

Working Paper

What is a Good Piece of Information Systems Research By

Morten Thanning Vendelø

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Institut for Informatik

Handelshøjskolen
i København

Howitzvej 60
2000 Frederiksberg

Tlf.: 3815 2400
Fax: 3815 2401
<http://www.inf.cbs.dk>

Department of Informatics

Copenhagen
Business School

Howitzvej 60
DK-2000 Frederiksberg
Denmark

Tel.: +45 3815 2400
Fax: +45 3815 2401
<http://www.inf.cbs.dk>

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Morten Thanning Vendelø
Copenhagen Business School
Department of Informatics
Howitzvej 60
DK-2000 Frederiksberg
Denmark
Phone: +45 38 15 24 08
Fax: +45 38 15 24 01
e-mail: mtv@cbs.dk
<http://www.cbs.dk/staff/mtv/>

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Morten Thanning Vendelø

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I. Introduction

From time to time I speculate: What is a good piece of information systems research? As several people will know I am displeased with a good deal of the IS research I see when reviewing for and attending IS conferences. I find that it lacks rigor and progression, and tend to present new words, for example Business Process Reengineering, for such phenomena as IT-induced organizational change. In my own experience my dissatisfaction with much IS research originates from the fact that the field has either no or very weak research paradigms / programs. Hence, for IS researchers the field itself provides no or very weak possibilities for scientific navigation¹. Likewise, poor quality may come about if researchers rush with publication of their work, having in mind its contribution to their personal reputation. Ergo, a lot of publications may have more to do with careers and egos than with scientific advancement of our research field. Finally, I assert that the IS research field is more likely to create and pick-up management fads and fashions (Abrahamson, 1991; Scarbrough and Swan, 2001), as we have a disposition for viewing each new application of the information technology as creating a "whole new world".

All this of course makes me wonder whether I am just an outlier with some odd observations, or if there is in fact some truth to my observations. When going through some recent literature dealing with these issues I find that my concerns do not deviate from those of other observers. For example, when commenting on the results from a survey of our academic field, Avgerou et al. (1999, p. 136) note:

"- the academic field of IS remains ill-defined, often facing problems of recognition and legitimacy."

And they continue (Avgerou et al., 1999, p. 137):

"The emerging picture shows a field that is institutionally dispersed, cognitively diverse and methodologically pluralistic. We argue that the institutional dispersion is a weakness that requires remedying action."

Finally, Avgerou et al. (1999, p. 138) note:

"..., a variety of weaknesses were listed such as the lack of clear identity, recognition by other disciplines, theoretical foundation, standard terminology, and resources."

Ergo, other people in the information systems research community experience problems similar to those observed by me. Therefore, my next thought was; what can we do in our small local communities to remedy these problems?

II. What Do We Do Ourselves?

I think that one central issue that we have to deal with is whether we attempt to *study phenomena of interest* or if we perceive ourselves as being in the process of *building a research field*. Using the following definition of phenomena of interest:

"Examples of phenomena are dogs, windmills and organizations. These are properties of reality captured by concepts. The sort of phenomena we consider rely on a concept-correspondence which is relatively time-invariant because it is founded in history. This allows us to assert that different properties of the same phenomenon may be of interest from different perspectives. Some may be interested in studying friendship between dogs and people whereas others may study if dogs are cost-efficient alternatives to electric fences.

We understand phenomena as events or states of affairs that exist independent of the concepts that allow us to realize their existence. This premise allows us to define research fields in terms of phenomena. Even contrasting theoretical views acknowledge the existence of a certain phenomenon, for example, most people agree when the phenomenon dog has been encountered." (Knudsen and Vendelø, 2000, p. 3)

Concerning a research field I adopt the following definition:

"By defining a research field by its appeal to the phenomenon studied we assert that the study of social organizations comprises a research field which may, and in fact does, encompass several paradigms. This concurs with the understanding held by other students of organization (for example, Morgan, 1986; Pfeffer, 1997; Perrow, 1986; Scott, 1998). Likewise, different subject matters are studied within the same paradigm.

Paradigms are presumptions agreed upon in a research community and they serve to structure research, that is, a certain ontology and epistemology. Hence, we understand paradigms as properties shared by participants in research communities." (Knudsen and Vendelø, 2000, p. 6)

I experience that in many situations IS-researchers focus their research on the study of phenomena of interest, not really considering how their efforts could contribute to the advancement of science.

II.i Studying Phenomena of Interest

If we study phenomena of interest then it is likely that these phenomena have been observed by others, and thus, have been described and analyzed by other research fields. For example, when studying implementation of information technology or IT-induced organizational change, it might be of high value to look elsewhere and see what others have found when studying similar phenomena. In fact, as noted by Avgerou et al. (1999, p. 136):

"The emerging picture suggests that the study of IS in European academic is dispersed in small units with various names, which are hosted in various disciplines across the science/social science spectrum."

This might in part be explained by the fact that we fail to recognize that many of the phenomena, although it may be in disguise, encountered by IS researchers have already been observed and studied within other research fields, and thus, we seldom face new

phenomena. As noted by Sørensen (1994) there might not be as many "great ideas" as we tend to think. Instead, we might learn that a lot has already been said about these phenomena, and that we might spend our time more efficient by building on that work. Hence, carefully building on existing work becomes something that we must devote attention to. Sørensen (1994) also reflects this when he notes:

"One of the most important aspects of research is adding and relating to an existing body of knowledge."

"One of the purposes of relating your work to others work is the point of accumulating research so that "new" inventions are not invented again and again."

However, I think this not true for a lot of IS research. For example, what do we really know about Business Process Reengineering apart from case studies describing both success and failures of such processes? Well, sometimes they also define critical success factors for such processes. Now this topic is more or less gone, being a good example of management fads and fashions (Abrahamson, 1991; Eccles and Nohria, 1992; Scarbrough and Swan, 2001). Thus, we might ask why there is such a failure to acknowledge Business Process Reengineering as but one example of organizational change according to the rational model (Borum, 1995). I think that somehow the same is true for e-commerce. Right now we seem to be occupied with counting the number of transactions on the net, comparing these with the number of traditional transactions, and so on. In my opinion there seems to be many more interesting questions to work on, for example we may try to understand how e-commerce changes market structures and how markets function, or how e-commerce changes search costs for customers. In both cases questions dealt with in economics. Hence, there is good reason for us to allow our IS research to be informed by knowledge gained in fields where the phenomenon of interest have been studied previously.

II.ii Building a Research Field

If we replace *phenomena of interest* with *building a research field* then we face the challenge of demonstrating that the phenomena analyzed is truly new, and thus, constitute a basis for a new field of research. Taking for example the Capability

Maturity Model (CMM), which have been quite popular in attempt to improve processes of software development. I think that in understanding if, why and to what extend implementation of this model improves organizational efficiency it is central to understand how processes of bureaucratization brings about organizational efficiency, rather than measuring the improvements attained in various projects.

If we want to build new research fields, we have to carefully choose phenomena of interest about which we can say that they are not covered sufficiently by other research fields. As the fact that information technology is involved does in my opinion not provide a reason d'etre for establishing a new research field. Rather we must show that new paradigms or at least new theories are needed to understand information technology in interaction with individuals, organizations and society. Yet, rather than building new theories and paradigms IS research tends to borrow theories and concepts from other research fields, for example, economics, organization studies, sociology, and psychology. Such borrowing of theory from other disciplines is of course needed in the formative years of a new research field. But oftentimes we do not witness skillful theory import. Referring to transaction cost theory by quoting Williamson (1975) is not sufficient, as there are in fact several different versions of transaction cost theory². Hence, I believe it is important for IS researchers to dig deeper into the theories they want to borrow from, meaning that they should read the original sources before they write their own stuff.

I do not perceive this as a call for an epistemological consensus in the IS research field, but for improved quality in scholarship, which I believe can be achieved through research informed by theory. I guess this concurs with the perception held by James G. March who in his presentation to the European Group of Organization Studies Colloquium in Helsinki, in July 2000 said that “a key to understand why focusing on epistemological purity³ is a mistake lies in the speculation that intelligence is more a function of the quality of thinking within a framework than it is of the choice of framework. As a result we will almost always learn more from superb scholars who profess quite different disciplinary or epistemological convictions than from scholars who share our prejudices but execute them less well.” Hence, my call is for more solid scholarly work in the IS research field.

III. Closure

Now back to my initial question: What is a good piece of IS research? Answering this with the above discussion in mind I think that a good piece of IS research must at least bring advancement to the field, for example by providing more elaborate answers to existing research questions, or by identifying truly new phenomena of interest. As an example of good IS research I count the research on de-escalation of commitment in IT-projects undertaken by Keil and collaborators (see for example, Keil, 1995; Keil and Robey, 1999, Montealegre and Keil, 2000), who have done a very good job in developing the research agenda of the psychological phenomenon of escalating commitment by bringing it into the IS research field. Other examples of good IS research would be the research on power in IT-implementation performed by Markus and collaborators in the 1980s (see for example, Markus, 1983; Markus and Bjørn-Andersen, 1987; Markus and Pfeffer, 1983), as well as the research in socio-technical information systems development methodologies by Avison, Wood-Harper and others (see for example, Avison and Wood-Harper, 1990; Avison et al., 2004).

I hope this outlining of some of my ideas concerning what could constitute good IS research will stimulate others to think about what they believe is good IS research. One way of doing so might be by picking an article that you read recently and consider to be a good piece of IS research. Go through the article and try to figure out why you think it is "good." Inspiration for this might be found among my words, but more likely you may come up with some different criteria, either because of your prior experience, or because the phenomena of interest to you differs from mine.

IV. A Small Afterthought

As an afterthought I will like to state that it is my hope that I overestimate the problems facing the young field of IS research. Recently when I studied the evolution of the field of organization studies, it occurred to me that may be the IS research field is undergoing a development similar to the field of organizations studies, and that the paradigmatic problems currently facing the IS research field are quite similar to those faced by the field of organization studies half a century ago. In the fifties when that field was still in its formative years many people had difficulties in seeing it developing into a field of its own. Kenneth Boulding (1958) for example, "argued that although the field of organization studies from the beginning was trying to separate itself from the parent disciplines (sociology, economics, psychology, and political

science), it was fundamentally dependent on developments in the disciplines themselves for a successful take-off" (cited from Augier et al. 2003, p. 24). In a similar vein Helmer (1958, p. 172) noted: "There has been a lot of talk in the last decade or two about organization theory as the up-and-coming thing. Yet the trouble with organization theory to date is its continued non-existence. This is true despite the fact that numerous sporadic efforts in this general area have succeeded in providing a variety of insights into the mechanism of all kinds of organizations" (Augier et al., 2003, p. 24).

As of today we know that these prophecies about the future of organization studies did not come true. A recent analysis of the field of organization studies by Augier et al. (2003) "... confirms a picture of the gradual creation of a knowledge domain, a knowledge community, and a scholarly identity. This process is revealed by two conspicuous features: First, the field of organization studies has constructed a history itself, a set of connected stories. Each wave of new references has faced a long, and relatively harsh, filtering process over time that has produced a few durable ancestors. As older references have achieved standing that assigns them to an ancestor role, the overall reference list has aged. The field has come to exhibit a sense of a somewhat shared intellectual history, a history that has been constructed with a first generation that is pictured as more or less immaculately conceived in the first two decades after 1945.

Second, the domain of organization studies has increasingly differentiated itself from other fields and from the social science disciplines. Although citations in books and articles dealing with organizations are to a very large number of journals, the concentration of citations has increased over time as the field has come to identify a few primary outlets. This increased concentration has occurred in parallel with an increased differentiation from the journals in the major social science disciplines. There has been a substantial increase in references to organizations journals and a substantial decrease in references to disciplinary journals, especially disciplinary journals in anthropology, political science and psychology" (pp. 24-25).

The fact that the field of organization studies survived with success, however, does not allow for a laid back attitude towards the maturation of the IS research field. Transforming an ill-defined and institutionally dispersed research field into proper paradigmatic field of scholarship takes effort and dedication from scholars in the field. Addressing the European Group of Organization Studies Colloquium in Helsinki on

this issue James G. March suggested that such a “field of scholarship is distinguished by a set of agreed upon fundamentals, shared understandings of critical unresolved questions, agreement on standards for the formulation and evaluation of assertions, and a history reflecting the gradual accumulation of accepted knowledge, organization studies as a whole fails to with respect to all criteria, though there are sub-fields of organizational scholarship that come close.” Consequently, objectives can be set for the pursuit of making the IS research field a more accepted research discipline.

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¹ In fact Avgerou et al. (1999, p. 148) note: "..., nevertheless many are concerned that the field lacks theory and is fragmented."

² See Langlois and Robertson (1995) For a brief overview of the various branches of transaction cost theory.

³ According to Pfeffer (1993) such purity offers advantages that translate into individuals and discipline reputations, resources, equity, clarity of standards, ease of collaboration, and status.