3-13-2001

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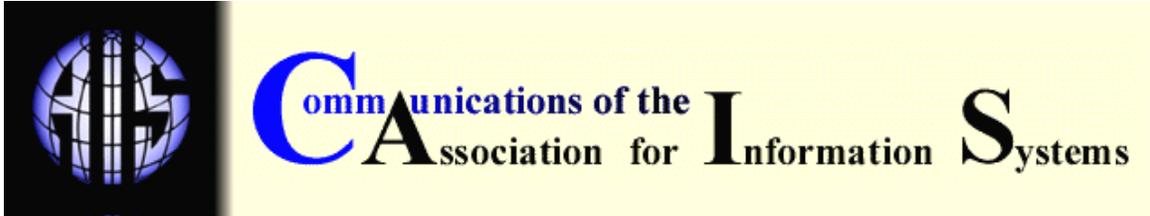Follow this and additional works at: https://aisel.aisnet.org/cais

THREE ISSUES CONCERNING RELEVANCE IN IS RESEARCH: EPISTEMOLOGY, AUDIENCE AND METHOD

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ABSTRACT

Academic concerns about making research into the design, construction and use of information systems (IS) more relevant to practice is a persistent theme in the IS literature and in recent ISWorld discussions. This essay addresses three questions implicit in this discussion: Is there an agreed upon epistemology underlying IS research? To whom should IS be relevant or alternatively what ends should this research serve? Does the choice of research method contribute to the creation of relevant IS research? These questions are explored from the perspective on an unrepentant idealist.

I. INTRODUCTION

The recent ISWorld discourse on the relevancy of IS research to practice covered a fair amount of territory with positions being staked out by those I would term "the pure researcher" and the "in-the-trenches practitioner" with variations in between. The gist of the various arguments expressed in this e-mail thread were directed at three broad points. Those are that:

- academic IS research is useless and pointless largely because practitioners do not pay attention to it.

- it does not matter a wit if practitioners pay attention because, after all, pure research exists for its own sake. Let others find relevance in the work some time/day in the future.

- it is indeed relevant because it educates indirectly, via teaching, textbooks, workshops, method books and so on. IS research therefore informs practice indirectly, even if at some indeterminate later date.

From within those arguments the thread drifted between a variety of issues including opinions about "what constitutes relevance", "what research approaches provide relevance", the temporal and situated nature of relevance, e.g., when is it/might it become relevant, and finally "to whom should we be relevant." I wish to contribute to this discourse in three specific ways.

- First, I reiterate a point from an earlier, though germane discourse in our field. Namely that there is not a single undisputed view of what constitutes 'science' nor is there a uniform epistemology underlying IS research.

- Second, I explore the issue of 'relevant to whom?' or alternatively stated, 'research to what end(s)?'

- And third, I will suggest that one way to assure greater relevance is to use research methods that require relevance.

II. WHAT CONSTITUTES SCIENCE?

it seems to me that implicit in much of the thread about IS research relevance is a predominant worldview with which I do not agree. That is, research requires a degree of distance, stand-offishness, and 'objectivity' on the part of the researcher. Such a view holds that a researcher is an objective observer of what are essentially social phenomena: the interplay of people, technology and organizations within the design, building and use of information systems. The researcher is assumed to be in a privileged position from which s/he is later able to speak for the phenomena observed. This point of view governing research method is a requirement of one branch of science, e.g., that derived from logical positivism, and which many in our field view as the 'normal science.' It is, however, NOT the only view of science. Research arising from different views of science is not constrained by the same limiting assumptions of the first view. It may have its own set of limitations, of course, but we must not assume them to be the same. These limitations do however have a way of bounding what we may see and the outcomes of our research. I suggest that one consequence of the disengaged researcher assumption may inhibit one type of research relevance. I will return to this idea in Section IV.

Many in the field lived through the "great debate" from 1984 to 1995 over quantitative versus qualitative (intensive) research. Others joined the debate mid-stream. Those who missed personal hand-to-hand combat in the "method wars" may have studied them in PhD research seminars around the world. Hence I do not wish to replay the arguments in this space. But if Lynn Markus was accurate in her plenary address at the 1997 Philadelphia IFIP WG 8.2 conference on research method, then the field seems to have come to terms with the multiple epistemologies and ontologies that now inform IS research; a truce has been declared . Remembering a bit of this history is germane because a good bit of that debate addressed the question of relevance. I don't believe that the issue was ever really resolved except that part of the argument revolved around method as a surrogate measure of research relevance.

III. RELEVANT TO WHOM AND TO WHAT END?

If it is important to *be* relevant then, to whom should we be relevant? What ends and what masters should research serve? Let me admit at the outset that I am an unabashed idealist who believes that as academics we should serve mankind and seek to improve the human condition. My value system informs my research, you might even say biases it. I don't believe this to be so, but there are those who believe that research must be value free or value independent. The debates and methods wars of the past fifteen years have dealt with the issue of the value in research so I shan't revisit old territory. But I recommend that anyone interested in exploring this issue further read Bruno Latour's book *We Never Have Been Modern* [Latour, 1993]. It takes on both the positivist antipositivist and modernist and postmodernist position with an equal and deft hand. It exposes the shared assumptions and Achilles Heel of each position. It also illustrates how all research, like all social action, is situated and is the product of many forces, the researcher's values included. Since I do not believe research to be value free I declare this point right up front.

Many of these values are apparent in our literature. Let me illustrate. The organizations and information systems typically studied in our literature are business systems and organizations; those are under the control of management. Moreover many in our field were trained and now are employed by business schools. It should not therefore come as a surprise that another implicit theme (dare I suggest *bias*) in our literature is that of the right of supremacy and the inherent goodness of management. Of course there are exceptions, but even in the research branches

studying government, health care and nonprofit organizations there is a distinctly managerial bias. Our work often privileges the opinions and positions of managers, professionals, and those in control of organizational power structures. Our literature on the diffusion of technology, models of acceptance, and decision-making almost solely examine these issues from the framework of the managing classes. Then we wonder why we don't often get the systems 'right' from the user's point of view. Perhaps in studying our objects of inquiry from the bottom up might yield some surprising insights. Labor unions, community centres and community organizations serving the poor and disenfranchised and powerless need the insights and the skills we have to offer. Moreover, in listening to the voices of these constituencies we learn a lot about the impacts of the systems we construct. It challenges our values, preconceptions and sharpens our research senses. How? As one example there are those in government and industry who are attempting to define away the digital divide with a linguistic and definitional slight of hand by referring to people without internet connections, computers, phones, credit cards or perhaps even banks accounts and credit as 'new markets.' A single field study in the schools and community centers of our inner cities quickly disabuses a researcher of the notion that a digital divide, a kind or digital capital gap, does not exist. The benefit to traditional research of such an experience, is that in seeing a problem through different lenses one may then find new ways to address old issues. It refocuses and challenges ones own worldview.

I personally find work that looks at these phenomena from the bottom up, to be interesting and insightful. I am currently examining enterprise systems form the view of labor; a bottom up examination of the ways and impacts of ERP implementation. It is opening my eyes to ISD and implementation in a host of unexpected ways. I am working with doctoral candidate Lynette Kvasny in exploring the digital divide from within the nonprofit community, working to address issues important to those who are disenfranchised by the very technologies and the infrastructures we celebrate and take for granted.

IV. ASSURING THAT IS RESEARCH IS RELEVANT

The foregoing brings me to the third point. That is, *how* might we assure that our research is relevant?

Perhaps one answer is occasionally to employ research methods that actually require relevance. There are, after all, many analogs in other fields. Take medicine, for example, where practice and research go hand and in hand. In our own research tradition there are methods in which the explicitly expected outcomes include the solving of a problem or building of a system. Some approaches even require that the process of research is a learning process for the researcher *and* the organization. Organizational learning is directly built into the method and research protocols. Once such approach is Action Research. A second approach is called by various names. I prefer the term we use at Georgia State University, 'Improvement Research'. It describes a research approach wherein one actually builds and then tests systems. Both action research and improvement research require the researcher to operate in a kind of a dual mode, that of research (discovery/explanation) and action (service).

In Action Research the domain of the study is an organization with real people facing problems in need of solutions. The researchers and organizational co-actants jointly frame the problem, define the research framework and theories to be used to both study the process and to organize the shot at a solution. Action is informed by theory. Theory is adjusted based on real time action. The outcome goals include:

- solving the problem,

- organizational learning

- theory development or refinement, and perhaps,

- theory testing.

Because it is action and learning oriented, the research is by definition relevant. Like any other research approach it does not guarantee successful outcomes. It may go bust yielding uninteresting or no findings. But when the pieces do fall into place, the outcome is not only relevant, it is also quite satisfying.

Improvement research implies identifying a problem and building, then testing prototype solutions. It differs from explanation or other descriptive research for it too is inherently focused to doing or building something versus explaining a phenomena. As such it arises from a world view in which one goal of the research is to *be* relevant. Quoting from Vijay Vaishnavi [2000] "The goal of improvement research in information systems and technology (IS&T) is to improve the performance and effectiveness of IS&T artifacts." Whereas explanation research methods and traditions are based in natural science and are well established, improvement research methods and traditions are relatively new and are closer to engineering or computer science modes of research.

Improvement research addresses another facet of the "relevance for whom" question. An unspoken orientation in the IS literature seems to be attention to organizational actions relative to the use of IS. However, the socio-technical nature of the issues in IS research also suggests that research be relevant to those who build computing artifacts. For example, Morrison and George, , Markus , and Freeman, Javenpaa and Wheeler, have each argued that IS research should be both more connected to, and say more about, the IT/IS being studied. Studies trying to understand the import of the social impacts of technology should not ignore the technology itself, for it also has role as situational actant Improvement research therefore provides one means to approach the relevance issue from two different perspectives.

V. CONCLUSION

In conclusion, I return to earlier themes. I am of the opinion that for our research to be relevant we must first believe that it *should be relevant*. This statement is not so easy as it sounds for it is a kind of declaration of value. It may require giving up a veneer of objectivity and dispassionate observation. In a way it means taking sides. Therefore, it may also follow that the researcher may need to make an explicit declaration as to whom s/he expects the research to be relevant. If we are to relinquish objectivity, something akin to the ethnographers 'confessional' may be more appropriate. Let the reader as adjudicator of the work judge if the researcher's position is unduly compromised.

The issue of relevance may require a freeing up of the researcher's thought process as to what constitutes methods which may, in any given research inquiry, help improve relevance. Relevance is related to the research topic or question, the audience and the inquiry approach. Thus there is a kind of double dance of research question and inquiry method. This dance may demand that a researcher must be a tad opportunistic in selecting the way a topic will be studied. Finally, bonding with non-managerial sponsors may also be a means for researchers to loosen their own thinking by altering their research experience and research settings. There is nothing like an audience that does not agree with one's own viewpoints to help sharpen one's thinking. Similarly, working among those whose value systems or worldview may be vastly different from one's own cannot help but force a researcher to stretch intellectually, perhaps be startled and even see new meanings in his/her work. Perhaps the work will even become more relevant.

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ABOUT THE AUTHOR

Duane Truex, is interested in the social impacts of IS development, especially the impact on workers and how emergent properties of organizations may be reflected in emergent ISD. Truex is active in the IFIP 8.2 community, on the editorial board of the Information Systems Journal, and is co-editing a special issue *The Database for Advances in Information Systems* on a critical view of ERP systems. He is a professor in the Computer Information Systems department at Georgia State University, a Leverhulme Research Fellow in the United Kingdom at the University of Salford, England, and a visiting Full Research Professor at Aalborg University, Denmark.