic discipline in its own right, is still unfulfilled but in the path of being fulfilled.
- IS research relevance claims are nothing but hypocritical attempts by academics to maintain the status quo.

These arguments are presented briefly in Section III and then enumerated by the participants and the discussants in the sections that follow.

III. THE ARGUMENTS

IS RESEARCH RELEVANCE: SUBTLE ACCOMPLISHMENT

It is undeniable that interest of industry practitioners is dwindling in research published in "top-tier" IS journals, i.e. journals that are seen as representative of the best IS research. Thus, it is difficult to argue that achieving research relevance is a major accomplishment of the IS field.

However, the argument that IS research is relevant in a *subtle* way can be made based on one main conjecture: IS research is highly relevant in an indirect way. That is, it can be argued that the type of IS research that is published in "top-tier" journals provides a foundation for relevant results and further relevant research. The recent discussion about the structure of knowledge in IS development (ISD) and the interpretation of ISD as knowledge work [Klein, 2000] provides a good example for this claim. In addition, often such research is highly relevant as a negative pointer to practitioners, i.e. it does not tell them what to do, but is helpful in determining what *not* to do. This result can also be observed in the ISD literature. The lesson here is that high-quality IS research is broad in scope and that the primary audience for that research are individuals who then either apply that research in industry projects or use that research to produce research that is narrower in *scope* and thus closer to the needs of practitioners, consultants, and action researchers.

IS RESEARCH RELEVANCE: UNFULFILLED PROMISE

The "unfulfilled promise" argument is not as self-congratulatory as the "subtle accomplishment" argument. It maintains that the promise of conducting relevant research is still unfulfilled yet on the path of being fulfilled. It denies the "subtle accomplishment" argument by maintaining that the current IS research culture does not provide enough motivation for its audience to fulfill their role, that is, to apply that research in industry projects or use that research to produce research that is narrower in the *scope* of its relevance and thus closer to the specific needs of practitioners.

After all, those who can apply IS research in industry projects, such as consultants, use other sources of knowledge such as books and magazine articles that address their needs more directly. And those that are willing to produce IS research that is narrower in scope and close to the specific needs of practitioners, e.g., action researchers, do not seem to be given much space in "top" IS research journals (the percentage of action research studies published in "top-tier" IS research journals has been reported as being close to 1% of the total - Orlikowski and Baroudi, 1991; Lau, 1997). But, the "unfulfilled promise" argument maintains that this situation is slowly changing for the better.

IS RESEARCH RELEVANCE: SERIAL HYPOCRISY

The "serial hypocrisy" argument is that, while "relevance to practice" is frequently proclaimed as a virtue in public (espoused theory), in reality practical relevance does not matter (theory in use). What we say we believe we do is quite different to what we actually do. Our existing values and practices, embedded as they are in longstanding institutional practices, ensure that the frequent calls for IS research to become more relevant to practitioners are doomed to fail.

These calls are doomed to fail because IS faculty are rewarded for publishing in top research journals, as opposed to practitioner-oriented journals. Any IS faculty survey of IS journals shows that the more research-oriented journals are more highly regarded than other kinds of journals [Athey and Plotnicki, 2000; Mylonopoulos and Theoharakis, 2001]. The "serial hypocrisy" argument takes the view that proclaiming our relevance to practice (or saying we should be more relevant) is a very worthwhile activity, as long as young IS researchers do not take these proclamations too seriously. We must remember that IS research is done primarily for its own sake and for publication in top research journals.

III. THE PANEL DISCUSSION

The panel discussion involved two main segments, "introduction and definition of IS research relevance" and "presentations and debate". In the first segment Ned Kock provided a brief introduction of the panelists, a description of the goals and format of panel, and a definition of IS research relevance. Kock also briefly described a Web site that was created to enrich the panel discussion (Section V). The presentations and debate segment of the panel began immediately after this.

In the "*presentations and debate*" segment, three of the panelists (Heinz Klein, Paul Gray and Michael Myers), who are senior academics and who are important contributors to IS research, discussed issues related to IS research relevance in ten-minute presentations, illustrating their discussion with examples based on their own experiences. They were followed by two panelists, an academic with a distinguished "relevant research" record (Jack Rockart) and an industry practitioner who contributes to and makes extensive use of IS research (Ray Hoving). Rockart and Hoving commented on the presentations by the three previous panelists, each using five minutes.

At the end of each of the five presentations, Ned Kock invited the audience to provide their opinions and ask questions, which were answered by the speaker and the other panelists. The panel featured a high level of interaction between audience of approximately 250 people and the panelists. The speakers and IS research relevance topics discussed were the following:

- Heinz Klein discussed IS research relevance as a "subtle accomplishment".
- Paul Gray discussed IS research relevance as an "unfulfilled promise".
- Michael Myers discussed IS research relevance as "serial hypocrisy".
- Jack Rockart and Ray Hoving discussed, from an industry-practitioner's perspective, the views presented by Heinz Klein, Paul Gray and Michael Myers.

IV. THE PRESENTATIONS

This section presents the arguments made by each of the panelists. The text also includes new ideas that emerged from the interaction between the panelists and the audience. The texts are not based on transcripts of the live presentations, but on the panelists' reconstruction of what they said at the Conference.

HEINZ KLEIN: RELEVANCE AS A "SUBTLE ACCOMPLISHMENT"

Introduction

My basic point is that IS research accomplished much more for the practice of IS than it is given credit for. It is therefore counterproductive for us, the members of the IS research community, to "undersell" our accomplishments, let alone to lament our irrelevance, because it does not help us to identify the direction and priorities for improving our future contributions. Hence, my argument is not that we are as relevant as we could be, but rather that we made essential strides that need to be better recognized and that we need to reallocate priorities and resources to achieve a new level of practical relevance.

In the following I will briefly elaborate on how I support the above claim that IS achieved much more relevancy than is commonly acknowledged. For this claim I draw on analysis of the literature in IS development to illustrate my ideas with an example. There are good reasons to believe that this example would apply to many other specialties in IS, because IS development is important for most specialties. Finally, I present conclusions for lifting the practical relevance of IS as an academic discipline to a new and higher level.

The Basic Claims Elaborated

Much of the criticism of IS being irrelevant is based on an unrealistic and impractical model of what it means to be relevant. The common notion is that Professor Clever does some research and explains to practitioners how they could use it to solve their strategic or day-to-day business problems. This kind of commonly espoused model of knowledge creation and transfer is very unrealistic ("not relevant"). Knowledge is created through complex social communication networks, which feed on each other's results as diffused in diverse media and venues. The last elements in these knowledge creation and diffusion nets are the leading practitioners, who supply the last missing link to create "application" knowledge. These practitioners create "application knowledge" that is similar to the knowledge that a judge creates when applying the law (as a sort of abstract theory) to solve a court case relying in part on other types of knowledge from the legal community including law professors, lawyers, and other judges. In this metaphor, our theoretical research supplies the theoretical concepts and insights that correspond to the body of law regulating a domain of judicial practice, such as business law or family law.

My second claim is that much action-relevant knowledge does exist, but it is inaccessible, because the current coding of relevant knowledge is highly inefficient, typically embedded in "difficult-to-understand" articles published in premier journals. In principle, two issues contribute to this syndrome:

- Language issues (e.g. Benbasat and Zmud's [1999] point on "article's readability",) that cannot easily be resolved by dropping "polysyllabic jargon".
- Storage and retrieval difficulties connected to the constant changes in vocabulary both in industry and academia.

While it is certainly true that some jargon is empty and some unnecessary - and that industry is just as guilty as academia in producing this kind of jargon - much of it does signal important shifts in perspective and substance. Because of the ever-changing terminology (mixing old wine with new in totally new bottles), IS as a discipline does not have an effective way of pulling related bits of application knowledge together and indexing it in an efficient way. Just as many other applied social sciences, IS lacks effective information coding and retrieval schemes when compared, for example, to law, medicine or chemistry, so that relevant knowledge can easily be found "just in time" when needed. (In Iivari et al. [1998] we elaborate this aspect under the heading of "representational power" using the confusing IS development methodology as an example). Note that this diagnosis is quite a different from saying that our research is simply "irrelevant".

In a forthcoming paper my colleagues Rudi Hirschheim, Juhani Iivari and I did an extensive literature analysis on the body of knowledge in information systems development that might be typical of the core competency of IS in systems development as opposed to the specializations of software engineering in computer science[1] or strategic planning in management. In this context, we found that the relevant body of knowledge of IS development is very widely dispersed over many books, journals, and other publications and hence is extremely difficult to retrieve. However, there does seem to exist a specific, practically relevant body of process knowledge for information systems development in IS differing from SWEBOK [2001]. On the other hand, a good way of representing and retrieving this knowledge does not exist. To illustrate these claims, let me just point out here that the usual business case format for communicating practical knowledge to practitioners is unsatisfactory [Ivary et al. forthcoming] for the following reasons. We currently have three types of cases, none of which are very well suited to communicate application-oriented knowledge to experienced practitioners:

[1] Known as SWEBOK and available at www.swebok.org

- Research Cases (positivist, interpretive or critical): These cases report something new, make a point that claims to be new knowledge, and emphasize method.
- Teaching cases: These cases call for a solution applying something learnt beforehand and are for students, not for experienced practitioners.
- Illustrative cases (usually "vignettes"): These cases demonstrate an abstract concept or idea in a concrete setting (cf. the example of applying core competency to work force development). They are somewhat useful for communicating new ideas, but fall short in details for communicating the full range of application knowledge.

Given that abstract categories by their nature cannot capture application knowledge, we have a serious communication problem for application knowledge even when application knowledge exists in the recognized quality research literature.

Conclusions

The above considerations are only a sketch of my argument that would need much more space to be properly developed. It leads me to the conclusion that we need to distinguish four types of knowledge in IS research and give all of them the same level of recognition:

1) Theoretical knowledge of the descriptive-analytical or normative analytical type (e.g. rational decision models). So far IS research made the greatest progress in this category.

2) A second type of knowledge is ethical knowledge that helps with reasoning about value issues. This type of knowledge, which Aristotle conceived as a field distinct from the theoretical sciences, is also theoretical and captured in various ethical theories. So far, IS research mostly ignored this type of knowledge. Yet, it is very important for acting in practice as most important practical problems entail serious value issues that should not be left to unguided seat of the pants decisions or power plays [Klein, 2000].

3) A third knowledge category is technical knowledge, consisting mostly of rules of skill or practical know-how with or without a theoretical grounding. Such knowledge is often systematized into techniques that can be followed like a recipe to achieve well-defined objectives. IS research contributed substantially to this type of knowledge.

4) The last category, application knowledge, builds the bridge between the previous three categories and complex real world situations. It is important to recognize that such knowledge is of the interpretive type and is created in the act of successfully applying knowledge of the first two types and, to a lesser degree, of the third type. It is partly theory grounded and partly based on experience, especially in areas for which no satisfactory theory exists. We suspect that the areas for which no adequate theory exists are numerous in IS practice. Application knowledge has a very personal dimension. Being tacit rather than explicit, it often cannot be articulated even though it may be shared among a community of practice. It is therefore an important separate category (that was first described in Aristotle's Nicomachean Ethics). Application knowledge is substantially more than mapping out conclusions following from some piece of theory research.[2]

IS, as an application-oriented discipline, is most deficient in research into the ethical and application knowledge (types two and four), but relatively successful with theoretical and technical knowledge (types one and three). Yet, the first three types are just as important for practice as the last type, because they represent intermediate products. To say that theoretical knowledge is irrelevant for practice is like saying that raw materials are unimportant for manufacturing consumer products, because the consumer cannot use them. Therefore, we have reasons to be proud of the accomplishments in theoretical and technical knowledge. To call theoretical knowledge "impractical" or "irrelevant" amounts to denying the interdependences that exist among all four knowledge categories.

In the future, if we are serious in our commitment to become more directly relevant (to manufacture more "consumer products" from our IS research knowledge base), two new research avenues need to be pursued.

- We need to create a better understanding how successful communities of practice (such as medicine or law) create and transfer application knowledge. This

[2] A forthcoming paper with Rudi Hirschheim "All Quiet on the Western Front? Reflections on the Current State of IS Research", devotes a whole section to elaborating on this type of knowledge.

understanding is similar to what Markus [1997] called "the appreciation of practicality in IS research".

- We need to allocate resources to investigate application knowledge. This requirement cannot be met by simply insisting that each theory paper also points out some practical application. Rather we have to recognize the importance of application knowledge as requiring a separate type of research, for which the current stock of research methods and refereeing standards are not well suited. The best of the currently known research approaches is action research, especially if conducted with the "detective mindset" discussed next.

Apart from action research, we can start with investigating how the network of the IS community transforms knowledge from various venues into different formats and representations, some of which are then actually applied in a practical situation. By "IS community" I mean the whole network of people engaged in the IS-related knowledge creation and diffusion industry, consisting of university based researchers, industry researchers, IS consultants, editors of various type of journals, trade journals reporters, conference organizers, providers and professional training course instructors for experienced practitioners ("intensive, hands-on seminars"), and perhaps others. Case research could be adapted for this purpose, but it currently lacks a detective-like mindset to discover where the knowledge comes from that informs practitioners. Rather than merely describing what the problem was and how it was resolved, we need to approach case research with the mind frame of an investigative reporter. How did the different types of practitioners involved in a practical problem solving process come to perceive "the problem" in a certain way? How did they choose to represent and communicate it (and why)? Last, but not least, on which knowledge sources did they draw to resolve the problem, how did they become aware of these knowledge sources, and what enabled them to appropriate their "products"? Substantial resources will be needed to initiate and pursue this type of research.

PAUL GRAY: RELEVANCE AS A "UNFULFILLED PROMISE"

Introduction

In May 2001 the Communications of AIS, which I edit, published 26 articles on IS relevance. I assume that was the reason that Ned Kock, the panel organizer and chair invited me to participate. He asked me to talk about the unfulfilled promise of relevance, which I believe is truly the case.

Academic IS, as represented by the people who attend ICIS, its premiere meeting, is a field that ought to be relevant to practitioners and the work that practitioners do should be relevant to IS academics. Yet, it is rare that an academic attends a trade show (other than Comdex in Las Vegas) and even rarer for a practitioner to appear at an academic meeting unless specifically invited. However, if you look at the two sides, they claim to be looking at and trying to find solutions to the same problems.

I discuss some of the reasons why the promise of relevance is unfulfilled. The focus is on short-term relevance. I recognize that some research will not become important until many years in the future.

Major developments of the 1990's

We think we are relevant, but we miss a large chunk of what is going on in the world of practice. Consider some of the major applications developments in the 1990's. The following is a short list:

- Data warehousing
- Data mining
- Customer Relationship Management
- Knowledge Management
- Outsourcing and ASP's
- Commercial packages replacing custom software

In addition, the 1990's saw the flowering of inventions of previous decades such as systems integration. The topics listed pervade the trade press. Pick up a copy of *Computerworld* or *CIO* magazine and you will find most of them discussed in every issue. Yet, in academia, we're hardly teaching these ideas and we're not doing much research on them either.

To prove the point, I asked the people in the filled room at ICIS 2001 whether their institution taught any two of these subjects as full course electives (not just chapters or mentions in their introductory IS course). Only three or four people raised their hand. As for research, I am an avid reader and subscriber of our major journals. Most of these topics are conspicuous by their absence except for occasional special issues (e.g., Chung and Gray 2000).

I grant that academic IS took E-commerce and rode that particular horse quite hard. Others have used the largesse of the ERP software firms and endured the pain and tribulations of making that software run. These efforts are commendable but E-commerce and ERP are only a small fraction of what is going on. Furthermore, while the research on e-commerce is extensive, particularly by information economists, by comparison very little is being published on ERP.

Causes of the Imbalance

What causes the imbalance between what is going on in the field and what we teach? I offer three hypotheses that could well be used for a PhD dissertation.

1. We, in academic IS, are hooked on the PC. We and our colleagues in other departments have very good equipment in our offices and in our student laboratories. The problems we consider in our courses are small enough that we can get our arms around them. Some would even say that the problems are toy problems. They are easy to teach to students who spent their lives in a PC world. The applications listed above, however, are enterprise applications that involve mainframes or massive computing. Both students and faculty are uncomfortable with that. So, they avoid it.

2. Most academic IS departments offer only a few electives and academics tend to teach the subjects of their dissertation. They also tend to research their dissertation topics and many who supervise PhD candidates insist that their students work on these topics as well. The net result: new topics take a long time to enter the curriculum.

3. Because most IS faculty are (or view themselves as) social scientists, they primarily follow the social science research paradigm. More about that later.

We Lag Industry, Not Lead It

A part of our relevance problem as a profession is that we generally lag industry, rather than lead it. As we discuss in the next subsection, we don't encourage invention so that we can stay ahead of industry. We spend our time studying what is.

What are the implications? Consider the lead time from when a new invention appears in the market to the time research papers appear about it. My contention is that it is usually more than 4 years. Table 1 is a typical sequence

Table 1. Time from Innovation to Publication

Time from Start	Action
0	The innovation reaches the marketplace
6 months	An academic realizes that the innovation is interesting and merits research. This period is the time to recognize the problem
1 year	Six months are spent to plan the research, raise funds, obtain equipment, and formulate the study. With luck, a willing PhD candidate is found to actually do the work.
2 years	A year passes as the experiment is run or the interviews are done or the survey is sent out and all the straggling responses are obtained.
2.5 years	Six months are needed to complete the data analysis and write the paper.
4.5 years	It typically takes two years to get a refereed paper through a top journal and physically in print. (If the first journal rejects it, the time may creep up to 3 years or more).

I may be off by a half year or so in one direction or the other, but the times quoted are typical. The important point is that in a rapidly moving field such as ours, by the time the first papers appear, the innovation is already history. This result is one of the reasons why other fields make the charge that IS papers report on what was, not what is or what will be.

Another reason for the lag is that, unlike science or psychology, the objects of our studies change as technology changes. Our results that depend on technology are valid only a short time. Even some things that seemingly do not depend on technology really do. For example, we study the system design process and describe life cycles. Yet, as technology changes, system design changes and so does its life cycle. We are at a disadvantage compared with psychology, for example, where human behavior remains relatively fixed and data about 19-year old sophomores applies to people of most ages.

Most of our work is single author (or at best two authors). Sure, if it's a dissertation, some faculty members stick their name on the paper so the team looks larger, but it is really the work of the PhD candidate. The single author syndrome is reinforced by tenure committees whose preferred model is the lonely researcher burning the midnight oil. Industry is able to accomplish large tasks that academia can't even attempt because it assigns large teams to work on problems. The result is that we cannot (or do not) tackle large or interdisciplinary problems. Our being individually smarter than people in industry is not enough.

What We Don't Encourage

We are stuck in the social science paradigm. This paradigm leads to very elegant experimental and survey studies with superb statistical analysis. Many in our field are in schools of business where the social science paradigm reigns. The net effect is that we focus very narrowly and we ignore large portions of our field because they don't follow that paradigm.

Let me give you an example. In CAIS, Volume 7, Nos. 1 and 2 [Au, 2001, Ball 2001] two doctoral students at the University of Minnesota plaintively cry out for studies of "design science". They define "design science" from the literature. To Nunamaker et al. [1990] design science is applied research, or research that applies knowledge to solve practical problems. To March and Smith [1995], design science attempts to create things that serve human purposes, as opposed to natural and social sciences, which try to understand reality. Design science is technology-oriented. Design is a key activity in fields such as architecture, engineering, and urban planning that may not be thought of as "sciences" per se. Au's and Ball's point is that, although IS roots are in design science, it is no longer the mainstream of what we do. From my point of view it is symptomatic of our malaise that it takes trainees to remind us of what we could and should be.

We are not inventing new things. Our competitors in computer science are widely sought after because they do invent. They created a veritable cornucopia of languages, data models, the mouse, the internet, electronic mail, and much more that is highly relevant. The list in IS is relatively short. After group support systems, telecommuting, multi-dimensional data bases, and critical success factors, the list becomes skinny.

Where We Have the Advantage

It is clear that it will take a long time to make design as important as social science. So, if we are to be relevant, we will need to play to our strengths. We do have the advantage in a few areas [Westfall 1999]:

- Doing research contrary to commercial interests (e.g., the work on spreadsheet errors [Panko 2000], the dirty secret of Excel and Lotus).
- Seemingly unsolvable problems (e.g., software failures [Glass 1998]).
- Practical issues not economically attractive for firms to pursue (e.g., both the University of Arizona and the Center for Research in Information, Technology, and Organizations (CRITO) at the University of California at Irvine work on such projects. They are jointly funded by the National Science Foundation and industry).
- Management issues, where our affiliation with business schools pays off.
- Teaching IS and using IS in teaching (e.g. in distance learning).

I'm sure there are other such niche markets.

What Can Be Done?

We should look not at our many shortcomings in fulfilling the promise of relevance but at the opportunities that are in front of us.

The first opportunity is in electronic publishing. The officers and Council of AIS decided that as a new, leading edge society we should publish the society's journals electronically rather than in hard copy. The result are two journals (CAIS and JAIS) that can use the latest media and techniques for disseminating knowledge.

A second opportunity is in broadening our model for what is acceptable for dissertations and research. We have a lot to learn from science and engineering here. We need people to create rather than observe; to consider creating something new as important as studying something old. This opportunity requires changes not only inside IS departments and groups, but also in what work the top journals accept and the views of tenure and promotion committees on what research is appropriate for promotion.

Third, we can broaden our scope by encouraging team projects and sponsored projects. For example, the University of Arizona created a long-term team operation around its group decision support studies just as the University of Minnesota did in previous years with its graphics research.

Fourth, we can be entrepreneurs and find monetary support for relevant research. To do so, many of us will need to go against a business school culture that thinks it is somehow beneath academics to obtain corporate and government funding to do research.

Most important, though, is a change in viewpoint. If information systems is to be perceived as relevant as a profession then we must look forward not backward. We should help create the future, not just study the past.

MICHAEL MYERS: RELEVANCE AS "SERIAL HYPOCRISY"

Introduction

I would like to start by making it clear that I am enthusiastic about and supportive of practice and making our research relevant to practice. In fact I was a practitioner in the IS industry just over ten years ago (I worked for IBM). I am also keen to see initiatives such as *MISQ Executive* succeed.

However, I believe that all of us in the IS research community, and I include myself here, are guilty of serial hypocrisy. That is, what we say we want, and what we REALLY want, are two different things.

In other words, while relevance to practice is frequently proclaimed as a virtue in public, in reality practical relevance does not matter. What we say we believe we do is quite different from what we actually do. Our existing values and practices, embedded as they are in longstanding institutional practices, ensure that the frequent calls for IS research to become more relevant to practitioners are doomed to fail.

Argyris and Schon [1974, 1978] make a useful distinction between "espoused theory" and "theory-in-use." Espoused theory is the world view and values that people believe their behaviour is based on. Theory-in-use is the world view and values implied by their behaviour, or the maps they use to take action. They suggest that people are unaware that their theories-in-use are often not the same as their espoused theories, and that people are often not aware of their theories-in-use. In other words, there might be a big difference between what people say they want and do and what they actually do.

Espoused Theory in IS

An espoused theory is what we say we believe and what we say we do. I believe that are at least four things that we often say about our field.

Firstly, we usually describe IS as an applied discipline. We say that it is concerned with the application of IT in organizations. The implication of this statement is that IS is not concerned with "pure" research. We are by definition an applied, as opposed to a pure science, and hence intimately concerned with the practical application of IT in organizational and business settings.

Secondly, we say that IS research should be relevant to practice. The calls for IS research to be relevant to practice seem to be made on a regular basis. And because we are an applied discipline, many of us feel guilty when so many articles are clearly not relevant to practice. But we think IS research should be, and it would be a good thing if more articles were.

We want more practical articles, ones that can be used by IS practitioners, as opposed to ones that are too theoretical or philosophical.

Thirdly, we say that practice is the touchstone of good IS research. A good research topic is one that is relevant for IS practitioners.

Fourthly, we say that the audience for IS research includes IS academics and practitioners. In fact, many IS journals in their call for papers explicitly ask authors to spell out the relevance of their research for IS researchers and IS practitioners. Some require a specific section discussing the relevance of the article for both audiences. Thus it is commonly assumed that the audience for our research includes both groups.

Theory-In-Use In IS

Theory-in-use relates to what we actually do (as opposed to what we say we do). At least four points can be made here.

Firstly, our two top IS journals, *MIS Quarterl*y and *Information Systems Research*, became more theoretical and less practical over the past decade. In fact both journals no longer accept what used to be called "application articles." Rather, all articles now need to have a strong theoretical base. In reviewing articles submitted to these journals, the emphasis is on the appropriate use of theory and the contribution to IS research, rather than on the practical applicability. The editorial guidelines for both *MIS Quarterl*y and *Information Systems Research* explicitly state that any research articles without an adequate grounding in theory will be rejected immediately.

Secondly, the audience for these and our other top journals is primarily academic. The Society for Information Management (SIM) stopped bundling *MIS Quarterly* with its membership dues some years ago because many of its members found MISQ of no practical use (although a significant number of SIM members still subscribe to MISQ). A related point here is that, if the audience is supposed to include IS practitioners, why is it that almost all the reviewers of academic papers in our top research journals are academics? If we were serious about having an IS practitioner audience, I believe that we would insist on having at least one IS practitioner on the review panel for every manuscript. The fact that we never do illustrates that the audience is primarily academic.

Thirdly, despite many proclamations about the value of IS research being practical, the reality is it does not matter. As stated in our first point, both *MIS Quarterl*y and *Information Systems Research* became more theoretical and less practical. But rather than suffering as a result, the reputation of both these journals continued to grow (at least within academia and within most business schools). In almost all the various journal ranking surveys done by IS faculty, both *MIS Quarterl*y and *Information Systems Research* are listed at the top [Mylonopoulos and Theoharakis, 2001; Whitman et al. 1999]. The strong reputation that these journals continue to enjoy is clear evidence that practical relevance is not important (as far as the academic community is concerned).

Fourthly, IS faculty are rewarded for publishing articles in first tier journals. Those of us who have been involved in promotion and tenure meetings know that what counts the most is peer-reviewed articles in top journals. Our entrenched promotion and tenure systems in universities reward the publication of academic articles in these outlets.

Thus I believe that our existing values and practices, embedded as they are in longstanding institutional practices, ensure that the frequent calls for IS research to become more relevant to practitioners are doomed to fail. While I agree that proclaiming our relevance to practice (or saying we should be more relevant) is a very worthwhile activity, I advise young IS researchers not to take these proclamations too seriously. The most practical thing they can do is to focus on their research. That way they are far more likely to succeed in having their research articles published in peer-reviewed academic journals. And that way they are far more likely to get promotion and tenure in a good school.

JACK ROCKART: COMMENTS ON THE PRESENTATIONS BY KLEIN, GRAY, AND MYERS

The three speakers argued their points well. There is something in each of the talks with which I can agree although, as you will see, I tend to favor the last two.

To some extent, I agree with Heinz Klein. He points out the need for different types of research – including what he terms “application knowledge” which I gather is relevant research. In addition, I agree that we have seen instances of research that appeared to have little pragmatic value turn out later to be of major importance to management. He named some of these. I would add the work by Peter Chen on E-R modeling which appeared academic at the time of its origination, but proved to be of great use. Unfortunately, much of what I see being turned out by many IT faculty looks to be merely extending theory, often in minor ways, and rarely providing new and vital approaches to the field.

It is when I turn to Paul Gray and Michael Myers’ arguments that I feel most comfortable. I would like to stress three of Paul’s points. First, most other professions honor both theoretical (basic science) and clinical research. This is true in medicine, in engineering, and in many other disciplines. In the effort of business schools to become the equivalent of our academic counterparts, however, we currently reward only the former. Perhaps it is time to provide tenure to outstanding “clinical” researchers in our schools – and especially in our field where we can be of significant assistance to practicing managers. Today, in particular, CIOs are far too busy to work on developing new concepts and new frameworks by which to understand the rapidly changing IT environment.

Second, there *is* a definite need for what Paul calls “entrepreneurship”. By his definition, we have been practicing entrepreneurship at the Sloan School’s Center for Information Systems Research (CISR) for the past 29 years. An average of 20 corporate sponsors a year provided us with the money to do research – between $500,000 and a million a year. Perhaps more important, to ensure that their money is well spent, our sponsors provided us with insights into their key issues and served as research sites as well. We draw on both theory and our management knowledge as we work on these issues primarily through field-based research. Three or four research seminars a year allow us to present our work, test our ideas, and learn more both from our sponsors and the additional companies who provide us with research sites. Our work is clearly relevant and sometimes path-setting – e.g. Scott Morton’s DSS and Weill’s work on infrastructure.

Third, good publishing outlets for relevant work is important. As is becoming more widely known, a group of us initiated a new journal – Management Information Systems Quarterly – Executive (MISQE). It is aimed at presenting academically sound relevant research to managers, but also to academics who will use it in their teaching as well as a source of research ideas. We received many excellent articles and will publish in the first few months of 2002.

With regard to Michael’s piece on “serial hypocrisy”, I could not agree more strongly. Every untenured faculty member needs to know what the real rules of gaining tenure are and should not listen to the siren of “relevance”, especially if he is on the faculty of one of the “top” schools. Developing new theory or extending old in major ways is what is rewarded…no matter what is stated. There may be places where this is not true, but I have not found them. To make this point most strongly, I would urge non-tenured faculty <u>not</u> to submit to MISQE but to aim at MISQ or Information Systems Research.

RAY HOVING: COMMENTS ON THE PRESENTATIONS BY KLEIN, GRAY, AND MYERS

My perspective is somewhat different, having been a practitioner in the IS field for 30 years, a consultant for the recent 5, and only the equivalent of a year or two in academia as an adjunct professor and lecturer. As a member of this panel, I am representing the Society for Information Management (SIM), an organization of primarily practitioners (CIOs, Directors) in industry. I assume practitioners are at least one intended target for research findings.

First of all, I have great empathy for academics in our field. IS is based upon the business application of computing technologies. Compared to other disciplines such as engineering or finance, our field is new and evolving rapidly. Fundamental theories are hard to find. Applied research has a short life span of value. Meanwhile, IS professors are expected to keep their course material up to date and publish findings under paradigms established in more traditional fields.

Like Jack, I found some agreement with all 3 of the presenters. I resonated most with Paul Gray’s comments.

Heinz makes a good point that theoretical research is often misunderstood by practitioners who are too close to today's thinking. Breakthroughs in the representation of data as described by Heinz, are examples of theory overcoming tradition to become new practices. Because of our early stage in evolution, there is much room for new IS theoretical breakthroughs. I imagine many young aspiring professors dream of hitting that homerun theory. Unfortunately, many strike out along the way. This makes most theoretical work less relevant to either academia or the practitioner. Nevertheless, efforts should continue in this area.

Paul makes very valid points that academia is not connecting well with the world of practice and thus lagging rather than leading industry. Some of this lag is the result of the limited time IS professors are given to keeping up with industry trends. It is also because many academics have never had practical experience, and have a very hard time relating to applied topics. Tom Davenport wrote: "Academics have largely dropped out of the race to invent concepts and approaches that help managers deal with IT in business…it's left to the consultants". Unfortunately, Davenport's is a strong indictment and a trend I believe should be reversed.

Thanks to Michael's boldness, he got us to acknowledge the hypocrisy of some academic behavior. It caused me to make the facetious remark that the inmates are in charge of the asylum! Being the first to admit lack of understanding and exposure to academic behavior, it seems to me that some of the reward structures are out of whack. This misalignment includes the pecking order of journals, requirements of tenure, single author preferences, and reward for theoretical versus applied research. And further, it seems not much can be done about it. An academic friend of mine once said it is "easier to change the course *of* history, than a course *in* history." Charles Darwin once said, "It's not the strongest of the species that survives, nor the most intelligent, but the one that is most responsive to change." Businesses that do not respond to changes in fundamental technologies and marketplaces do not survive. The same thing holds true for academia.

From the view of the practitioner, I see the following expectations for good academic research:

- **Relevance.** Webster defines relevance as "important to the matter at hand". Therefore, if the intended audience is the practitioner, then, by definition, the material needs to be important to what's on their mind, not what the academic wants to research.
- **Rigor.** I read several articles debating relevance versus rigor in preparation for joining this panel. Quite frankly, I do not believe it is an either/or proposition. The practitioner expects both relevance and rigor no differently than a business executive expects both an insightful strategy along with executable tactics. There is, however, a diminishing point of rigor return. The Chairman of my former company once reflected that our engineering mentality "Would rather be precisely wrong than approximately right." Far too often, the search for rigor excellence by academics causes a detailed analysis of unmeaningful data. I believe they should back off the sanctity of rigor and accept the notion of getting things approximately right.
- **Responsiveness.** Publishing cycles measured in months or years rather than weeks may be okay for more traditional fields such as mathematics. However, use of such cycles will guarantee that most any applied IS research will be relevant yesterday, not today. In my opinion, there no longer is a valid excuse for such lengthy publishing cycles. In our field, electronic publication has become the norm. We no longer have to wait for long printing and book distribution cycles. The other excuse that sufficient time must be given for thorough critique is valid only to a point. I have been asked to critique papers for publication. I suppose out of courtesy to the reviewers, we are given months to look over 20 or 30 pages of manuscript. A solid few hours is all that is really necessary. Cycle review times should be significantly compressed in order to publish applied IS research in time for maximum use.
- **Readability.** Readability is a pet peeve, although I will be the first to admit calling the kettle black. I find academic papers very hard to read for at least 2 reasons. First is the use of fancy words, creating a false air of intelligence. I think such papers mask the reality, which is a lack of substance. Perhaps paper length should be limited by

> syllable count rather than word count! Secondly, academics seem to have to cite all their research before drawing their own conclusions. That's okay, but up to a point. I reviewed a paper the other day that must have had 90% of it paraphrasing other people's work, and the remaining 10% attempting (and failing) to come up with something new. There's got to be a better way to attribute the thinking of others while presenting original thought.

I thought Jack Rockart summed it up very well: get the right message for the right audience. Until the reward structures in academia change, professors aspiring for tenure must do theoretical research and publish papers for other academics to read. Those papers should not pretend to be relevant for practitioners. Once tenure is out of the way, I would hope professors would be willing to interface closely with industry, find out what are the relevant topics and issues, and then go about researching for insightful conclusions. We practitioners really need this help. We need it from people whose motives are primarily to share knowledge rather than to sell consulting services.

There are positive cases of how this can be done. Jack's MIT Center for Information Systems Research (ISR) is the most well known. SIM's Advanced Practices Council (APC) is another excellent example. In APC, the topics are chosen by practitioners, but deliberately focused towards advanced, less explored, areas. The research is conducted mostly by tenured professors. They produce breakthroughs in applied thinking that are very relevant to our industry. They are judged and rewarded by their customer, in this case, the member companies of the Advanced Practices Council. The real benefit goes to our profession, as these findings are eventually published to all SIM members.

IV. PANEL WEB SITE

Given the complexity and controversial nature of the topic and limited time available at the conference, a Web site was created to provide additional information and extend the panel discussion beyond the conference. The Web site was inaugurated approximately two months before the conference, and maintained as an archival resource indefinitely after the conference. The Web site is available at: http://www.mis.temple.edu/kock/ICIS01

The Web site contains links to the panelists' personal Web pages, a RealVideo welcome message by the panel chair, a description of the panel, a listing of IS research relevance resources on the Web (including key references of relevance to the panel), and an online discussion forum. The online discussion forum was open to the public and received postings from approximately two months before to two months after the conference, after which it was kept as an archive.

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