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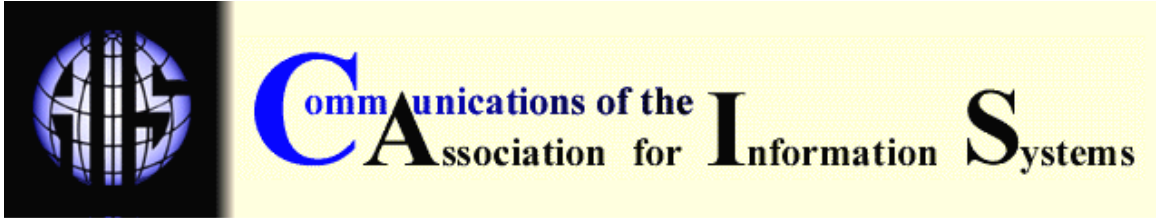
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UNDERSTANDING AND EVALUATING RELEVANCE IN IS RESEARCH

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ABSTRACT

This commentary examines two questions of interest to the relevance debate in IS research: why is relevance a problematic issue in IS research, and how can IS research relevance be assessed accurately and fairly. Answers to these questions are centered around the pluralistic and dynamic nature of IS research: that IS research is influenced by multiple stakeholder groups with conflicting perspectives, and continual changes in this domain prevented the formation of a shared consensus of what IS research should be. Since such a consensus is unlikely to emerge, given the nature of the discipline, any evaluation of IS research relevance must take into account the target audience for that research and yardsticks appropriate for the intended stakeholder groups.

I. INTRODUCTION

The issue of relevance in information systems (IS) research was first raised by Peter Keen in his keynote address at the 1990 IFIP conference at Copenhagen (Keen 1991). Since then, the relevance issue resurfaced time and again (e.g., Benbasat and Zmud 1999; Robey and Markus 1996; Westfall 1999), but there is still very little consensus on whether IS research is relevant. This commentary addresses two questions at the core of the IS relevance debate:

1. why is relevance a problematic issue in IS research, and
2. how can IS research relevance be evaluated accurately and fairly.

II. UNDERSTANDING IS RESEARCH RELEVANCE

Relevance is defined by Webster's dictionary as "being pertinent or applicable to the matter in hand." Two key issues underlying this definition are "pertinent to whom" and "pertinent how." Research issues relevant to one audience may not be relevant to another, and hence, it is important to clearly define the audience, stakeholders, or benefactors of IS research before engaging in a relevance debate. Such stakeholders include IS and business managers (for funding and learning from research), IS academics (for consuming and evaluating research), students (for benefiting indirectly from via curriculum enhancements), university administrators (for evaluating research), government (for funding and consuming research, and making policy decisions), and the society at large (Keen 1991). Other potential stakeholders may include system designers, social scientists, computer scientists, and so forth, but their involvement in and consumption of mainstream IS research is fairly minimal. Further, the IS research community is an eclectic collection of academics with backgrounds as diverse as computer science, economics, psychology, sociology, and political science (among other referent disciplines), and each sub-community bring its own unique perspective to bear on the relevance issue.

The "pertinent how" issue is related to the subject matter under investigation. IS researchers are primarily concerned with theory and knowledge building (in areas such as IS impacts, IS implementation/use, and interaction with organizational elements), IS and business managers are mostly interested in instruments, checklists, or findings that can be readily implemented (e.g., cost justifying IS, IS implementation strategies), university administrators are interested in quantifiable measures of research productivity (for promotion and tenure decisions), students are interested in direct contributions to IS curriculums (e.g., electronic commerce courses), while policy makers are interested in the social and business impacts of IS (for formulating appropriate policy decisions). Given this diversity of stakeholder groups and their divergent perspectives ("lens") of IS research, it is not surprising that very little IS research is *simultaneously* relevant to all constituents. Consequently, IS research seems to suffer from a "credibility gap" within the business community, and is often decried as being "fuzzy, irrelevant, and pretentious" (Benbasat and Zmud 1999).

The pluralistic conception of IS research relevance may be partly attributed to the relatively young and dynamic nature of the IS discipline. Unlike other business disciplines, IS does not have a historically entrenched research tradition, but "emerged" about 30 years ago in response to industry needs for infusing computer technology and talent as a means of enhancing worker productivity and competitive advantage. Tied to changes in the dynamic IS industry, IS research topics also changed continually from in-house system development to packaged software implementation, from software insourcing to outsourcing, from operational impacts of IS to strategic impacts, and so forth. Academicians trained in the tools and methodologies of "rigorous" research often "fell behind the curve," rendering their research outdated or irrelevant in the eyes of practitioners (in contrast to consultants, who supposedly perform less rigorous but more timely research), though such research may be perfectly relevant from the perspective of other academicians. Further, continuous changes in focus hindered the stakeholder groups from developing a unifying paradigm of IS research or a "shared understanding" of what would constitute relevant IS research. Given the pluralistic and dynamic nature of IS research, it seems unlikely that such a shared vision will be accomplished in the near future.

III. EVALUATING IS RESEARCH RELEVANCE

Since relevance is difficult to accomplish in IS research, two follow-up questions are whether IS academics should abandon the pursuit of relevance in their research and if not, how to evaluate the relevance of others' works. Academicians, IS or otherwise, have a two-fold role in society: creating knowledge (via research) and disseminating knowledge (via teaching). The experiences and outcomes of research are channeled into journal articles and books contributing to a cumulative body of knowledge, which not only stimulate further research but also prepare students to face the challenges of the industry. Not all IS research is equally relevant for teaching purposes, and some relevant teaching content is acquired from industry rather than from academic research (since the industry often leads academia in this area). Relevance helps define what is "valid knowledge" (from both research and teaching perspectives), creates a common body of knowledge, develops a coherent identity for the discipline as a whole, and hence should not be abandoned in IS research.

However, judging the relevance of IS research is particularly difficult given its differing perceptions across various stakeholder groups. Any assessment of relevance should take into account the goals of that research and the audience it is intended for. For example, a research experiment exploring the effects of group support system (GSS) on individual performance is best judged by other academicians familiar with the GSS domain and experimental methodology. Given the nature of this research, assessment of relevance should be based on the study's epistemological contribution to knowledge building in GSS and IS as a whole. Likewise, research aimed at designing an organizational solution for a specific business problem can be best judged by practitioners, based on the efficacy of the proposed solution in solving the focal problem. Similarly, research aimed at improving IS education should be evaluated by other educators in terms of its value-add compared to other educational innovations. Without formal training or

understanding of IS research domains and methods, it is as difficult for practitioners to judge the relevance of a "research paper," as it is unfair to the author of the paper being judged. Likewise, it is problematic for academics to accurately judge the relevance of practitioner-oriented projects, if judges are not well versed in the domain of that research. The pluralistic nature of the IS discipline suggests pluralistic criteria in evaluating IS research relevance. It is important to employ the right yardstick to assess the relevance of IS research initiatives.

Selecting the right yardstick is also a critical issue in disciplines that are less pluralistic than IS (e.g., physics, economics, medicine). Most of these disciplines distinguish between two types of research: basic and applied. The goal of basic research is to identify laws and theories for better explaining observed phenomena (i.e., knowledge creation), while that of applied research is to apply these laws and theories for solving real-world problems. Applied research is typically more successful (than basic research) in attracting corporate funding by virtue of its appeal for real-world problems, and its relevance can potentially be judged by business practitioners. On the other hand, basic research, which provides the foundation upon which applied research is based but may be several years or decades from delivering any tangible business value, is best judged by other academics involved in similar knowledge building efforts. Using the expectations of business professionals to evaluate the relevance of basic academic research is hence likely to be as frustrating as using a hammer to plant a tree or using a shovel to drive a nail in a wall.

Of course, IS research that addresses the concerns of multiple stakeholder groups will have broader relevance among a wider community of IS academicians and practitioners, and is certainly the preferred way of conducting research. In particular, the Society of Information Management (SIM) Advanced Practices Council routinely sponsors (applied) academic IS research that address practitioners' concerns. While such initiatives can certainly go a long way in bridging the chasm between IS academics and practitioners, one must also recognize that basic IS research may be relevant to a different segment of the IS population without directly serving the interests of any particular stakeholder group.

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