Communications of the Association for Information Systems

3-13-2006

Beyond Rigor and Relevance Towards Responsibility and Reverberation: Information Systems Research That Really Matters

Kevin C. Desouza
University of Washington, kdesouza@u.washington.edu

Omar A. El Sawy
University of Southern California, elsawy@marshall.usc.edu

Robert D. Galliers
Bentley College and London School of Economics, rgalliers@bentley.edu

Claudia Loebbecke
University of Cologne, claudia.loebbecke@uni-koeln.de

Richard T. Watson
Terry College of Business, University of Georgia, rwatson@uga.edu

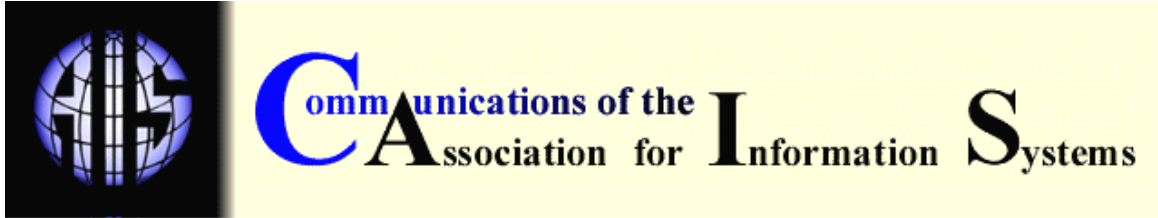Follow this and additional works at: https://aisel.aisnet.org/cais

BEYOND RIGOR AND RELEVANCE TOWARDS RESPONSIBILITY AND REVERBERATION: INFORMATION SYSTEMS RESEARCH THAT REALLY MATTERS

Kevin C. Desouza
University of Washington, USA
kdesouza@u.washington.edu

Omar A. El Sawy
University of Southern California, USA

Robert D. Galliers
Bentley College, USA

Claudia Loebbecke
University of Cologne, Germany

Richard T. Watson
University of Georgia, USA

ABSTRACT

This paper is an outcome of a panel discussion that was held during the 2005 International Conference on Information Systems (ICIS). The panel, titled, "IS Research that Really Matters: Beyond the IS Rigor - Relevance Debate," was organized to foster a discussion on conducting IS research that is significant, in terms of impact, and responsible to the needs of stakeholders. To this end, panel members were asked to push the debate on IS research beyond the traditional arguments of rigor and relevance in research towards IS research that reverberates and is responsible. In doing so, the panel members shared their views on the definition of significant research, who are the stakeholders of IS research and what are their needs, and how do we move the field ahead.

I. INTRODUCTION

Information systems have made and will continue to make significant impacts on our societies. IT-based information systems have had profound impacts on all areas of human endeavor. Most of these impacts have been positive, for example, in finding survivors during natural disasters such as the recent Tsunami. However, in some instances, we have witnessed destruction and suffering resulting from misuse and abuse of IS, the most salient example being the 9/11 attacks that were planned, coordinated, and executed using a wide range of technology infrastructures.

While IT-based information systems continue to lead transformational efforts in our societies, the MIS research community has yet to keep pace with or lead such efforts. Consider, the role of IS in modernizing healthcare delivery, in improving social development in under-developed nations, and in improving the quality of life. IT artifacts are contributing in each of these areas, yet,

mainstream IS researchers seldom pay attention to these issues. It is hence not surprising to find that seldom are IS research and IS researchers sought out for their expertise and knowledge on such critical societal problems as these.

While much of the current debates about the IS field revolve around rigor versus relevance and legitimacy arguments, the debates on tackling societal problems have been all but absent[1]. Moreover, while rigor and relevance are valid concerns for research projects, there are more salient issues that need to be considered as well, namely, the issues of reverberation and responsibility. It can be assumed that in order for research to make a significant impact on society, it has not only to be rigorous and relevant, but it must also look at pressing problems of our society. At the 26th International Conference on Information Systems (2005), Kevin Desouza organized a panel to start the debate on how to move the IS field ahead in terms of tackling problems of critical importance to our society. Panelists were chosen due to their varying experiences and stances on the issues being debated. Claudia Loebbecke, the current President of the Association for Information Systems, and a faculty member at the University of Cologne, brought a European perspective to the debate. Omar El Sawy drew on his experiences with practitioner engagements via the Center for Telecommunications Management at the University of Southern California. Bob Galliers is currently Provost & Vice President for Academic Affairs at Bentley College and is also Editor-in-Chief of the *Journal of Strategic Information Systems*. He discussed issues of globalization and parochialism in IS research and some of the broader issues requiring our attention. Rick Watson[2] contributed experiences from his efforts in creating the zero-dollar textbook and also an electronic parallel of the Peace Corps. Finally, Kevin Desouza, who was just completing his IS doctoral studies at the University of Illinois at Chicago at the time, took the role of panel moderator.

II. GOALS AND ORGANIZATION OF THE PANEL

The panel members agreed to debate on: (1) How to define significant IS research, (2) How we define the boundaries of the discipline and how we might identify appropriate stakeholders, and (3) How the IS community can make IS research more significant by improving the internal processes and practices within the IS domain, and by being more conscious of our external orientations by studying significant societal problems.

Panel members were asked questions in each of the above-mentioned areas, in a three-round format, and each panelist was given approximately five minutes to respond to the questions. Questions were posed in the following order:

Defining Significant IS Research

- How do you define "significant research"?

- What are the key characteristics of research that really matters?

- What would a Nobel Prize in IS be awarded for?

Defining the Stakeholders for IS Research and Understanding their Needs

- Who are the stakeholders of IS Research?

- What are the significant problems confronted by our stakeholders?

---

[1] The work of colleagues in the IFIP WG 9.4 being a notable exception (e.g., Krishna & Madon, 2003)

[2] Bob Galliers and Rick Watson are both former presidents of AIS – in 1999 and 2004-05 respectively.

Moving the IS discipline ahead

- What sorts of problems do you think IS researchers should study?

- How do you best move IS research ahead in order to have significant impact?

- Have we moved beyond Rigor and Relevance to Reverberation and Responsibility?

III. THE VIEWS

Below are the viewpoints of each panelist. During the panel discussion, Kevin Desouza did not share his views, as this would have interfered with his role as moderator. His views are included in this paper, however. Each section will begin with a summary of the views of the panelist, followed by a narrative from each panelist in his or her own voice.

OMAR A EL SAWY

Omar argued against the position that we should look at broad societal problems. His contention was that we must first focus on our own backyard by helping practitioners in organizations, a job we fail to do well even now. Then we will be in a better position to generate useful practices that can be transferred to solve critical problems by others who perhaps know more about the problems than we do. He believes that a prerequisite to doing research that makes a visible impact on society is understanding how to do so, including how to more effectively structure and shape the way that practitioners participate in IS research, while leading rather than following industry practice. Omar asserted that we need to seek more creative ways to use practice to creatively inform theory in the IS area and proposed different ways of mixing academics and practitioners in structured discussions, using his experiences with an industry-sponsored research center. It is his belief that through structuring these interactions more judiciously, it is much more likely we will craft research that is not only theory-driven but also practice-focused.

Defining Significant IS Research

IS research, like all research in other fields, has significance when it advances our theoretical, conceptual, and practical understanding of the critical issues and phenomena around the IS field. The more the research makes an impact and critical difference to the stakeholders and the domain that it serves, the greater its significance.

On some days when I wake up in the morning, I get those spurts of inspiration in my inner self that tell me that we can save the world through IS research: We, the IS academic community, will help resolve the AIDS problem through information systems, help manage natural disasters through electronic dashboards, and help resolve poverty issues and social welfare in poor countries through IT-enabled solutions. But on most days, I see my role as a business school professor and researcher – my role and mission, and the job that I am paid to do, is to help advance the theory and practice of business management through information systems. If my research can help advance our understanding of critical issues which improve business performance and the management of information systems, then I am satisfied that it is making a significant impact. I am not a professor in a school of social work, nor in a school of public policy, nor in a school of public health, nor in a school of environmental science – I am at a school of business that educates future managers and does research that influences the practice of management. The majority of the ICIS community are faculty in business schools, and that is the rule rather than the exception. If IS research can help businesses function better, and businesses can in turn help transform societies, then that is research that really matters. Let us make sure we take good care of our own backyard, before claiming that we will conquer world vegetation.

I think the criteria used by the Alfred Nobel Foundation to award Nobel Prizes provide a very good guideline for research that really matters. Nobel Prizes are awarded for (i) creativity and breakthroughs in research that uncover new understanding that drives the development of a field;

(ii) the development of new technologies and techniques that provide innovative solutions to critical problems in a field; and (iii) doing work that has a large impact on society and the well-being and prosperity of the world.

I believe work that fits the above criteria could be awarded a Nobel Prize. Usually these criteria are satisfied only when work is pursued in depth in the same line of inquiry for a sustained period of time, and the impacts made on the work of others is highly significant. The fleeting and ephemeral nature of IS research focus, as well as the relative youth of the IS field (ICIS started in 1980), make it harder to identify work in the IS field that is worthy of a Nobel Prize at this time.

Defining Stakeholders of IS Research and Understanding their Needs

As a business school professor and researcher, my views of who are the stakeholders of IS research is fairly well defined. They include the business community (managers and professionals who use IS to manage, as well as those who design, build, and manage IS); the non-profit organization community (whether local or global); and the academic community (professorial peers who produce and consume research, journals which regulate and disseminate publication of research, as well as research funding institutions).

I think the most significant problem is that the nature of the interactions between the academic and practitioner community are not structured to yield maximal benefit for research that matters. First, it is only a very small percentage of IS academics who take advantage of the deep intuitive knowledge that reflective managers can have in generating non-obvious research hypotheses from the field. Second, it is also a very small percentage of practitioners who feel comfortable and know how to usefully interact with academics around research issues that lead and impact practice.

Moving the IS Discipline Ahead

I believe IS researchers should study problems that are most critical and central to their stakeholders and that ultimately advance both theory and practice simultaneously. They should also study issues that they have a passion for --- research without passion robs us all of the creative potential that is inside each of us and which produces our best work.

Increasing the effectiveness of engagement within the IS academic community is one suggestion that can move research ahead to have more impact. First, let us make sure we learn from each other and take advantage of cumulative knowledge in the IS field. Scholars have become increasing sloppy over the years in reviewing past work that is relevant to the issue that they are researching. An increasing number of references in articles appear to give lip service to past work, rather than carefully examining it and engaging with it. Review papers that bring together a body of knowledge should be encouraged and valued more than they are now.

There is also a need to better structure the interactions between academics and practitioners so that they can better learn from each other, and thus engage in research that is likely to have more impact and benefit. The Society for Information Management (SIM) annual paper competition, which is a collaborative effort between practitioners and academics, is one good example of a way of doing that. Structured round-table discussions that mix together practitioners and academics are also another vehicle that has been shown to be successful. At the research center that I am part of, we call them "Speak & Spark" discussions, and they help articulate the front end of research projects in a friendly and lively setting.

Let me comment on "Reverberation." Sometimes (and perhaps more often than we care to admit), we see research papers or studies that are both rigorous and relevant, but that are flat and uninspiring. The study is well executed methodologically, and the results have implications for practice, yet the paper is "closed" and does not provoke or stimulate or excite others to push this further. Conversely, there are papers that are perhaps less rigorous and even less relevant, but that somehow "reverberate" across the research community and provoke other researchers to do more studies around them, and even inspire practitioners to come up with innovative

practices. This "reverberation" typically comes about when there is deep engagement between academics and practitioners, and the researchers are passionate about their topic. The pioneering work of John Rockart at MIT is one example of that.

We need to encourage the submission and publication of some articles because they have "reverberation" potential, even if the rigor and relevance is reduced slightly. We should also ensure that the IS research community be global and take advantage of that. We should also remember that there is much to learn from third world countries about IS and the adoption of new technologies. I learned more about the design of information systems for strategic decision making in messy turbulent environments from work I did in Egypt with the Egyptian Cabinet than I possibly could have learned in a developed country setting, and it helped me in my later research work. The language barrier is not a major issue. I think the hurdle is the ability for global engagement with a broad-minded perspective that assumes every setting has something unique and useful to offer.

Finally, on the pragmatics, risk-taking in IS research at the doctoral student and junior faculty level requires the guidance of a senior mentor who can make sure that there is a safety net. In a portfolio of research projects, having one risky project may be worth the adventure if you are passionate about it. Having a risky portfolio, however, is not a good idea at the junior level from a career perspective.

ROBERT D GALLIERS

Bob Galliers argued that, while we may not as yet be ready to deal with broader societal problems, we need to change our stance on this very quickly indeed if we are to remain at all relevant. He asserted that we need to be less parochial and constrained in thinking about what is the appropriate subject matter of the information systems field, both in terms of what we consider to be the IS domain, and of what we consider to be appropriate research and research outlets. Unless we are ready to embrace diversity and richness of the IS field, both in terms of idea generation and research publication, we will not be poised to make a strong and significant contribution to larger societal problems.

Defining Significant IS Research

The word 'impact' comes to mind – does our research really have impact? This is not just in terms of the number of citations and the manner in which it informs thought within the IS academy, but it relates to who it is we are trying to reach – our stakeholders. IS is an applied field. We have therefore to define who it applies to, but first we have to identify the boundaries of the field. There has been much debate of late, sparked by Weber (2003) and by Benbasat & Zmud (2003), about what really matters to the field of IS. Their focus is on the IT artifact and on the design of IT-based IS. I draw the boundary much more broadly. IS, to my mind, relates not just to the design of IT-based systems in organizations, but, for example, IS in society, the digital divide, impacts on society, IT governance, issues of security and privacy. The impacts therefore need to be related to these issues and to the relevant stakeholders associated with each.

Finland's Millennium Technology Prize – that country's equivalent to the Nobel Prizes –was awarded to Tim Berners-Lee, now of MIT, in relation to his work in developing the WWW (http://www.millenniumprize.fi/). I would give Tim Berners-Lee a Nobel Prize as the World Wide Web has had a significant impact on both the practice and research of IS, and computing in general.

Defining Stakeholders of IS Research and Understanding their Needs

In light of my answers above, it would come as no surprise that I say "the human race" are the stakeholders. But stakeholders will differ depending on which aspect of our field one is concerned with. If, like Weber or Benbasat & Zmud, one's concern is the design of IS, then presumably one's stakeholders are confined to the developers and users of the IS, and those who are impacted by it. But, if one is concerned with the digital divide, then one focuses in on members of

society – but which society? In the West, it might be the technological have nots in comparison with the technological haves – the disadvantaged, in inner cities and in rural areas, and politicians and policy makers. In sub-Saharan Africa, for example, the stakeholders are an entirely different group, and their problems are also very different. If, as some do, we cast our boundary more tightly to the academy, then we might see our stakeholders as our peers, but also our students. For example, does our research impact our teaching?

Moving the IS Discipline Ahead

I would argue for expanding the focus of our work to include societal issues, and issues that really impact us as human beings, rather than being wedded to the much more narrow agenda that has been the norm till now. In North America, we can see parallel agendas, depending on whether one has a CIS or MIS focus. In Europe, there has been a broader tradition. In Scandinavia, for example, even the question of IS design has been viewed much more as a socio-technical issue than has been the case elsewhere. Matters associated with the work environment, of worker emancipation, of ergonomics, of communication would come into play, rather than simply technical design. There has been a somewhat similar approach in the UK, with the work of Land, Mumford and Checkland being seminal. For example, Mumford bases her approach on principles enunciated by the socio-technical school that first found form in the work of the Tavistock Institute after the Second World War. Land talks of both formal, designed systems and informal, undesigned systems that work together in tandem. Checkland (1981) distinguishes between data and information: "information = data + meaning. " In other words, we apply our knowledge to impute meaning to the output of systems (data) in order to make sense of those data. Computers process data; only human beings process information. Hence, IS is a socio-technical subject, and the boundary of IS has to include human beings – which brings us back to the Scandinavian approach.

In order to move the field ahead, Editors need to decide whether they wish to publish more of the same – to be conservative – or to help in developing our field outside of traditional norms – to be radical. If they choose the more radical path, then they have to select and guide their reviewers more carefully. Editors might also consider publishing more polemic or viewpoint articles. In addition, we also have a responsibility towards making IS research accessible to third-world countries, especially where English is not the dominant language. We claim to be international and global but remain highly parochial (Galliers and Meadows, 2003). Perhaps a small first step might be to ensure that there are non-English speaking colleagues on editorial boards.

For the most part, unfortunately, I do not think we have moved to a position of doing research that reverberates and is responsible. But there are pockets of activity that do reflect the agenda I've been painting. In Scandinavia, there has been this long tradition of concern for the individual as indicated above. The socio-technical school in the UK has a similar tradition. Those whose interests relate to wider societal issues, for example, colleagues in the IS dept. of the LSE, paint the field with broader brushstrokes that incorporate reverberation and responsibility, as do those who incorporate ethical issues into their agenda. In order to be more responsible and to undertake work that reverberates, we have to take greater risks.

CLAUDIA LOEBBECKE

Claudia Loebbecke took the stand that doing significant IS research can be achieved by taking a portfolio project approach to thinking about IS research. A central tenet of her argument is that evaluating IS research projects by the impacts on their stakeholders, especially in a funded environment, can help researchers pick out problems that are relevant, interesting, approach them rigorously, and also articulate significant contributions. She specifically addressed the role of projects funded by industry and their function in stimulating research in cutting-edge areas that are in parallel with industry initiatives or are ahead of current industry practices. She also commented on the knowledge and support systems from the European academe perspective that encourage industry-funded research.

Defining Significant IS Research

Significance - I guess everybody would agree - mainly depends on the stakeholders. The always repeated and old story refers to journals, rigor and relevance, and promotion committees. But the title of this discussion is on IS research that really matters. So, to whom do journals and promotion committees matter? Mainly to the authors, i.e., to us! None of us can ignore this very important aspect of professional life and career building. However, I think we should take a more modest stand and may admit or hope that there is IS research that is significant beyond our personal careers and even beyond the IS community. So I dare to push it as far as saying, I define significant IS research as IS research that is significant beyond academia, being important to real life, and influencing businesses or societal aspects. Significant research must impact people beyond tenure candidates, journal editors, and promotion committee members.

I would not give any Noble Prize in IS. Why? Because, as far as I can think, the most important, far reaching, and impressive contributions that we tend to cover in the IS community really stem from computer science, engineering, or other such disciplines. Just consider the $100 Computer that Nicholas Negroponte wants to distribute in large circles of Africa. Such a project for sure has an enormous impact, but the real innovation was to get a computer built for $100, and this for sure is not IS work. The IS community (at least I myself) would instead at a later point report about the impact of such a significant contribution, e.g., how the $100 computer changed the lives in country A, B, or C, how the impact differed between cities and rural areas, or what immense learning progress and knowledge diffusion could be achieved with it. All these latter issues will be written up in papers to be published in IS journals, but the Noble Prize (if any) would be given for designing, building and maybe connecting all the devices, not for - ex-post - talking about - at best - analyzing them.

Defining Stakeholders of IS Research and Understanding their Needs

Besides the traditional groups (students, managers, consultants, and fellow researchers), I think major stakeholders are those who contribute the money to the research. At least in Europe, they have quite a lot to say. This is true for the European Commission or national equivalents of the NSF, but it applies even more to companies and company networks as funding organizations. They co-shape our research by setting topic constraints and funding limitations, by restricting access to company data access restrictions, by and requiring approval for publishing newly gained insights. These stakeholders expect to get value for their money. Usually such value increases with keeping findings confidential. Of course, this is against the main reason why we do research, i.e., publish. It collides with our interest as researchers: How much do we care about our findings compared to the opportunities of publishing and consequently taking professional advantage of the research?

Being employed in the academic system, I think, we are responsible to a pre-defined audience. But, for the sake of this discussion, and at least dreaming about what really matters, we would be responsible to all those (hopefully many) who could benefit from our intelligent, efficient, well-educated, and very industrious hours of work. How can we talk about responsibility, if we live in a system where practically all the (research) work is for keeping ourselves happy?! What do we - hardworking and scientifically well-trained as we are - contribute to our smaller and larger societies?

Moving the IS Discipline Ahead

The IS field offers a wide range of topics that really matter and can be researched. IS researchers should be future oriented, trying to contribute to a better world, make businesses more efficient, help handicapped people, tackle poverty, improve climate trends, and so on and so forth. In order to pursue IS research that really matters, we would have to give up on our promotion paths. Is that realistic? No! But as outlined above, it would be required. Successfully doing such kind of IS research would have to be reflected in PhD and tenure decisions. So, for now, unfortunately, I would not recommend PhD students or junior faculty to aim for 'IS research that really matters.'

My recommendation to PhD students would be to stick to their career paths. As stated above, not too much IS research that really matters seems publishable or would count for promotion. And, I am afraid, doctoral students and junior faculty members will not be in the position to change promotion criteria – maybe this is something to think about for senior colleagues, the deans, provosts, and promotion committee members in the room.

Unfortunately, we have not moved beyond Rigor and Relevance to Reverberation and Responsibility, and besides, we keep repeating the rigor versus relevance debate all the time. To get started in order to move the IS discipline ahead, though, maybe some senior scholars with enough publications could acquire and design such IS research projects that really matter. Speaking as a European, I am almost positive that in the case of visible real-life impacts, schools would appreciate such efforts. But how do we know who will make it into the news with his or her contribution? So, the question remains how to evaluate the majority of junior researchers who contribute in their little niches, but not up to a level to convince a tenure committee.

RICHARD T WATSON

Rick Watson argued that IS research needs to get closer to wealth creation rather than reporting. Wealth creation (or value creation) is defined as the outcome of someone creating a product or service that someone else consumes. To this end, he focused on knowledge exchange issues in the IS research domain. The closer we can get to wealth creation, the more power IS scholars will have as we in live in a system that is centered on wealth creation. He addressed issues involved with the global exchange of knowledge and presented his concept for an electronic parallel of the Peace Corps. He also discussed how senior leaders of our field can promote significant research by encouraging new types of research undertakings mentoring junior faculty, and reforming the current incentive schemes of our academe.

Defining Significant IS Research

Significant IS research creates wealth or solves societal problems. IS research needs to get closer to wealth creation rather than reporting the wealth creating efforts of others (e.g., studying how companies implement ERP, CRM, or some other system). The closer we can get to wealth creation, the more power IS scholars will have, as we in live in a system that is centered on wealth creation. Scholars in engineering, biotechnology, and medicine, for example, have greater access to grants because their research model is driven by innovation rather than the retrospective social science paradigm of IS. Also, we need to get involved in solving societal problems, and in particular we need to consider the most pressing problem of our times, global warming.

The Nobel Prizes for medicine and science are often awarded to work that changes practice or thinking about a phenomenon. In the case of IS, the only work that I can think of that changed practice was Peter Chen's entity-relationship modeling (Chen, 1976). His work is the foundation of all methods of data modeling.

Defining Stakeholders of IS Research and Understanding their Needs

The major stakeholders of IS Research are other IS researchers, students, consultants, and practitioners. We need to seriously understand the needs of each stakeholder group. Practitioners and consultants are concerned with the timeliness of our research. We operate on different time lines, and our long publication delays mean that much of our work is past its 'sell-by-date' when it appears. Our students need us to bring our research into the classroom to give them an enriching and firsthand experience. This is sometimes possible but highly dependent on the type of research and the class.

A major problem for IS researchers is power asymmetry. The most important people in our business of knowledge accumulation are the authors, and they have the least power. Research shows that it is very difficult for new scholars to gain tenure, and the authors of this research attribute the problem to a 'take-no-prisoners' style of reviewing. Innovation and creativity, and

thus wealth creation, are severely constrained by the power imbalance. Authors are the source of the value, and they should have the most power in a knowledge accumulation system. Another one of the important problems for IS researchers is that we are still hostages to the paper and postage system thinking of the last century. First, our few electronic journals are essentially electronic versions of paper (pdf or html). We do not encode knowledge, and we pay for it in high search costs and lowered productivity. Second, we don't have a theory database, yet implicitly we seem to agree that theories are our most important resource. Imagine a business that did not have a HR or customer database. We would ridicule it, but aren't we rather ridiculous in our lack of adoption of the very technology we study and teach.

Moving the IS Discipline Ahead

The most important problem we face is global warming, and we should be involved in some way in addressing this civilization threatening issue before it is too late, if it is not already. The root cause of global warming is overpopulation (Figure 1). It is well known that educated women have smaller families (Figure 2), so IS scholars should address this issue, which seems to the most amenable to our skills.
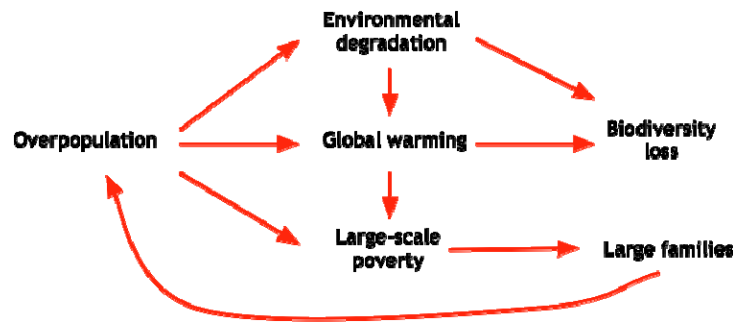
Figure 1: Global Warming
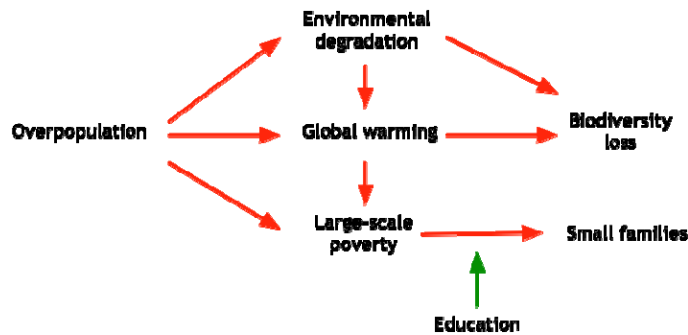
Figure 2: Global Warming and IS Research

With other IS scholars, I am working on creating a parallel to the $100 laptop project – the $0 textbook. IS scholars can design and create the infrastructure for all scholars to become involved in creating free textbooks for developing countries.

In terms of moving the field ahead, I think there is a significant role to be played by our senior leaders. Senior leaders of our field can promote significant research by encouraging new types of research undertakings mentoring junior faculty, and reforming the current incentive schemes of our academe. However, we will not be able to bring about change without making promotion and tenure decisions at the department level. Change and innovation are shackled by tradition and what might have mattered a decade ago. We need to realign incentives with wealth creating and societal problem solving research.

Finally, I don't think we have reached a point of doing IS research that is responsible and reverberates. We have not accepted that we have a responsibility for addressing important societal problems.

KEVIN C DESOUZA

Kevin Desouza was concerned with issues of problem selection and knowledge creation. In his view, the answer to making IS research significant is found in how we choose problems to study. He shared his views on how doctoral candidates should be provided training, counsel, and incentives to take on riskier research projects so as to challenge some of the fundamental assumptions of the field, in the hopes of breaking new ground and doing significant IS research.

Defining Significant IS Research

I make my comments as a doctoral student, and a recently appointed faculty member. I have had the privilege to work on problems that are wicked and difficult to solve in deterministic fashions. For example, some of my recent work examines informational issues in the government intelligence and national security sectors. Here the role of information systems is salient and the study of informational problems can make significant impacts to the state of practice. However, are these kinds of problems appreciated in our discipline? I do not think so. While there are several reasons for this, the most important one is the narrow sightedness of how we define significant research. Unfortunately, to many, significant research is relegated or considered akin to significant statistical results of research studies. This is definitely the case in how we manage the publication of scholarly products.

In my opinion, significant research is that which makes a measurable impact in the quality of stakeholder lives. So, if you define the stakeholders of IS research as managers and executives, than significant research would make a difference in the practice and execution of management.

I would not give a Nobel Prize for any IS research. Nobel Prizes are awarded for research that makes a significant impact on a field both in terms or research and practice. While there have been some instances of IS research that have made an impact in terms of future IS studies, the impact has not been significant in terms of impacting research in other disciplines. Moreover, I have yet to see research that has made a significant impact in terms of the practice of management stemming from IS studies. One reason why we do not have such studies is that most problems examined by IS researchers are studied in silos and are chosen due to the ease by which they can be decomposed into simplistic reductionist models. The outcome is that research does not appreciate the complexity of environments, both at the industry and societal levels. Hence, research products, especially those that are written up in our mainstream journals, seldom get any attention from external stakeholders.

Defining Stakeholders of IS Research and Understanding their Needs

The stakeholders of IS research are all human beings, the entire human race (as noted by Bob Galliers), both present and future. Therefore, the significant problems facing our society become critical research areas for IS scholars. Examples of these include: eradication of global poverty, improvements to healthcare systems, improving the quality of life, development of underdeveloped nations, population control, improving educational systems, and even the improvement of management of organizations. We have a moral responsibility to leave the world in a better place than how we found it. IS research provides us an avenue for seeking ways to

better our societies. IS research must contribute to solving societal problems; else I fear we may never do anything significant.

Moving the IS Discipline Ahead

As the rookie on the panel, I do not have the wisdom or the tenure to suggest much on how we might move the field ahead. However, I can share some thoughts on changing the nature of doctoral programs. Most doctoral programs do an excellent job of training scholars and researchers. We learn how to conduct research investigations, write up our findings, make presentations, and even secure faculty positions. This is all good and well. The area that I think is neglected is the aspect of encouraging risk-taking in doctoral research. Risk-taking behavior should be encouraged and not discouraged during the doctoral training program. Most students are told not to do risky research and to focus on studying traditional problems by augmenting traditional theories. The end result is that they are trained from the onset to value the status quo. Radical innovation is discouraged in favor of getting papers out of the dissertation in top-tier journals in our field, all of which do not yet encourage risky research. I for one rebelled against this and did things that were not considered traditional IS research, and I can honestly say that I learned from all experiences.

The other aspect of IS doctoral programs is the lack of sufficient cross-disciplinary collaboration. IS doctoral candidates seldom collaborate with other doctoral students in other disciplines, especially those outside their primary college. This is a major deficiency that is in need of dire attention. Unless doctoral students, the future of the IS research community, draw on external disciplines to inform their work, we will be encouraging silos-research, which cannot have the potential to make significant impacts. The significant problems faced by our society require trans-disciplinary collaboration for solutions.

I do not believe that we have reached a point of doing research that reverberates or is responsible. I hope that we will someday. In order to do so, we must move away from focusing on the two most noted R's (Rigor and Relevance) and focus on the new R's (Risk, Reverberation, and Responsibility). I believe that while it may be easier for senior faculty to lead the way to reforming IS research, doctoral students and junior faculty cannot resign themselves to playing a passive role. Change must happen both from the top as well as from the bottom. Change from the top can be in the form of ordered and structural reforms to practices, while emergent change can be encouraged from the bottom-up.

IV. CLOSING COMMENTS

It is our hope that the panel at ICIS, and this commentary, will lead to serious discussion in the IS field about reforming current practices of research. IS research has the potential to make significant impacts on our society, and it will remain an untapped reservoir of knowledge and expertise, unless we are able to position and leverage it optimally.

V. REFERENCES

Benbasat, I., and Zmud, R.W. (2003) "The identity crisis within the IS discipline: defining and communicating the discipline's core properties," MIS Quarterly, 27 (2), 183-194.

Checkland, P. B. (1981). *Systems Thinking. Systems Practice*, Chichester, UK: Wiley.

Chen, P. (1976) "The entity-relationship model - toward a unified view of data," ACM Transactions on Database Systems (1) 1, pp. 9-36.

Krishna, S. & Madon, S. (eds.) (2003). *The Digital Challenge: Information Technology in the Development Context*, Aldershot, UK: Ashgate.

Weber, R. (2003) "Still desperately seeking the IT artifact," MIS Quarterly, Vol. 27 (2), pp. iii-xi

Galliers, R.D and Meadows, M. (2003) "A discipline divided: Globalization and parochialism in information systems research," Communications of the AIS, 11, 5

VI. ABOUT THE AUTHORS

Kevin C. Desouza is on the faculty of the Information School at the University of Washington. He is a founding faculty member of the Institute for Innovation Management (I3M) and is an affiliate faculty member of the Center for American Politics and Public Policy, both at the University of Washington. His immediate past position was the Director of the Institute for Engaged Business Research, a think-tank of the Engaged Enterprise, a strategy consulting firm with expertise in the areas of knowledge management, crisis management, strategic deployment of information systems, and government and competitive intelligence assignments. He has authored *Managing Knowledge with Artificial Intelligence* (Quorum Books, 2002), co-authored *The Outsourcing Handbook* (Kogan Page, 2006), *Managing Information in Complex Organizations* (M.E. Sharpe, 2005) and *Engaged Knowledge Management (*Palgrave Macmillan, 2005), and edited *New Frontiers of Knowledge Management* (Palgrave Macmillan, 2005). His most recent book is currently in press – *Agile Information Systems* – to be published by Butterworth Heinemann (2006). In addition, he has published over 50 articles in practitioner and academic journals. Desouza has advised major international corporations and government organizations on strategic management issues ranging from knowledge management, to competitive intelligence, and crisis management. Desouza is frequently an invited speaker on a number of cutting-edge business and technology topics for national and international, industry and academic audiences.

Omar A. El Sawy is Professor of Information Systems in the Information and Operations Management Department at the Marshall School of Business. He is also Director of Research at the Center for Telecom Management (CTM) at USC which is an industry-sponsored research and education center that focuses on the networked digital industry. His interests include redesigning electronic value chains, the design of vigilant information systems for turbulent dynamic environments, and knowledge management around business processes in fast-response contexts. El Sawy holds a Ph.D. from Stanford Business School, an MBA from the American University in Cairo, and a BSEE from Cairo University. El Sawy has lectured, consulted, and carried out research in four continents, has been an IS advisor to the United Nations Development Programme in Egypt, and a Fulbright scholar in Finland. El Sawy is the author of over 70 papers and his writings have appeared in both information systems and management journals. He is the author of the book *Redesigning Enterprise Processes for e-Business*. He is a six-time winner (most recently in 2005) of SIM's Paper Awards Competition which brings together practitioners and academics.

Claudia Loebbecke holds the Chair of Business Administration and Media Management and is Director of the Department of Media Management at the University of Cologne, Germany. Since July 2004 she served as AIS 'President-Elect, taking up the Presidency in July 2005. Before joining the University of Cologne, she also worked for INSEAD, McKinsey &Co., Erasmus University Rotterdam, and Copenhagen Business School. She spent her sabbatical from October 2002 to March 2003 at INSEAD and at the Sloan School (CISR), MIT. Loebbecke is Senior Editor of the *Journal of Strategic Information Systems*. She has published over 100 internationally peer-reviewed journal articles and conference papers. In 2004, she won the second prize in SIM's Paper Award Competition.

Appointed to his current post of Provost and Vice President for Academic Affairs in 2002, **Bob Galliers** came to Bentley from the London School of Economics, where he was Professor of Information Systems and Research Director in the Department of Information Systems. He retains his connection with LSE as Visiting Professor. Before joining LSE, he served as Lucas Professor of Business Management Systems and Dean of Warwick Business School, and earlier as Foundation Professor and Head of the School of Information Systems at Curtin University in Australia. Galliers is editor-in-chief of the *Journal of Strategic Information Systems,* and a fellow of the Royal Society of Arts, the British Computer Society and the AIS, of which he is a past

president. He has published widely in many of the leading international journals on IS and has also co-authored a number of books, the most recent being: *Exploring Information Systems Research Approaches* (Routledge, in press), the third edition of the best seller, *Strategic Information Management* (Butterworth-Heinemann, 2003), *Rethinking Management Information Systems* (Oxford University Press, 1999) and *IT and Organizational Transformation* (Wiley, 1998).

Richard Watson is the J. Rex Fuqua Distinguished Chair for Internet Strategy and Director of the Center for Information Systems Leadership in the Terry College of Business, the University of Georgia. He has published in leading journals in several fields as well as authored books on data management and electronic commerce. His current research focuses primarily on electronic commerce and IS leadership. He has given invited seminars in more than 20 countries for companies and universities. He is past President of AIS, a visiting professor at Agder University College, Norway, Fudan University, China, and a consulting editor to John Wiley & Sons. He has been a co-chair of ICIS and a senior editor for *MIS Quarterly*.