

RELEVANCE VERSUS RIGOR IN IS RESEARCH - A REVIEW OF MAJOR ARGUMENTS AND DIRECTIONS FOR THE FUTURE

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ABSTRACT

A synthesis of major arguments related to the debate on rigor versus relevance in the IS field is presented. The debate within the IS community about which of the two is more valuable has been raging for a long time. IS field investigates issues related to a varied and fast changing technology and therefore finds itself at the center of influences that try to pull it in multiple directions. This paper provides a summary of the major arguments and provides commentary on how they are likely to shape the nature of the debate and development of IS field.

INTRODUCTION

The issue of relevance of a theory is at the core of its existence. Dubin (1969) states that the theories of social and human behavior address themselves to two distinct goals of science: a) prediction, and b) understanding. By prediction, Dubin means that we can foretell the value of one or more units making up the system, or that we can anticipate the condition or state of a system as a whole. In both instances, the focus of attention is upon outcome. By understanding Dubin implies the knowledge about the interaction of units in a system. Since the ultimate objective of a theory is to enhance the knowledge about the environment it is embedded in, its relevance can be described as the extent to which it can help predict and understand the phenomenon to those who apply it in a given context. Thomas and Tymon (1982) offer five criteria of relevance or practical usefulness which are: a) meaningfulness, goal relevance, operational validity, innovativeness, and cost of implementation. According to Shrivastava (1987), the standards for judging the usefulness of research depend on the needs of the users, and the ways in which the research results are used. In the context of the Information Sciences, much of the research being conducted today is concerned with the ongoing relations among information technology, individuals, and organizations (Orlikowski and Baroudi, 1991). However, the alignment between the actual practitioner requirements and IS research agenda has been less than expected (Rosemann & Vessey 2008). They further state that without relevance to the practicing community, the very existence of a research discipline such as IS could be questioned.

According to Zmud (1996) the studies examining IS research issues are being driven not by the concerns of practice but instead the institution represented by the IS research literature. Zmud further states that many of the constructs researched by IS investigators fail to resonate among practitioners due to their marginal relevance to practitioners' core concerns. This state of affairs

is neither new nor unique to IS. Other fields in business studies have struggled with the same issues in the past (Thomas and Tymon, 1982, Gordon, 1976).

This paper presents a review of the raging debate on rigor versus relevance in the IS field that took place some years ago. The conclusion of this paper is that while some researchers such as Robey and Markus (1998) emphasize that it is possible for IS discipline to achieve both rigor and relevance, it might be more likely that at the current juncture, the IS field needs to pay extra attention to the relevance aspects in its research. Having said that, it still bears to reason that without rigor, the IS field could get bogged down in the practicality issues and any field that shifts its gaze away from future, stands to lose in terms of its match toward refining and developing its theoretical intellect. Most of the arguments in the debate appear eager to promote the cause of relevance in IS discipline, however Lee (1999) raises much deeper issue of the myopic positivist slant in today's IS research, an issue that could come to haunt it in not too distant a future.

THE PRACTICE OF RELEVANCE

The debate was originally started by Benbasat & Zmud (1999) who argued that IS research lacks relevance to practice and suggested tactics, procedures and guidelines that the IS academic community might follow in their research efforts and articles to introduce relevance to practitioners. Citing discouraging feedback from practitioner community regarding actual usefulness of IS research publications to their objectives, authors explain why one tends to observe a lack of relevance to practice in IS research today, and offer guidelines that IS academic community should follow to introduce greater relevance in their research effort.

Attributes of Relevance

Discussing the issue of relevance itself, Benbasat and Zmud argue that the choice of topics addressed by IS researchers is influential in determining its relevance in the eyes of the practitioners. Articles targeted at enduring or current organizational problems, challenges and dilemmas as well as articles that address timely business issues tend to be well received by practitioners. In addition, the article's implications should also be implementable. They further state that apart from the above categories, two other types of research also tends to be favored by the practitioners, a) research that synthesize an existing body of research, i.e. which classify, categorize, and summarize major themes and findings and; b) research that stimulates critical thinking by challenging the reader's causal assumptions or by identifying emerging trends, structural changes, or paradigms. The former allows the practitioners to conveniently assimilate the existing body of research and the latter contributes to enhancing or restructuring the mental models managers apply to their practice. In addition to the content, style and tone are important issues in determining practical relevance of research. IS practitioners prefer shorter articles that use exhibits liberally, avoid turgid prose, focus more on contextual description and less on literature review and methodology description, and appreciate prescriptive advice. Regarding rigor, the managers appreciate efforts to use appropriate methodology by researchers but consider unnecessarily long descriptions of methodology a negative aspect of research.

Reasons behind lack of relevance in IS research

Benbasat & Zmud offer five reasons they believe are contributing to the lack of relevance in current IS research. These are:

a) Emphasis on rigor over practice relevance:

In order to gain legitimacy as an academic discipline of substance and standing, IS researchers and the editors of top IS academic journals have tended to emphasize rigor over relevance in their journals. Due to perceived weaknesses in IS research in the past, IS academics have since endeavored to achieve compatibility with other well established research disciplines in their effort to remove lacunas in IS research. In the process, relevance was relegated to secondary importance.

b) Lack of cumulative research tradition:

The inability of IS researchers to engage in developing a tradition of cumulative research has caused lack of strong theoretical models that can offer confident prescriptive advice. The authors state that part of the reason for this state of affairs is because multiple frames already exist in referent disciplines for IS researchers to apply to their own research. Also, the inherent research philosophy of IS researchers has been to invent than to adopt what already exists, therefore the importance of well-defined collection of research constructs and instruments has been undervalued. Also, there has been a significant proliferation of IS journals leading to lack of supporting research for previous work.

c) Rapid and continuous rate of change of information technology

The dynamism of the technological environment which inspires research in IS fields also a) adds much complexity and uncertainty to the research efforts, b) results in chasing after practice rather than leading practice, and c) typically leads to reporting results from rigorous research studies years after the technology is accepted or rejected by practitioners.

d) Limited exposure of IS researchers to business and technological context in which IS phenomena transpire

IS researchers have insufficient exposure to the environments within which IT usage and behavior actually takes place. Due to the constraints on their time from academic responsibilities as well as the nature of the contact being limited to recruiting and consulting, the academics lack the rich interaction necessary for developing a framework of research that can keep up with the latest trends in information technology development.

e) Institutional and environmental constraints inhibiting freedom within the academia.

The authors state that the patronage and power structures within academic institutions wield significant influence on the degree of freedom academics have in pursuing relevance. The nature of the academic patronage system deters deviation from the defined research agendas of academic institutions. These agendas focus more on rigorous research than on its relevance to practitioners. To redress this situation, financial influence of corporate entities on setting research agendas needs to be encouraged.

Suggestions for increasing relevance of IS research

Benbasat & Zmud offer nine recommendations to both researcher and journal editors in order to enhance the relevance of IS research. These are:

Recommendation 1 – The foremost criterion to be applied in selecting research topics should be directly related to the importance that key stakeholders: journals, colleagues, and particularly practitioners are likely to place on that topic. The authors need to pay extra attention to well known areas of interest to practitioners, secondly IS community need to make areas of practitioner interest central to any debate agenda within the IS community.

Recommendation 2 - IS researchers should look to practice to identify research topics and look to the IS literature only after a commitment has been made to a specific role. Researchers need to interact with practitioners more closely and identify areas for research that would be of interest to them a few years hence.

Recommendation 3 - More discussion on the core research issues in the IS field that have the potential to influence practitioners is needed. Members of the IS research community must define important phenomenon related to the various segments of the IS discipline, and editors of the leading IS journals must work with authors to ensure that such information is widely disseminated within the IS community.

Recommendation 4 – When deciding whether or not to begin a new research project, IS researchers should focus on the likely outcomes, rather than the inputs, of such efforts.

Recommendation 5 – In order for IS researchers to be more proactive in a direct sense, it is imperative that the IS research community produce a cumulative, theory-based, context-rich bodies of research. Giving example of the theory of planned behavior, (TPB), authors contend this theory is rich enough to be relevant to practitioners as it is applicable to organizational performance context, it offers predictions, it contains variables that can be manipulated by practitioners, is applicable to a variety of contexts.

Recommendation 6 – In order for IS research to be more relevant, it is important that authors develop frames of reference which are intuitively meaningful to practitioners to organize complex phenomenon and to provide contingency based approaches to action.

Recommendation 7 – In order for IS research to become more relevant, IS academics should portray the outputs of their research in ways such that it might be utilized by practitioners to justify and rationalize IT related initiatives.

Recommendation 8 – The vast majority of IS research articles should be crafted in a clear, simple, and concise manner such that they are accessible to the entirety of the potential readership of a journal

Recommendation 9 – Editors and editorial boards of all IS journals need to critically examine their current postures, reviewing procedures, and editorial decisions concerning the balance between rigor and relevance with the goal of publishing manuscripts that are characterized by both.

Authors conclude that the world of practice has much to offer the IS academic researcher, practice provides strong signals regarding what we should be studying as they validate the emerging innovations. Authors urge that academics and the practitioners engage in a dialogue about a)

critical success factors, b) important challenges, c) important questions practitioners have been unable to find answers for and d) the issues that will be important to the practitioners three to five years from present.

Review of Benbasat and Zmud

Benbasat and Zmud indeed raised very important issue of relevance in IS research, however their recommendation that positivist research methodology is the only appropriate channel for IS research is likely to face resistance from their colleagues in the field. Orlikowski and Baroudi (1991), state that much information systems research reflects a positivist orientation, a research tradition that has its roots in natural sciences. They caution against exclusive reliance on a single view, as it is at best a partial view, and also that the dominance of positivism has limited what aspects of information systems we have studied. They suggest inclusion of interpretive research philosophy and critical research philosophy in the IS research to broaden the perspective and to understand the implications of IS research from a much broader viewpoint. Orlikowski and Baroudi, and other researchers are likely to argue to bring to prominence other dimensions of IS research that have so far been overlooked by the discipline. The inclusion of diverse perspectives of research philosophies could help enhance the relevance of IS research as it will uncover hitherto hidden dimensions of issues influencing information system development and use.

The following arguments by Davenport and Markus supports Benbasat and Zmud on their reliance on positivist side of IS research.

THE CRITICAL IMPORTANCE OF RELEVANCE

Davenport and Markus, (1999), in their response to Benbasat and Zmud (1999) state that Benbasat and Zmud did not go far enough in their recommendations for emphasizing relevance in IS research. They strongly advocate making relevance an essential part of IS research which it is lacking. They recommend emulating researchers in medicine and law instead of those in other business school fields. They lament that IS academics experience the same institutional pressures towards irrelevance as other business faculty, e.g. promotion and tenure evaluations based on publications in refereed academic journals but not in practitioner journals. The authors state that the IS field differs from other management fields in that it is much more dynamic. They point out that the differences of our field require us to take a different approach to research. Emulating other management fields is not appropriate for IS, its redemption lies in conducting more relevant and timely research than other management fields.

Davenport and Markus, while concurring with Benbasat and Zmud about the need for excellence in scholarship, state that it is not a zero sum game with respect to relevance. They point towards alternative research models such as applied theory, evaluation research and policy research which offer avenues for both rigor and relevance to their adherents. The authors caution that these research models are considered extraneous by mainstream IS research outlets as they do not fit current definitions of acceptable IS research. The authors champion the inclusion of practitioner favored journals such as the Sloan Management Review and Harvard Business Review in mainstream research publication and by including them in tenure and promotion evaluations. The

research evaluation framework should be modified to include practitioner related research in judging researcher productivity.

In calling for a reassessment of self-evaluation by IS academics regarding their position vis a vis consultants, Davenport and Markus raise concerns about perceptions of leadership assumed by the academia as it may not be a true account of reality. They highlight the strong following of consultant research in terms of its implementability for practitioners. Asking for the inclusion of students also in the constituency of those the IS academics need to influence, the authors stress the importance of reducing the current dichotomy in school syllabi content and IS research as the former is heavily based on consultant research.

In asking for a complete re-conceptualization of the existing IS research framework, Davenport and Markus point out that journal reviewing and promotion and tenure reviews are important mechanisms for maintaining irrelevance. The IS academia needs to alter the criteria it uses to assess research for publication and career progress. The authors reiterate their arguments for making IS research more relevant to practitioners as it will influence the respect awarded to IS academia and the long term access to essential resources, which originates in the practitioner community. The authors suggest that a target portfolio for the IS field's cumulative research output with high science, applied theory and practical research in equal proportions and stress that all IS academics need to value relevant research.

Review of Davenport and Markus

Davenport and Markus raise relevant points, however their analogy of law and medicine journals as role models for IS research journals is comparing oranges with apples. The contributors to law and medicine journals are intensive practitioners of that profession and have usually first hand experience of problem solving in the complex reality of real world. Most of the contributors to IS research journals have on the other hand usually possess at best second hand knowledge of real world problems and most of them have no IS industry experience at all. The role model for IS research journals has to be basic science journals where researchers grapple with the development of generalizable theories and abstract phenomenon in the domain of information technology. The need to interact more with the practitioners to get a better grasp of the complexities of real world issues. The relevance of IS research will be guided how close the interaction is between the researchers in academia and the practitioners in the field in terms of exchange of knowledge and experience.

THE IMPORTANCE TO CAREER

Applegate and King (1999), discuss a hypothetical but entirely plausible situation confronting the researchers conducting research in the IS field which is posing challenging questions about the very essence of the field's direction and basis of existence itself. Researchers in the field, interested in conducting relevant research, are facing pressures from the establishment including senior faculty and research advisors to avoid tackling questions that are complex and realistic as it is inordinately difficult to execute such research projects. Instead they are being guided in the direction of doable research, which follows the well treaded path of strong theoretical constructs and proven theory. The fact that such research is often banal and trite is assumed to be a risk worth

bearing for the sake of conducting research in the most convenient manner. However, the top research journals expecting new and current problems often deny publishing such research which is detrimental to the researcher's career prospects. This contradiction in the objectives of the research guides in academia and journal editors can seriously undermine the career prospects of unwary young researchers who are denied tenures and stable career choices. The fact that department research guides are indispensable to a young researcher and often hold much more influence over the researchers in training leads one to the conclusion that the onus for the solution to this problem also lies with them. Young researchers need to be appraised of the critical balancing act required to be a "successful researcher" and a "successful published researcher". The selection of problems for conducting research need to take into account the demands of feasible research and the top journals requirements to move the field further by publishing only cutting edge research. These two perspectives require researchers needing publications for career advancement purposes to not be swayed by either and think long and hard before embarking on a research project.

Review of Applgate and King

The issue of relevant practical research not being permitted by agenda setters in the academia is one of the contributing factors inhibiting the development of relevant research in IS field. The advice given to graduate students, young faculty to focus on doable issues runs counter to the objective of conducting relevant research. Researchers are often hindered by the inability to gain an opportunity to conduct research on issues close to the requirements of the industry due to the lack of existing theoretical concepts in that area whereas the research journals would only publish apparently cutting edge issues. This points to the need for a fresh look at widening the base of well-established constructs to new and emerging technologies. Established researchers need to guide new researchers on issues related to building sound theoretical constructs pertaining to new technologies.

CENTRALITY OF EMPIRICAL RESEARCH

Lyytinen (1999), a researcher at the university of Jyvaskyla in Finland, brings European perspective to this debate. Lyytinen questions the basic terminology adopted by Benbasat and Zmud such as the concept of relevance. Arguing that easy digestibility of research by CEO's in one afternoon may not be the right approach to constructing the meaning of relevance, as relevance is closely intertwined with how researchers perceive practice and how that practice could be influenced. The author states that the lack of well established IS institutions hinders development of good relations with industry in order to fully understand and shape practice. The current institutional framework of rigor, scientific expertise and tenure policies often leads to opportunistic research behaviors that tend to ignore practice. The author also questions the heavy emphasis on empirical research commonly followed in North America as being part of the problem. According to him, quite often researchers are looking for problems for which they might obtain an easy solution in terms of well-established methodology. Lyytinen suggests that the overwhelming focus on empirical research in North America prohibits work on such cutting edge problems such as installing new systems or visioning new models of technology use. Developmental research that involves both constructive and empirical elements is largely missing because of preponderance of empirical methodology adopted by most researchers. Terming the argument of Benbasat and

Zmud that rigor is inevitable in absence of conscious effort to prevent it, the author states that on the contrary, relevance will replace rigor if not prevented. Comparing the situation in North America with that prevailing in Europe, the author emphasizes deeper level of reading prescribed to students there and a lack of desire to simplify the language in order to make it more palatable to everyone.

Review of Lytinen

Lyytinen raises the issue of overdependence on empirical research in North America compared to the rationalist, dialectical investigations done by European researchers. Lyytinen states that empirical research leads the researchers to look for problems for which there already exists a solution in terms of established methodology. While this criticism of research practices in North America have some validity, it is not clear if the European rationalistic debate always produces better results than empirical research method. In any case, researcher need to stop looking at issues from the point of doability and include rationalistic thought process to problems that might help with finding solutions for researchers and practitioners. Exploring the not so well trodden avenues of research will help shed light from perspectives that might not have received enough focus in IS research.

BEYOND POSITIVISM

Lee (1999), while agreeing with Benbasat and Zmud (1999), deconstructs their arguments in terms a) their self-avowed positivist orientation, b) the instrumental model used to define relevancy of research, and c) need for the IS research community to take a broad approach to the matter of relevance. Lee states that Benbasat and Zmud did not go far enough in the sense that they gravitated towards the positivism, which is the epistemology of research followed by the natural sciences. Lee also makes the distinction between natural sciences and professions in that law and medicine are professions not natural sciences as claimed by Davenport and Markus (1999). According to Lee, inquiry in natural sciences follows the goal of truth in formal propositions, inquiry in the professions pursues the goal of effectiveness in actions. Inquiry in the natural sciences produces the knowledge about what the world is, enquiry in professions produces knowledge about how to intervene in the world and change it in order to satisfy real-world needs. Lee claims that IS, if it wants its research to be relevant to practitioners, ought to emulate the manner of enquiry adopted by professions. Author explains that the approach inherent in natural sciences research is essentially theory driven, i.e. driven by the needs to refine theory so that it predicts more accurately, rather than practice driven, i.e. driven by needs to resolve real-world problems. Adding that natural science approach need not be the only approach followed by IS researchers, it needs to be combined with the approach followed by professions.

Regarding ramifications of instrumentalism for relevance, Lee defines it as including the following elements. A researcher formulates, tests, and validates a theory that specifies independent variables, dependent variables, and the relationships among them. In doing so, the researcher is careful to make sure that, first, the dependent variables represent the outcomes that the practitioner is interested in achieving, and, second, the independent variables represent factors that not only indeed influence the outcomes but also can be manipulated or changed by the practitioner. A practitioner could then apply the theory by manipulating the independent variables in order to

achieve the desired levels in the dependent variables. This assumes that the practitioner's problem was clearly definable in the first place. Lee cautions that the instrumental model of practice does not apply in all situations. Lee also explains that practitioner's problem may not always be clearly definable as the practitioner's task environment is murky, and the variables are not even known.

Review of Lee

Lee (1999) states that the positivist philosophy of research inherent in empirical research is fraught with complications. He states that the difficulty of capturing social reality in formal propositions, quantifying it and subjecting it to experimental controls are the reasons as to why organizational research has not reached the level of scientific maturity that characterizes natural sciences. Advocating the claims of interpretivist methodology, Lee (1991) states that because the world of intersubjectively created meanings has no counterpart in physical sciences, the methods of natural science are at best inadequate to social science. Lee's arguments go to the core of IS research because it questions the nature of constructs investigated by IS researchers. It raises questions on the way IS researchers capture reality in their constructs, a process that might need to go back to the drawing board.

CONCLUSION

Despite the fact the debate on rigor versus relevance began many years ago, the solution to this vexing issue is still elusive in IS research. The debate on the critical issue of rigor versus relevance raises important questions regarding the nature of research conducted by IS researchers. The fact that practitioners do not look upon IS research as being implementable is indeed a weakness of the IS field. The issue will probably need a much deeper investigation and soul searching regarding the kind of research being conducted in IS field, the way it interprets reality in its research constructs, the way it classifies problems worthy of investigation, and the heavy emphasis on doable empirical research. The field might need to divide itself on the lines of theoretical and practitioner. Also tenure and promotion committees might look at allowing researchers to include practitioner research in their portfolio.

References are available upon request from Sameer Dutta