

**RELEVANCE VERSUS RIGOR IN
INFORMATION SYSTEMS RESEARCH:
AN ISSUE OF QUALITY**

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PLENARY PANEL

RELEVANCE VERSUS RIGOR IN INFORMATION SYSTEMS RESEARCH: AN ISSUE OF QUALITY

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BACKGROUND

Much research in information systems, particularly that involving hypothesis testing and laboratory experiment, is seen by some to place too much emphasis on methodology at the expense of what is being investigated. This research perspective is driven by *scientific method* and applied frequently in situations where it may not be appropriate, for example, where values and goals play an important role in understanding process, or determining outcomes. Research approaches based on scientific method (frequently called *functionalism*) are grounded in logical positivism and rest on the following tenants:

1. **The main thrust is a search for causal relationships.**
2. **It is based on empirical evidence.**
3. **There is a strong implication that research is value free.**

In functionalism, the change agent is seen to be an expert located external to the system being studied.

The main argument against the tenants of functionalism is that causal relationships apply only if one accepts determinism (unless factors that influence behaviors are explicitly represented). Causal relationships are in conflict with the notion of free will, where the choices actors make influence outcomes. Additionally, empirical evidence can be misleading: there are realities other than what is apparent, for example, in *facts* taking on different meanings in different situations (e.g., Durrell's *The Alexandrian Quartet*). On the other hand, the absence of data leads to speculation. The issue, then, becomes what data mean; that is, how data are interpreted. Then, too, the undisclosed values of the researcher and the assumptions imbedded in a method form a subtle bias.

Alternative approaches to the functionalist perspective include:

1. **Neo-humanism** - which rests on a concern for human dignity and self-determinism. Examples are socio-technical approaches to system design, such as Mumford's *Ethics* method. Change, from the neo-humanistic perspective, comes from within the work group, with its members acting as change agents.
2. **Social Relativism** - which is based on social consensus, where society is viewed as the change agent.
3. **Radical Structuralism** - where change takes place through radical acts.
4. **Post Positivism** - which uses technology to advance human dignity.

These approaches have in common an emphasis on time, what is salient at each point, as well as concern about motivation, and individual and organizational learning. Case analysis is the primary method used to reconstruct events over time and for deducing values and other states of mind. Ethnographic methods are used to ascribe meaning to behaviors. We refer to these perspectives as *interpretive*.

THE ISSUE

If one scans the major information systems journals,⁶ it becomes apparent that most published research is in a functionalist mold. Given the limitations of this approach, why does it dominate?

Some possible explanations are:

1. Almost all information systems research performed is in the functionalist tradition.
2. The majority of researchers have been trained at universities where functionalist methods predominate. They are not skilled in other perspectives and, in fact, are hardly aware of them. As a result, they use functionalism whether or not it is appropriate.
3. The reviewers and editors of major journals have functionalist backgrounds and are, consequently, biased, either consciously or unconsciously, against other approaches.
4. Interpretive research is difficult to do well. Consequently, although it is being done, interpretivistic research tends to be rejected on quality grounds.

This panel will focus on two themes. The first concerns the extent to which there is bias against non-functionalist research in the information systems community. The second deals with the likely sources of bias in information systems research and, especially, the role of various institutional factors.

THE PLENARY PANEL SESSION

Hans-Erik Nissen: Welcome to the plenary session *Relevance v. Rigor in Information Systems Research*, which will be headed by Jon Turner. It is Jon who originally suggested that we have this session and who has gathered the panelists for it. So please, Jon, begin.

Jon Turner: Thank you very much. I now understand how the gladiators felt!⁷ I hope none of you have taken any hard objects to your seats; you are quite close (laughter). The topic of our panel, to try and complete a circle, is *Relevance v. Rigor in Information Systems Research*, and it ties to the theme of Peter Keen's opening address. We will deal with two issues that have been implicit, if not explicit, in many of the presentations which have been given at the conference. First is the question of bias in information systems research and particularly, whether there is prejudice against non-positivist approaches. We will explore the issue of why information systems research appears to be narrowly focused and frequently not particularly relevant to practice. Second is the role of institutions in forming the perspective for information systems research, particularly the part played by the dissertation, the advisor, and the norms that are transferred in our institutions.

We have a distinguished panel. On my left, and she certainly belongs on the left (laughter), Dr. Tora K. Bikson of The Rand Corporation. Tora has doctorates in behavioral science and philosophy. I feel safe with her on the panel because she protects our flank from the closet philosophers lurking in the audience. On my right, although he really doesn't belong on my right; he belongs further on the left than Tora (laughter) is Professor Kalle Lyytinen of the University of Jyvaskyla. In fact, this whole panel is skewed left with me on the right (laughter); it is certainly not a symmetrical distribution! Next, we have Professor Lars Mathiassen of Aalborg University in Denmark, and then Professor Wanda Orlikowski from MIT in the U.S. Lars and Tora will speak to the first issue, *Is there bias in information systems research?* while Wanda and Kalle will address the second, *The role of institutions in creating research norms.*

Now, just a word about format. First, in keeping with the tradition of disclosing all aspects of our methods, we **are** recording this session. Because we want to leave plenty of time for interaction, the position statements of the panelists will be held strictly to seven minutes. After these opening remarks, I will take questions from the floor. Please be kind enough to identify yourself, by name and institutional affiliation, so that we may

note this in the written transcript. In producing the edited version, I will make every effort to capture the essence of ideas expressed here.

Now to begin, dealing with the bias question in information systems research, Lars and Tora, with Lars going first.

Lars Mathiassen: It is quite a challenge to say something relevant about relevance and rigor in the short time allotted to me. I will start by making a couple of observations. Then I will make the point that the distinction between relevance and rigor is of little help when we want to design and improve the way we work as information systems researchers. Instead, I will suggest a different distinction that I believe is more useful in meeting the quality challenge.

My first observation relates to Peter Keen's keynote speech. He observed that there is a bias towards rigor in the information systems research community and he argued strongly in favor of a change towards relevance. Although I basically agree with his position, Peter Keen overemphasized the point and simplified the challenge we are facing. In Europe, and especially in Scandinavia, we have research traditions that are strongly biased towards relevance without being concerned about rigor. I think Peter Keen made the mistake of overemphasizing the political message to such an extent that it becomes difficult to distinguish ourselves as researchers from other trades, such as, journalists and politicians. Hence, my first observation is that the real issue is *quality of research* and we need a rich picture of the problems involved. The issue is certainly not relevance or rigor, but rather, how to balance the two.

My second observation relates to the meaning of *rigor*. Please note that there is a difference in spelling and semantics between the English and American definitions. English *rigor* is related to *sternness, strictness, strict enforcement (of rules, etc.)*, whereas the American *rigor* provides us with a more narrow meaning of *exact precision or accuracy*. It seems to me that the American *rigor* suggests that we should produce papers, books and texts, in general, that are exact and accurate. In contrast, the English *rigor* is more relative in suggesting that our research should be in strict conformance with rules. This interpretation certainly allows for viewing an anthropological study to be rigorous, in the sense of following the rules for this kind of research game. From this point of view, who can then really be against rigor as the major quality criterion for information systems research?

Now I will turn to my real message. I want to argue that the dialectics between relevance and rigor is of little practical help if you are a Ph.D. student, a Ph.D. advisor, the editor of a journal, or if you are engaged in establishing, or redesigning, a research group. My point is that we all want relevance, but when and how can we know that something is

relevant? Can we recognize and appreciate relevance in the process of doing research? In my experience, relevance can only be decided later, as the result of an interaction between our research results and real world organizations. In contrast, rigor can be evaluated much earlier without relating to the research context. There is, consequently, in this way an unfortunate difference in our ability to measure relevance and rigor. Because of this difference, our community tends to emphasize rigor, instead of relevance, and to stress rigor in striving for quality in research.

I suggest to you, that in dealing with the difficult issue of quality, it is much more practical and challenging to focus on the dialectics between *diversity* and *integrity*. Again, I refer to Peter Keen's presentation, and I strongly agree with him, that we should strive for diversity in our research. Diversity and rigor go hand in hand in establishing a research community that is rich in interpretations, theories and concepts, but at the same time strict in enforcing (and developing) the various rules of the different research traditions. Information systems are complicated to understand and they interact with many facets of organizational life. Our efforts are both to understand information systems as they are and to provide practical advice on how to design them in better ways. A research discipline facing this kind of challenge cannot run the risk of being one-sided and narrow in its approach.

In addition, we should, as individuals and groups, strive for integrity. We ourselves are members of many organizations, we are intensive users and producers of information, and most of us are quite frequent users of and dependent upon computers. What is the relationship between what we say as researchers and what we do as organizational actors? Integrity is a practical approach to relevance and quality. By constantly testing ourselves, we appreciate the complexities and difficulties in developing good systems and our research becomes more convincing and useful as a result. Integrity combined with creativity is a solid starting point for developing high quality information systems research.

Jon Turner: Thank you very much, Lars. Tora..

Tora Bikson: When this panel began to develop a set of themes related to rigor and relevance for discussion at today's meeting, the chair - Jon Turner - asked us to address the issue of whether traditional information systems research paradigms tended to be biased in favor of narrowly-construed empiricism. In my view, this is a very interesting question - not because of its concern about creeping positivism but rather because of its assumption that there are traditional information systems research paradigms. In fact, I would make the contrary assumption that this field has not been in existence long enough to have a traditional research paradigm (see Figure 1).

Independently of whether or not one shares Kuhn's view of normal science paradigms, I don't think we have a tradition yet in any sense in which there are said to be traditions of science. Nonetheless, there are research paradigms that are frequently referenced in journal articles in this field - usually implicitly (e.g., in uses of sampling techniques,

*Traditional information
systems research paradigm
is an oxymoron*

Figure 1

inferential statistics, and so on). How did we get these fully-developed models of what information systems research should be? If information systems research doesn't have an indigenous tradition, I would suggest that we got the paradigms we are using by borrowing them from disciplines with more long-standing traditions. So, the main issue I will address is whether we have borrowed appropriately: are the models on which we rest our information systems research in the U.S. too narrowly empirical or positivistic? Do they emphasize rigor at the expense of relevance?

I should note, in passing, that I'm no longer sure about whether there is a general *pro-empiricism* bias in the U.S.. I, too, consulted a dictionary in preparation for this panel; *Webster's Ninth Collegiate Dictionary* (see Figure 2) gives a melange of meanings. While they are perhaps not quite neutral, they are, on the whole, more negative than positive toward empiricism. Webster notwithstanding, I'd like to begin by clarifying what I mean by research in general, and by information systems research in particular.

In defining research, I, too, emphasize process: a systematic process of reducing uncertainty about the object of study (see Figure 3). In case of information systems research, I assume the things being studied have some critical social features and some critical technical features; that empirical approaches are useful for getting hold of it; and that the findings are supposed to apply to individuals and groups of individuals in organized settings.

em•pir•ic 1 : CHARLATAN 2 : one who relies on practical experience
em•pir•i•cal 1 : relying on experience or observation alone often without due regard for system and theory 2 : originating in or based on observation or experience 3 : capable of being verified or disproved by observation or experiment
em•pir•i•cism 1 a : a former school of medical practice founded on experience without the aid of science or theory b : QUACKERY, CHARLATANRY 2 a : the practice of relying on observation and experiment esp. in the natural sciences b : a tenet arrived at empirically 3 a : a theory that all knowledge originates in experience b : LOGICAL POSITIVISM

Figure 2 Meanings of Empiricism

Now, with this working definition in mind, what's wrong with the kind of paradigmatic bias we often see in information systems journal research? I would argue that we have borrowed paradigms from old hypothetico-deductive systems, although not necessarily causal inference paradigms. Rarely does information systems research follow the classic tradition of science in trying to do to experiments which support causal inference. But I believe that our field emphasizes hypothetico-deductive research, and that in doing so it

- Research refers to procedures for the systematic reductions of uncertainty.
- Information systems is a relatively new subfield of research, in which:
 - some critical aspects of research problems are social;
 - some critical aspects of research problems are technical;
 - empirical approaches to understanding the problems are useful; and
 - findings are expected to apply to individuals and groups in organizations.

Figure 3 Other Assumptions

emphasizes the logic of verification instead of the logic of discovery (see Figure 4). That emphasis might be appropriate for a science that is a hundred years old and that has a large theoretical superstructure from which it can derive hypotheses for verification. I'm not sure it's appropriate for a just-beginning field of inquiry.

A second problem of inappropriate paradigm borrowing of this sort, closely associated with the first, is the devaluing of descriptive, exploratory, and other pre-paradigmatic approaches to the reduction of uncertainty. The paradoxical result, then, is that the information systems field

- Emphasis on logic of verification over logic of discovery
- Devaluing of descriptive and exploratory approaches
- Inattention to critical *foundational* work, e.g., on defining the units of study, making robust typologies, operationalizing key terms

Figure 4 Problems for a Newly Emerging Field such as Research from Misdirected Borrowing of Paradigms

is unable to give sufficient attention to the critical *foundational* work (see Kalle Lyytinen's research) that needs to be done before a field of science can become truly rigorous. So, in rapidly importing a hypothetico-deductive model of science, we failed to do the basic descriptive work; we didn't develop taxonomies and test their reliability; we haven't built

widely shared and validated conceptual frameworks. In short, we now lack much of the apparatus of traditional science.

There are, to be sure, significant counter-tendencies. Peter Anderson's research question, *Can we develop classes of signs as they apply to computer interfaces?* (see these proceedings), is a case in point. He poses a very good question, although it's not a hypothetico-deductive question; rather, it's a question about whether we can begin to do the foundation work in this field of science. I think that very basic field-defining questions of this type are devalued by leaping to a hypothetical deductive paradigm. Surely this is a leap of faith, rather than an inductive leap! Unfortunately, the field as a whole suffers as a result. For instance, trying very hard to do old-style hypothetico-deductive research in this new arena, i.e., testing hypotheses from a theory, usually entails the use of quantitative methods and empirical inference statistics; but these efforts typically end up, for graduate students, in laboratory experiments where the individual is the unit of analysis and any theoretical modeling that is done is of individual behavior. It is perhaps cognitive behavior, or perception, or performance; but very rarely do you see the unit of analysis being the group or the organization. So we begin to miss out on relevance by not being able to apply the results to individuals working in groups. It should be noted, however, that this does not result from an inherent conflict between rigor and relevance. Rather, it is a consequence of the lack of group and organization-level paradigms borrowed for information systems research.

Having said all that, why would I argue that empirical approaches are nonetheless useful (see Figure 5)? In the first place, especially since information systems is a new field and we don't have a long tradition of meanings, and since it's a field which has both social and technical elements, we need the scientific equivalent of WYSIWIS;⁸ we need to have a way of knowing that what you mean is what I mean. Again, Peter Anderson's work (these proceedings) gives a valuable example in showing what operational definitions are good for. They provide a way for you and me

- Empiricism as intersubjectivity (the scientific equivalent of WYSIWIS);
- Shared meanings are especially important in this new field of inquiry:
 - for collaborative research (e.g., among social and technical researchers);
 - for comparing findings from alternative conclusions with participants in or users of the research;
 - for sharing results and corroborating conclusions with participants in or users of the research.

Figure 5 Why are Empirical Approaches Useful?

both to find a common designate of the terms we share in our experience. We may fight over the interpretation, or we may need to build a shared meaning, but at least we have a common referent in our experience. I believe that what we want to do when we rest research on experience, is to find the referential base that then allows us to develop shared meaning, do foundational work, and even perhaps test hypotheses. Such a base is especially important if we're trying to link up with previous research, and if we're trying to share our meanings with people who are supposed to benefit from our research, either as managers, or as users, or as systems designers. It is fairly important, then, that we be able to build shared meanings.

But is it true that this bias in favor of empirical rigor, which I've claimed is not quite as rigorous as it ought to be, leads us away from relevance? In the tradition of basic scientific research from which we are borrowing paradigms, science is said to be value free. That is to say, it's not directly useful, and if it's not useful, then it's not going to be relevant (see Figure 6).

In other words, the bias - particularly in university settings and in peer-reviewed journals - is against applied research. When we borrowed the hypothetico-deductive model, we were borrowing a model of basic science that lends itself to laboratory research. It doesn't

- Research is relevant if it can be usefully applied
- Conflicts between rigor and relevance in part reflect the difficulty of doing good applied research in this complex social and technical domain...

Figure 6 The Opposite of *Value-Free* is *Useful*

intentionally lend itself to the discovery of findings that will fit today's organizations, where questions are social and technical, where the technology is changing, where the culture of work affects the utility of a system, and so on. So the conflicts between rigor and relevance in part reflect the fact that we have a difficult, complex, and changing field and we've borrowed paradigms from fairly basic research.

Dick Mason underscored this point in his excellent article on the knowledge yield from research activity. He argues that research produces knowledge as a function of the extent to which it meshes *the richness of worldly realism* with scientific control. He also points out that those two dimensions of knowledge are sometimes in conflict with one another, so efforts to increase scientific rigor may work to the detriment of realism and vice versa. I would agree that it is so, but actually that it's even more true of the field of information systems than with others, because we've got to deal with problems of social and technical realism on one hand, problems of social and technical control on the other (see Figure 7). Further, I submit that the drive to be more rigorous, or to worry about whether we're too

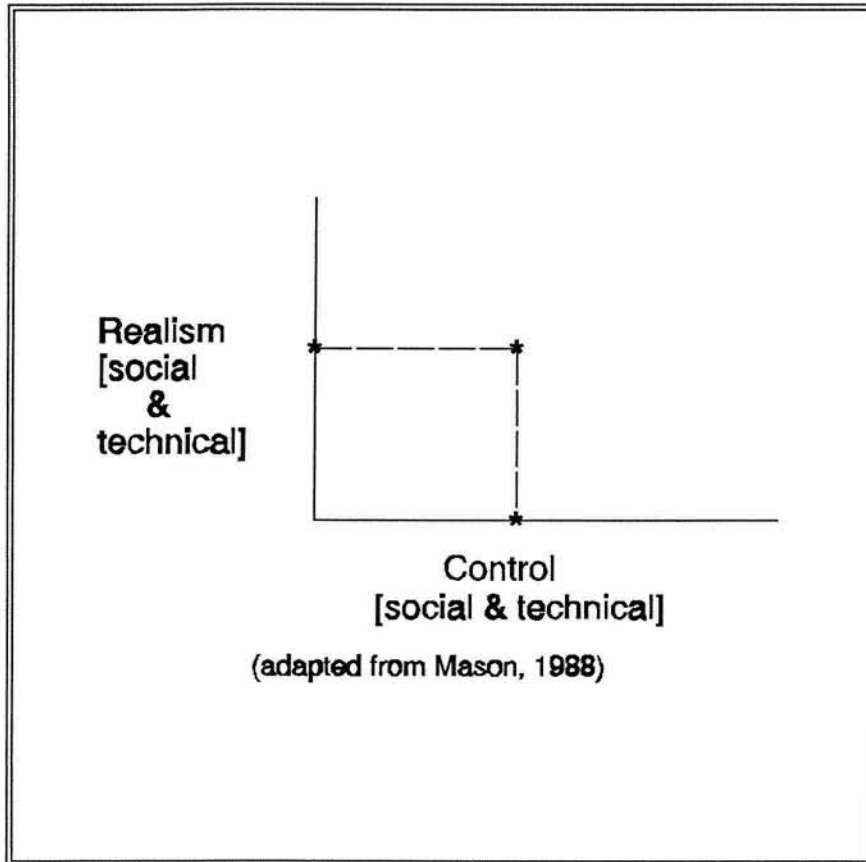


Figure 7 Knowledge Yield from Research

questions seem almost always to be posed to the behavioral part of the equation. To be sure, it is not an easy thing to put social and technical factors together in a research design that is both rigorous and relevant. But research approaches that ignore technology and its intrinsic connection to social organization, relying instead on models of individual behavior and paradigms derived from basic science, tend not to be either rigorous or relevant.

The conclusion, it seems to me, is that many of our rigor versus relevance problems are a consequence of our having appropriated paradigms that aren't suitable for information systems research, given the present state of development of the field. Rather than so much borrowed theory, we need more grounded theory approaches. We need paradigms that make conceptually based, rather than accidental, links between organized work and information technologies. We need methods that produce results relevant to groups as well as to individuals. Finally, I would argue that empirical research - without the constraints of a hypothetico-deductive model of science - can be expected to contribute to improved paradigms.

rigorous at the expense of relevance, is directed primarily to the social dimension of research knowledge. I suspect this reflects the fact that we have borrowed research paradigms from cognitive or behavioral psychology, and thus we tend to leave the technical dimension out of this discussion entirely. For example, the question of whether a particular experimental technology will generalize to other technologies is not one of the questions we typically raise when we're asking about the rigor of scientific inference. Such

As Figure 8 suggests, there are a number of steps to take now that could improve the rigor and relevance of our research. For instance, relying on interdisciplinary research teams requires the sharing of meanings across disciplinary boundaries and produces more balanced attention to technical and social dimensions. Further, work in field settings - especially over the length of time it takes to understand the properties of a technology in an organizational context - will unavoidably confront the issue of relevance. Moreover, carrying out research, in context, over time, depends in large measure on cooperation of technology users, who become co-participants in the process. In my view, these kinds of approaches will eventually result in better research paradigms, either because we are more consciously borrowing from a broader set of alternatives in multiple fields, or because we are innovatively building new paradigms suitable to the emergence of this new subfield.

- Interdisciplinary teams
- Multiple approaches
- Field settings
- Participatory research design
- Longer time perspective
- Build innovative paradigms!

The chief advantage of a new field of inquiry is the opportunity to challenge old constraints and create new synthesis...

Figure 8 Research Recommendations for Information Systems

Jon Turner: Thank you very much, Tora. Next, we have Wanda Orlikowski speaking on the role of institutions.

Wanda Orlikowski: In this panel on relevance and rigor in information systems research, we decided to focus on two aspects, research practice and research training. My assignment was to talk about research training and in particular, to comment on the dissertation as a vehicle for training prospective information systems researchers. Let me qualify these remarks by noting that I am only talking about behavioral information systems research, or information systems research that draws on social science paradigms and methods. My remarks, thus, do not bear on information systems research that draws on computer science, engineering, or economics paradigms.

In discussing the role of the dissertation I want to focus specifically on the institutional conditions within which doctoral studies are conducted and dissertations written. I want to suggest in this talk that some of these institutional conditions are currently inhibiting the use of alternative research paradigms and methodologies in the dissertation, and that this has long-term implications for the kinds of research we might expect in the information systems field. Let me hasten to say that I am only speaking about what I am

familiar with, and that is the U.S. I believe that in the U.K. and Europe, a fair amount of information systems research and doctoral studies occur in computer science and social science departments. This is not typically the case in the U.S., where most of the training of information systems researchers occurs in business schools. Hence, my remarks here must be seen to reflect their context, doctoral programs in American schools of business.

Understanding the context is important, because American business schools have a history and tradition of functionalist and positivist research. Alternative research approaches of the sort discussed here in this conference - action research, critical research, interpretivism, semiotics, etc. - are typically not found in business schools. I want to argue that to understand why this is so, we need to look at some of the institutional conditions which may be inhibiting the practice and teaching of these sorts of approaches. I want to further suggest that we need to pay serious attention to these institutional barriers if we are ever to nurture the sort of diversity in information systems research that Peter Keen talked about in his keynote address, and which has been a refrain throughout our discussions here at this conference.

Let me focus more specifically now on the training of young information systems researchers in business schools, and articulate some of the institutional barriers which I believe are inhibiting the adoption of nonpositivist research approaches. One of the first things that students have to decide when embarking on a dissertation is whether they want to do something original and significant, or whether they want to get out quickly. What I have in mind with *original and significant* is somewhat biased towards the side of relevance and nonpositivism. Positivist research tends to be incrementalist and replicative, a linear accumulation of knowledge over time. Real contributions to understanding usually come from nonlinear insights, from innovation, from taking risks, from looking at things from a different perspective. I have deliberately portrayed these two options as somewhat mutually exclusive, and while I do not want to say that someone cannot do something substantial and original and still get out quickly, I think it is difficult and rare in practice. The decision about how to proceed with the dissertation is not an easy one, and there are advantages and disadvantages to both strategies. Let me mention some of the arguments for each of these, as this will highlight the sort of institutional barriers that I have been referring to.

Let us start with the case for doing a significant piece of dissertation research, something non-incremental. One of the strong arguments for doing this, and one in which I personally believe, is that the dissertation is an opportunity for young researchers to lay the foundation of a research program - both substantively and methodologically - that will guide her or him for a number of years in the future. On the methodological side, the dissertation is akin to a medical residency, a time when one acquires the methods, the techniques, the skills and knowledge to practice one's trade for many years to come. It

is a training ground, a relatively protected, but not unrealistic, set of conditions within which one can develop, experiment, and establish particular research competencies. On the substantive side, the dissertation is a unique time in which one can devote a large amount of almost undivided attention to a particular research topic, to really explore and understand it from multiple perspectives, a time to read widely and deeply, to forge connections across disciplines, to question, think, and reflect. A rare time indeed, and very influential, as this period in a young researcher's life is likely to be intense and formative.

As with many socialization experiences, the dissertation can be seen to shape the future researcher. Consider what happens when doctoral students graduate. They move on to take junior faculty positions in other business schools, where they are required to teach some courses, sit on a few committees, and most importantly to work towards tenure. The pressure of *publish or perish* will likely lead our new junior faculty to try and leverage whatever dissertation research they have already conducted, and get as many articles out of that work as is possible. A year or so later when confronted with starting a new research project, the established practices, techniques, and skills acquired during the dissertation are the most likely ones called on, and typically a research study resembling the dissertation in substance and method is initiated. A few years later as tenure looms, our intrepid junior faculty - with any luck - have received some recognition and acceptance in a particular research area, are asked to sit on panels and review committees, and have begun to establish identities in their community. To change course at this point, to adopt a completely different research approach, to shift to a new substantive area of inquiry, to acquire new methodological skills, would be risky. Discarding a multi-year investment of time, money, effort, credibility, contacts, and routines would seem illogical at this time. Once tenure is attained, switching gears is even more difficult, for now one is established in the field as an expert in a particular area, with a particular set of methodological tools, with a cadre of students and followers, and perhaps even with research grants and contracts. The web of relationships, resource dependencies, intellectual commitments, and personal accomplishments that have grown around researchers entrap them. These networks are highly enabling in that they reaffirm, facilitate, and sustain the sort of work that has been done to date, and they constrain work that would require a radical change in research approach.

A colleague of mine at MIT, Marcie Tyre, and I have written a paper that describes the activity immediately following the implementation of a new technology as a rare *window of opportunity* for users to acquire knowledge about the technology and to develop and test procedures around its use. This window is extremely small, for technology once implemented tends to rapidly enter *production mode*, during which time not only are all energies and attentions focused on production and outcomes, but the process technology has disappeared from view. Under such conditions there is little time, let alone

motivation, to change either the process or the technology. I believe that a similar argument can be made for the process technologies we, as researchers, use to produce our research studies. We can think of the dissertation as a window of opportunity - temporary and crucial time in researcher's lives - that provides a rare occasion to learn processes, develop and test procedures, and that casts the mould that will shape the kind of research studies that researchers will pursue for many years to come. Continuing with the example, once the dissertation is done, the window of opportunity is closed, and researchers move on to production mode in their jobs as academics. The window may, of course, be re-opened later; but as we saw, this only happens in unusual circumstances or times of deep disaffection with the status quo. To the extent that most doctoral students in American business schools choose to do positivistic dissertations, they acquire a specific set of research process skills that are unlikely to change over the course of their careers as information systems researchers. As a consequence, it should not come as a surprise that information systems researchers are unlikely to pursue alternative research approaches to studying information systems phenomena.

Let me turn now to the other side of this debate: The argument for getting the dissertation done quickly. In American business schools there seem to be many institutional forces working towards encouraging this choice. An argument often used to counter the one that sees the dissertation as laying a research foundation is that doctoral students are too young and too immature to be thinking about, and establishing a long-term research program. While there well may be students for whom the articulation of such a program of research is inappropriate, it seems to me that if it is, in fact, true that doctoral students are too immature to start thinking about their research in a more long-term perspective, then we have not done a good job training them. If they cannot begin to make evaluations and reasoned decisions about appropriate research approaches, interesting research topics, and career objectives, then I do not believe that we have prepared them adequately. This argument assumes that, as researchers mature, they will be able to make the appropriate research choices. But as I have sketched above, the competency trap that befalls young researchers and the institutional reward system (that tends to recognize publications in mainstream journals) pressures them to keep doing what they know best, rather than seeking different and risky alternatives. It is thus unlikely that in the future, more mature researchers will take the time and effort to think deeply and searchingly about their research process and approach, to question colleagues and faculty, to reflect on alternatives and choice. Once employed in an academic institution, researchers enter production mode and the dictum of *publish or perish* discourages spending time reflecting, changing gears, and retooling. It is a mode of research which rewards production results, not process improvements.

On the topic of inadequate doctoral education, it is amazing to me that it is still possible to get a doctorate in philosophy from an American business school, and not be required

to take a single course in the philosophy of science or the philosophy of social science. Equally distressing is that many schools do not offer courses in qualitative research methods. While there may be three or four or five advanced courses in quantitative methods, few business schools offer instruction and preparation in qualitative fieldwork. We should not be surprised, then, that doctoral students end up choosing positivistic research approaches. After all, that is all that they have been trained in, and perhaps they do not even realize that there are alternative approaches that could be adopted. In this latter case, the choice has essentially been made for the students - a case of unobtrusive control - as they have not been exposed to the range of options available to them as researchers of information systems phenomena.

The second reason for adopting the quick route involves the analogy of the dissertation as apprenticeship. The doctoral student doing a dissertation is like a research apprentice who should learn from the master and be guided every step of the way by the master - the master in this case being the supervisor of the dissertation. This model is even more specific in that the apprenticeship, while it may also be substantive, is primarily seen to be methodological. Dissertation supervisors are usually senior faculty who have attained recognition and tenure by being good at that which is valued by business schools. And, traditionally, that has tended to be positivistic approaches to research. Thus, doctoral students adhering to the apprenticeship model of dissertations, will align themselves with senior faculty, and by default or design, will likely end up learning and appropriating the tenets and practices of positivistic research methodology. There are many reasons why such a dissertation strategy makes good sense for doctoral students. First of all, students are likely to learn a great deal from these senior faculty who have years of experience and accumulated knowledge to share. Second, senior faculty will have a great deal of legitimacy and credibility in the school and the field, and such an association can bring legitimacy and credibility to the students' work. Third, it is probable that senior faculty have well-developed programs of research into which doctoral students can fit and carve out niches for themselves. There may also be funding and access to research sites that accompany participation in the research program, facilitating and easing the dissertation experience. Choosing an alternative research strategy in American business schools - that is, choosing to do something nonpositivistic - often means having no senior faculty mentor to supply credibility, legitimacy, access, and funds, and no advisor to provide knowledge, guidance, and direction to struggling, fledgling researchers. Under such institutional conditions, it makes much more sense for doctoral students to take the less risky route, to follow the more structured and certain path, for not only is it more legitimate, but it is also psychologically safe, politically savvy, and probably financially beneficial. These advantages of this strategy also extend beyond the dissertation, into the job market and publishing process. Schools looking for job candidates are more likely to hire researchers whose approach they can understand and evaluate, that is, positivistic research. Students know this, and often tailor dissertations around their perceptions of what other schools are

likely to be receptive to. In the review process too, the historical dominance of positivism in American journals is likely to favor such research, often for the simple reason of tradition which predisposes editorial boards and reviewers towards the dominant research approach.

And last, but certainly not least, is the issue of money. Doctoral studies in American business schools are very expensive, up to \$25,000 annually. While most doctoral programs will support their students financially through some sort of tuition assistance program, these funding packages are usually limited to three or four years. For doctoral students that usually means spending two to three years engaged in course work and taking general examinations (if all goes well), and then having one more funded year within which to conduct the dissertation. It is usually very difficult to execute a non-positivistic research study in a year or less. The nature of these alternative approaches requires extensive stays in the field; time-consuming, qualitative data collection and analysis; and significant amounts of write-up time. As a consequence, students who cannot find extra means of funding, or who cannot afford the opportunity costs of another year of doctoral studies, typically end up choosing a more structured dissertation that will allow them to get done and out as quickly as possible. There are clear economic incentives for doctoral students to avoid risky and non-traditional research approaches.

I have painted a rather gloomy picture of the institutional barriers to doing relevant dissertation research. I have deliberately overstated the case to emphasize the extensive role that institutional conditions play in shaping research training. There are clearly fine institutions in America which are exceptions to the rule I have portrayed here. However, there are far too many places where the sorts of forces that I have been describing are a significant factor. I hope I have not been too pessimistic, as I believe that much can be done to improve matters, but before any steps can be taken we need to understand the institutional barriers to the sorts of research practice we would like to encourage. I hope I have managed to shed some light on these issues here today.

Jon Turner: Thank you, Wanda. Kalle...

Kalle Lyytinen: Doctoral dissertations and studies play a major role in the advancement of knowledge and also in the socialization and education of researchers and scholars in any scientific field. Therefore, it is vital to consider how concerns of relevance and rigor should be taken into account in such a new and heterogenous field as information systems; how they shape doctoral studies, and how they largely establish the research practices and patterns followed in the area. My goal is to briefly define what I mean by the concept of relevance and rigor in information systems research, and to outline the strategies and tradeoffs encountered when pursuing these objectives. In particular, I shall discuss how the issues of relevance and rigor arise in preparing doctoral dissertations and what

principles one can follow to increase the rigor and relevance in one's research. My statement draws largely upon work in the sociology of science (Whitley, Kuhn), and on the discourse oriented views of scientific research (Habermas). For the sake of brevity no references are included.

A doctoral dissertation plays several important roles, both for the scientific community as a whole, and also for each individual in his or her scientific career. The community roles include: improvement of knowledge and fostering of innovation; transfer and maintenance of problem-identification and solving capabilities (continuation of a paradigm); and sharing of knowledge and its criticism (scientific publicity) with a number of stakeholders (other researchers, tax-payers, would-be users of the knowledge, research administrators). On the individual level, the dissertation signals the achievement of a maturity and thereby provides the student with a passport to enter the scientific community as a free citizen; it provides a way to socialize him or her into research patterns and practices needed to survive in the community; and, finally it builds a foundation upon which to continue a scientific career.

In an ideal world, a good advisor should point out the accepted criteria for improvement and safeguard the student against non-improvement; her or she should locate the student in a disciplinary matrix needed to successfully practice research by cultivating necessary competencies; and finally he or she should teach necessary communication skills so that the student can make his or her research claims both warranted and clear.

The concepts of relevance and rigor in research are unclear, nebulous and devoid of accepted meaning. Relevance, in general terms, is concerned with the matter at hand. Rigor, in turn, is something which is strict, severe or harsh. (For British readers, the American spelling also reminds one of sudden chill with shivering as e.g., in rigor mortis - not necessarily a bad metaphor in some situations!!!!). When applying these general connotations to the information systems context, I would define relevance, in information systems research, as being concerned with: a) the improvement of knowledge related to information systems (concerned with the matter at hand), and b) something which is shareable and can influence the behavior of stakeholders in the information systems field (other researchers, practitioners, etc.). Relevance is defined in a dissertation through a problem statement and identification of research goals and thereby it is always dependent on a specific research community (paradigm).

The rigor of doctoral research is established by the canons of research that emerge within a specific disciplinary matrix. In this sense, being rigorous in research means that one follows strictly the canons that are handed down in traditions of the community. The components of strictness are: objectivity (anyone following the same canons/rules reaches the same results), and generalizability (rigorous manipulation of available evidence as

defined by rules of inference and calculation), and strength of argument (the research should develop arguments that support it against the widest possible challenge and it should show good scholarship). Usually, rigor in dissertations involves standards of format or use of inference rules that necessitate specific research skills (such as knowledge of and skills in statistics, formal logic, and scientific argument, etc.).

By and large the Ph.D. dissertation should address both issues of rigor and relevance. In other words, a Ph.D. dissertation should formulate and investigate a research topic which lends itself to rigorous treatment (in one way or another) and which is relevant (in the sense that it improves knowledge and can be defended against the claims of research stakeholders).

If the dissertation is not rigorous, it becomes just a matter of *opinion*. Another way to put it is to say that rigor deals with quality in research. Therefore, at its worst, without any quality control (in scientific sense), non-rigorous research turns into consultation or journalism. Rigor is promoted by good education in research skills, high methodology standards and a scholarly climate.

If the dissertation does not focus on issues that are relevant to stakeholders, it becomes esoteric. When it is not concerned with information systems matters, it becomes *play* and pseudoscience. Thus, non-relevant research is scientific activity without the real impact that science and research should have on practical affairs. Relevance does not mean, necessarily, that everything explored should be of immediate use and applicability. Rather, this depends on the stakeholder-set of the research project. Sometimes research that addresses internal problems and the logic of science (scientific criticism) can be more instrumental for the advancement of knowledge than is the replication of similar types of research that has close connection with practice.

All in all, by combining the dimensions of rigor and relevance into a 2x2 matrix, we can produce a grid of research situations as depicted in Figure 9. Research can be non-rigorous, or rigorous and it can be relevant, or not-relevant. The goals of any research project should aim, at the same time, at high rigor and relevance. However, this is very difficult to achieve and these *stars* are rare. They require serendipity, excellent research skills and strenuous effort.

The various schools and traditions in information systems research fall into different positions in this matrix (it is suggested that the reader locate himself and his/her research topic and those of widely known schools). The analysis of the figure also reveals that the lines drawn on the vertical and horizontal axis define the domain of possible dissertations. This domain is, of course, shifting and changing as information systems research evolves and new schools arise. In fact, it is clear that the various schools in the heterogeneous

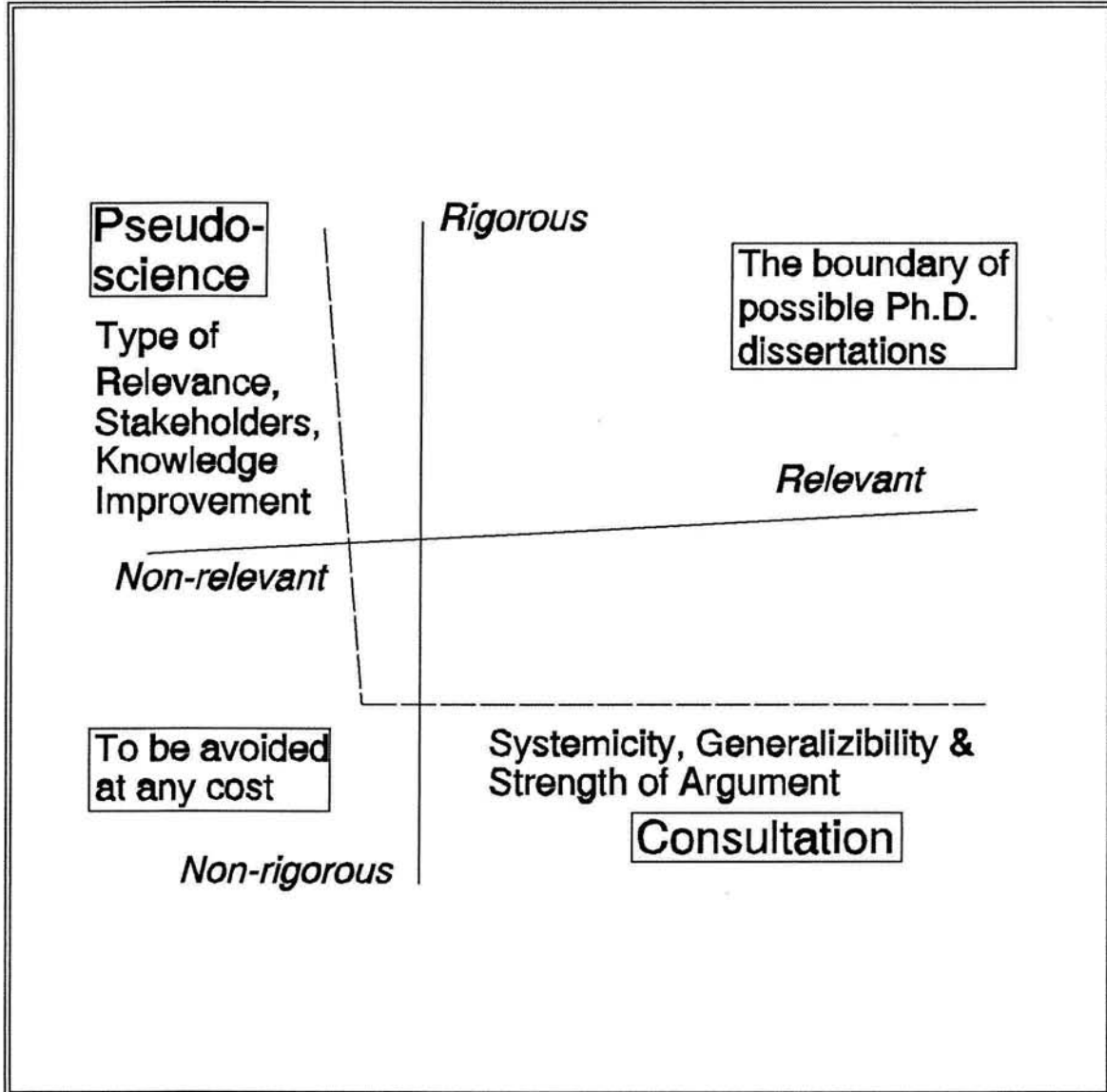


Figure 9 Research Matrix defined by Rigor and Relevance

information systems community would draw these lines differently. The positions outside this area belong to dissertation topics that are not suitable as a dissertation topic, or which are researched in a way that is below acceptable standards in the research school or tradition. The non-rigorous, non-relevant corner of the matrix should be avoided at all cost. Unfortunately, there are even recent examples of research that fall into this trap.

For each research project, we have at hand specific tactics that can be used to shift the dissertation into a better position in the matrix (isometric lines so to speak). To increase relevance, one should consider issues of stakeholder set, type of research, and knowledge improvement criteria. To increase rigor, concerns of systemicity, generalizability, and the

strength of the argument are central.

There are three general tactics that can be used to address the relevance question. First, one should identify the set of stakeholders concerned with the research being pursued. Several roles and stakeholder groups can be identified: academic community, general public, policy making bodies (for information technology policy), management, and professionals. The first one is obvious and it is necessary to address the issue of knowledge improvement in a scientific sense. However, in such a practical field as information systems, the confinement of the stakeholder set only to academic communities may, in the long run, decrease the impact and acceptability of information systems research. Usually, the larger the scope, the more relevant is the research problem (and also the more difficult it is to solve and address in a rigorous manner).

Second, type of research deals with the knowledge that is attained (aimed at) in the research. Does the research aim for knowledge that has explanatory power, statistical predictability, understanding, or criticism and philosophical (logical) argument? Each of these types of knowledge can be used in a different way and has a different mode of relevance. Excluding some of these as being relevant also excludes this type of research as being acceptable. Traditionally, only knowledge needs pertaining to explanation and statistical generalization have been accepted - especially in the North-American scheme. However, the situation has been changing rapidly during the recent years and more and more research addressing other knowledge needs are also accepted.

Finally, research relevance addresses the types of knowledge needs and how they relate to each of the stakeholder groups. For various stakeholder groups, different types of knowledge needs may be important. For example, management may need knowledge that helps to control (i.e., explanatory knowledge), whereas users and/or professional may need knowledge that fosters criticism and understanding.

Rigor should not be mistakenly confused with quantitative research. Rigor does not necessarily increase by the use of highly sophisticated and complex statistical methods. Nor does it necessarily improve by applying careful controls in experiments. Instead, rigor deals with the systemic nature and clarity of the arguments that are put forward to support assertive claims in the thesis. Sometimes this may require quantification or high degree of formalization, other times it may not. Instead, high formalization may even reduce clarity by ignoring relevant facts in the research situation, and thereby making research just formal play that does not increase insight and strength of the argument. For example, by not representing those aspects of a situation that influence outcomes, or those that are of primary interest to actors. Conversely, qualitative research is not necessarily less rigorous. Barley (1990) makes this very clear when he observes:

The reason that so much of the so-called *qualitative* research in organizational behavior is thin and of questionable worth has little to do with the fact that research is qualitative in nature.

Bad research is bad research, independent of the type of research machinery used and canons followed in carrying out the research.

Rigor aims to improve the clarity and structure of the argument. It provides, also, yardsticks to evaluate the generality and objectivity of these arguments (through internal/external validity, statistical power, etc.). The road to higher rigor involves addressing the issue of coherency of the argument - does every claim build upon the earlier ones and does the flow of the argument emerge naturally and completely? How well are generalizability issues handled, and is the researcher aware of the limitations and possibilities for the generalizability of the results? If too bold, or too modest claims are made, the research project may not be rigorously carried out (though it may look that way!!!!). In general, however, the problem is in claims being too bold rather than too modest (see e.g., Orlikowski and Baroudi in *Management Information Systems Quarterly*, 1989, on the issue of statistical power in information systems research)! Rigor also deals with scholarship and with the way the argument is presented. This is reflected in coverage of the literature, clarity of the conceptual analysis, carefulness in explicating, method choices, skill in method application (such as context analysis, protocol analysis) and how the logic of justification on the whole emerges.

To summarize, each doctoral student should address research relevance by asking:

1. What research interests are at stake here and what is the type of research pursued (i.e., how is the domain of research built up and what epistemologies work here)?
2. What are the possible audiences and stakeholders that would listen to my research? What are their knowledge needs?
3. What are the going concerns in the information systems field as whole that warrant the research effort (at least in the long run)?

In a similar vein, the issue of rigor should be evaluated by considering:

1. Do I exhibit wide scholarship and good reading in my work?
2. Have I invested in necessary research skills to carry out the research project and what level of competence is required to carry it out?

3. Is the context of justification clear in terms of general methodological rigor and the clarity and strength of the argument developed in the thesis?

I discuss, above, an ideal situation. In practice, trade-offs must be made due to available time and the investment required to achieve a specific quality/relevance level. Moreover, it is difficult to build up and maintain expertise in all paradigmatic villages of a multiparadigmatic community such as information systems. This can be overcome, to some extent, by building up multidisciplinary networks and invisible communities. Also, a student's own personal choices, i.e., the *problem of informed choice v. invisible handguiding*, may hamper the possibilities to improve the rigor of the research. Finally, curriculum planning in an environment that addresses both rigor and relevance simultaneously is much more complicated than in those environments that focus on only one dimension at a time.

Jon Turner: Thank you very much, Kalle. I'd like to open the floor to questions. I'm sure you're all waiting to go. Yes...

Lynda Davies (Griffith University, Australia): I'd like to ask whether we are really questioning what research is? It seems to me that we've forgotten that research is about taking risks and about facing the potential of failure. What we are really talking about is risk-averse research as opposed to risk-taking research. Doesn't the panel think that rigor is where we can deal with many marginal risks; and relevance should be something where we are still willing to take risks and potentially do non-relevant research?

Wanda Orlikowski: Well, in a way you're right. Instead of positioning this between positivists and non-positivists, it could just as easily been between non-risky and risky research. It is a historical accident that positivist research is the dominant method. So deviating from it will be risky for students, or for junior faculty, or for whoever. To the extent the dominant method is seen as the acceptable way of doing research, deviating from it will be viewed as perilous. If we can possibly change the nature of that debate and not see things as one thing being dominant and everything else being deviant from it, but maybe seeing more pluralism, or diversity, being acceptable, we may be able to move beyond the risky and non-risky issue. But, you said something else that I didn't quite catch.

Lynda Davies: In action research, what we are doing is negotiating what we are going to do with people within an organization. I've heard a lot of talk about being more relevant and if you're not careful and only give people what they want, it is very often at the expense of the research that you are trying to do. So I would say don't just go for

relevance, but allow that to be risky as well. You may come out with things that people can't use, but we can learn some things and that's the point of research.

Gadi Ariav (Tel Aviv University, Israel): I'd like to pick up a theme from Lars and Kalle and just quickly take it one step further. I'd like to challenge the whole premise that we are in some way enslaved; that with rigor and relevance you only get one at the expense of the other. I think that it's only true if you take rigor in a very strict positivist sense and I believe that is the point where Kalle ended and Lars actually started. Since you cannot establish relevance, except after the fact, a point I think well made, can anyone point to a major result that has been achieved through qualitative research? We are advocating relevance and doing qualitative research, but have we achieved something that we can put on our flag and push forward? If, on the other hand, we replace relevance with validity, if we are looking for something which is more valid, in the sense that we are looking at the true nature of the phenomenon, I guess we're on more solid ground, and consequently, more rigorous.

If you define rigor in a broad sense, and there's no way to avoid it when you see a doctoral dissertation, when you judge an article, or judge a piece of research, we are dealing with disciplined inquiry (Glaser and Strauss⁹) and that is the basis for rigor. I would suggest that the ultimate basis for judging is logic; how tight an argument is made. I guess the difference between good qualitative research and journalism is that in the former you make very explicit the structure of your argument. And you make that part of what you try to say and you know you are going to be judged accordingly. So, if we accept disciplined inquiry as the definition of rigor, then there's no distinction, there's no controversy and we can pursue both at the same time.

Jon Turner: There are two issues embedded in your point, Gadi, that I'd like to comment on. First, I don't think we push the *question of the question* hard enough. This is highly subjective, but we spend a lot of effort investigating uninteresting questions. It is difficult to craft a good question and we don't have the same guidelines that we have for applying our methods. We don't push ourselves hard enough in asking, *Is this an interesting question?* and, if we get results, *Have we learned something worthwhile?* I don't think an uninteresting research question is ever one of the reasons given for rejecting a paper (for publication). This is the real basis for relevance and we keep ducking the issue. Perhaps it is more gallant to tackle an interesting problem, where the way to investigate it may be risky and not be clear - where it is not at all obvious how to approach the research - than it is to do a wonderful job with an uninteresting question. The great skill in research design is to use one's tool kit creatively to find a mix of methods that allow investigating messy problems.

Second, we are frequently sloppy in applying our research methods. That is, we do not

give enough attention to quality issues, independent of the methods being used. For any method there is a set of issues that have to be addressed in order to achieve high quality, which cannot be left on the side. It is a major flaw if you don't deal with them explicitly, in the research design and subsequent write up.

Gadi Ariav: Regardless of whether you're a positivist or non-positivist.

Jon Turner: Exactly. If we want to improve research in information systems, I think we have to come to grip with these two issues: Finding really good research questions and attending fully to quality issues appropriate for the methods used.

Lars Mathiassen: Well, I just wanted to relate to the two last comments from you. The language game of research - I'm trained as a computer scientist and when I started in computer science, all these guys were talking about theory. I, along with some other students, wanted to do something relevant, and they called us the *external aspects* (laughter). So, they were talking theory and I was sort of an external aspect. It took me a while before I realized that they were sitting on a chair defining the way things work. For a while I said, well, from an external aspect point of view (laughter). Well, after a while I found out that it was a very bad political decision, so I started to say from a theory point of view (laughter). I tried to enforce into my subculture, together with others, the idea that theory was not necessarily written in greek letters and you didn't need an equation or something like that. What I am hearing is it is not so much a question of rigor v. relevance: rather, you should be offensive and you should say, well, the rigor of this qualitative study is such, and occupy the seat of rigor. It's our seat as well as others! So, theory is me, rigor is me, and I am struggling to define this word. I think that this is practical politics and this is the way that we can sort of make it easier to move ahead.

Rob Kling (University of California, Irvine): I'm also in the Information/Computer Science department and the term theory, for example in the computer sciences, has been basically a piece of imperialist politics (laughter). Mathematicians, if we ask what they do, they don't have theory, they have theorems (laughter); they have no understanding of digital systems in any systematic way. That's the beginning point, at least for those of us in the computer science world, to understanding some of the politics of what we do. My sense is that those who want to do qualitative research in business schools, as with most computer science departments, are basically finding a very tough political game.

I want to build on some of the comments of Wanda and Kalle, but let me answer Gadi's question first. You know, what are some of the good results of qualitative research? Just as an example, the studies of computerization and ideology, and almost all of the studies of the politics of computerization are done with qualitative research methods. Now, in a book *Computers and Politics*, I've also done some quantitative studies examining the

power of payoffs of computer systems, but by and large the literature about what people believe systems are good for, how they should be used, issues of values over systems, is almost completely a qualitative literature.

When someone says that acceptable articles are quantitative, that they must test theory, then this is a journal¹⁰ in which people cannot have discourse about the ideology of systems, cannot have discourse about the politics of systems development, or the politics of systems use. Consequently, whole realms of discourse are basically eliminated by what seems to be a technical choice of method, and this is, from my point of view, something close to intellectual crime (laughter). One can say, this study, whatever its style - whether its German critical theory, whether its grounded theory, whether its done in a sort of research tradition of experiment - is better than that one; it seems to be more carefully worked out, more reliable, the authors have honed their questions more sharply, they're more careful about the evidence, they're more careful about boundaries and that kind of systematic thinking is related to what we think of as rigor, what we think of as reliability and also what we find insightful about studies. To the extent that we're going for insight, it often comes about by very careful thinking and it is hard to find people who will say, *I really don't believe in deep thinking* (laughter)...*thinking carefully about the subject*. It's hard to find partisans of non-rigor!

Rigor today is a keyword. In the U.S. we don't talk about race, we talk about homelessness (laughter), or drugs, or other things which are related to race. Here, we don't talk about quantitative v. qualitative methods, we talk about rigor. And rigor, I think, is a token, a tag for that debate. For the researcher who has to deal with issues of, for example, power and domination, then the methods have to be open. But this means at times raising methods and questions which destabilize the status position of business schools in the U.S. Just to address that issue for a moment and this is another piece of the problem: business schools, particularly the high status business schools, give to the Fortune 500 or the Fortune 200 companies and are in a position, by and large, at the upper end of a system of privilege, and they don't want to have that system of privilege disrupted. The academics in business schools are trying to disrupt that system. As a piece of personal biography, I was on a campus wide committee for tenure and promotions for four years and in that setting it was easy to see the way in which the structure denigrates the schools of business and treats the technical worlds, the professions.

Business comes after medicine, comes after law, and when the faculty in business want legitimacy they point to something scientific looking about their work. And that's the part of the American system of academic statuses. So, for us to do work which doesn't support the system of statuses, which devalues quantitative work and something which looks like quantitative social science, also undermines and makes it harder for the deans to get faculty positions, money, buildings and resources. As a result, those of us who are

doing qualitative work are working in a frame which is fundamentally difficult because it doesn't support the status system. It asks questions which are scary, it asks about the clients of the business schools and yet these are some of the most meaningful questions and appropriate methods and therein lies, I think, much of the dilemma.

Rudy Hirschheim (University of Houston): I'd like to offer a slightly different view of rigor. I believe rigor has more to do with the *quality of the argument* than anything to do with mathematics or anything else. We seem to translate rigor into its quantitative component. That, if it's quantitative, it is, therefore, rigorous; if it is not quantitative, it is not rigorous. I think that this is a view that many people seem to take, and I think it is wrong. I think the quality of the argument makes it rigorous or not and the problem is, in the past, we've looked at journals and said we only accept rigorous arguments and mean by this, rigorous arguments that use mathematics as the base or quantitative methods as a base, because the community as a whole has generally accepted these as a vehicle for understanding the strength of the argument. The problem is, the community generally hasn't and doesn't know how to value or assess the strength of qualitative arguments. The studies that Rob pointed to are some good examples of very, very fine pieces of qualitative work that I think, generally, people recognize as being good and rigorous.

The point I want to make is that we quite often have lambasted the *positivists* for being narrow minded and not willing to accept research which isn't of that particular mold. Truthfully, and this doesn't go for everyone, but in fact there are many journal editors who actually would like to see good quality research where the strength of our arguments are based on qualitative data and they are analyzed very, very well. I think we need to find more people who are willing to review such articles. The point is we have a base of editors who are willing to look at qualitative research. They need reviewers who are going to be able to do a reasonable job in reviewing such qualitative papers and its not as bleak as we think because in 1984, when we ran the previous conference, we had a group of about 55-60 people who attended. The second conference, this one, we have about 180 people. What you do is you generally build up a group of people who are sympathetic to a cause, who want to learn, want to read more about it and gradually we will build up a nucleus of people who will be willing and able to write qualitative papers, and also review them. I don't think that its as bleak as it all seems and, just as a plug, there are also many more outlets to actually place this kind of work then ever before; new journals like *Information Systems Research*, for example, and Dick Boland's *Accounting, Management, and Information Technology*. These are all places where this kind of work can actually be published. And then one last thought, things aren't as bad as they could be in the U.S...they hired me (laughter).

Heinz Klein (SUNY-Binghamton): I would like to add to the comments of both Rob and Rudy Hirschheim. First of all, I agree with everything that Rob has said, except, I don't

think that it is all that can be said. He may have characterized the mainstream and the large majority, but there is a not to be overlooked minority of schools where positions are allocated on different criteria. He implied that all schools allocate positions by academic prestige as measured by the degree to which we are similar to physicists and mathematicians. There are other schools that consider how important the business schools are as milk cows; that they have large numbers of students compared with the faculty. Now if that is the case, it takes the deans considerable leverage to say, if we have a new position we can take in 100, 200, whatever more students which will reflect very favorably on the university budget. In such schools, the business schools can be richer and have a large degree of autonomy from this kind of academic pecking argument.

A second type of school are those where the power centers do not revolve around mathematics and the natural sciences, but about a liberal arts establishment. My school happens to be one of these, but I can assume that we are not unique. Now these schools with philosophers, historians, sociologists and so forth, look down upon business schools as philistine and ignoramus (laughter) and if you then do work of the type that I do, or Rob Kling does, you all of a sudden become respectable to philosophers (laughter). All of a sudden you move up in the pecking order. So, as Rudy said, things are bleak in the United States, but there are a lot of rays of hope that it can pick up.

Now, this brings me to my second point, what could we do? I think there are a lot of my friends here who are holding very senior positions. There is something we can do to spread the political movement as Lars Mathiassen clearly said. First of all, I think we can advertise those schools that are willing to accept qualitative work. I also believe that rigor is important, although I define it much more broadly - I think it is important to distinguish whose arguments. So we can say, there will be little problem, in these schools, to get committees together with a philosopher on it; it doesn't mean that you can bullshit happily away, but you may get another year if the dissertation needs to be cleaned up. So we can advertise that. Secondly, most of us are somehow or other involved in the hiring process. Why not advertise in our positions announcements that we look for experience in qualitative work. And that may change the climate.

Lars Mathiassen: Well I just want to make a small comment and I think we are sort of talking about the political process. I think we are overemphasizing the politics in relation to other researchers. So, if we talk in terms of credibility of what we do, and that was another very good point made by Peter Keen, we have to be more credible, in relation to other researchers, for instance, in assuring the rigor of our work. But, credibility in relation to the real world of information systems comes from something different, and I just want to ask a question, *How can we tackle that?* I mean, are we to compete with our reference disciplines in becoming a discipline the way they have, or do we have another possibility of creating credibility by relating in a different way to the practice of

information systems - by being more persuasive and useful. I'll just close with this question. I think that this is part of the collegiate process, a very important part.

Geoffrey Walsham (University of Cambridge): I agree that we need to be relevant in the information systems community and I think that is our main mission indeed. I would like to make a slightly more critical comment about the conference and about the comments we're making this morning. I'm a little anxious that we're all patting ourselves on the back that qualitative research is valuable, which I certainly believe it is. I'm a little anxious because the quality of our qualitative research is not very high. And, I am rather worried about what happens when we show our research to people in a reference discipline and get their comments. I think if you are using sociology, or psychology, or economics, or whatever, you should show your articles to people who are in those reference disciplines and listen to their criticisms and your naivete, or my naivete - since I speak from experience of doing this. I'm not saying that we then supplant the reference discipline, but I think we do move in the direction of rigor in qualitative research, which, I hope this isn't too critical a comment, I have not seen at this conference.

I would appeal to the editors of the journals that are sitting here to recruit onto their editorial boards at least one respectable representative of all the main social sciences. If they get a paper that relies on sociology, or economics, or psychology, they should refer it to that person at the least. I find, sometimes, the standard of refereeing of those people within the information systems discipline, who purport to understand the reference discipline, is rather lower than those in the reference discipline, which is not surprising. I think we should be a little careful in assuming that the quality of our qualitative research is currently high. I personally think its currently low, and I am suggesting a way, one way, in which we could try to address this issue.

Tora Bikson: I'd rather like to second that, but then go on to suggest that its really appropriate to do research with a team of researchers, one member of which might be a member of the reference discipline. I think its very offensive to people who are in the reference disciplines to see something coming from a completely different tradition, who probably wasn't trained in it, think that you can, overnight, become an excellent ethnographer by reading a few pieces that someone else has done and doing it. I think it lacks the socialization into the practice. This isn't to say that you have to go back to graduate school and get a degree in it, but the apprenticeship process that happens by working with a co-researcher who is trained in the discipline, I think, is a very good way of acquiring that kind of skill.

The same thing, by the way, is true for quantitative research. I think it looks as though its real easy and you can probably get a dissertation done in a year, just by giving a survey and doing the quantitative analysis. I think that that's a very bad set of

assumptions to make. It probably succeeds in some schools, but it is really quite a mistake. I think it is a good idea to have a card carrying quantitative social scientist on board if you're really going to undertake an extensive piece of work in that area, unless you have training in it yourself. So, it is really a mistake to think that somehow it is easier to do rigorous quantitative research than it is to do rigorous qualitative research. I'd like to put a pitch in on dissertation committees and on research teams and on publication teams, to include a person from a reference discipline instead of making the assumption that we can just do it all ourselves by having read a few books.

Kalle Lyytinen: Well, its just a short comment. I actually wanted to point out that I think one of the key issues is that we lack multidisciplinary networks, as a community, and as individual researchers. It is a problem of where you are; so, it's not just saying that you want to do that, but it's much larger. It is the problem of institutions and others, which are how they are framed and so forth. In order to investigate the question with rigor, one has to look at rigor as a surrogate for quality of the research; that is, how good it is. Yesterday, at the ICIS doctoral consortium, where the chief editors of the three major information systems journals, at least from the U.S. point of view, said that the acceptance rate is about ten percent, which shows that the quality, no matter whether its qualitative or quantitative, is very low.

Ken Kendall (Rutgers University): I've heard comments about quality and I'd like to return once again to the topic of bias. Having done both quantitative and qualitative research, I believe that people think quantitative research is a much purer, more honest system of doing things and not biased. That is not true of course, just as if we had all agreed that a first-come, first-serve system would be absolutely great, that it would be the fairest way to get a taxicab out here. But, we wouldn't want the elevators in the hotel to operate on a first-come, first-serve basis because that would be chaotic. That queuing discipline only applies in certain situations. So, when I do quantitative research, do factor analysis, I don't like to choose varimax just because its there and because other people accept it. I will find another way to rotate factors, and so on. Now, the key here is we have to have the same concerns in qualitative research. We don't want to feel that we are less biased than quantitative research. We should be subjective, we should consider this as being an artist in making decisions on our qualitative research. But, we should also have structure in what we do and that is a key feature for both quantitative and qualitative research. Would anybody like to comment on the call for structure (laughter)?

Tora Bikson: Basically, I think that that's part of the meaning of research; the definition I like is a *systematic reduction of uncertainty*, because I think structure is at the heart of it. I don't understand really why there is, except unless its a political debate, an argument between quantitative and qualitative research. Some problems are much better addressed with one set of methods then the other. I have seen such incredible misuse of quantitative

statistical methods that I would hesitate to recommend them to a graduate student who hadn't been well advised under what circumstances you want to use a particular test, what you have to make in the way of assumptions about underlying distributions, and so on. But, I think the same thing holds true of qualitative research as well.

Sten-Olov Forsgren (University of Umea): I would like to add another view. For me, rigor is very much a question of trust; I want to trust the results. One indication of people trusting their results is when they can use them. Perhaps this is very close to what Lars would say. We are just now grappling with important ideas and we are right in the middle of the research process. We are discussing important questions and listening to each other. We can feel it in the air when something important in research is happening. It is very important to have these feelings. It is the very first time for me at this conference and it is also a way for me to say thank you to Hans-Erik for a very nice experience.

Michael Epstein (MIT): It has often been said to me during my years in the doctoral program, a bit more than I'd like to remember or to admit, that much good social science research comes out of a very deep personal conviction, or a very deep personal value. I think that if you're speaking personally, you are presuming something that is of deep importance to me and stems from a deep need to understand something. That, in itself, will ensure that your research meets at least your own standards of rigor. So, I think that if there's one thing that I got yesterday, it is that research is an instance of information systems design and, as such, involves the appropriation of meaning. The researcher is often in the position, much as the systems designer is, of arbitrating between sets of potentially different stakeholders. I think a question is, *To whom is the research relevant?* It may not be immediately relevant to today's question, as framed by one particular group of stakeholders. Maybe part of the research process is convincing one or more sets of stakeholders of the importance of the research.

Wanda Orlikowski: I agree absolutely. One thing that we haven't talked about is *relevant to whose* interests, which is, of course, the point you raised. It also addresses Linda's original point, that is, if we care too much about the client, then there is a concern that we will be co-opted by them, or in fact, give up some integrity. So, I do think it is important to make sure that we have the target audience well defined, as well as ourselves in that loop.

Michael Epstein: If I could say one thing more, Peter Keen commented about the incredible power of the systems designer in appropriation of meaning and in managing that creative process. The same thing can be said about the power of the researcher in the appropriation of meaning among stakeholders in the research process.

Bonnie Kaplan (American University): I want to make two related comments. The first one has to do with bringing our work to the disciplines that we're drawing upon for advice and review. I think that is a superb thing to do with one caveat. They don't understand us, necessarily, any better than we understand them. And you can run into some difficulties, as well as some marvelous educational experiences. I wanted to propose one possible additional way of achieving something similar. There are a number of us here who do come from other disciplines and perhaps we can serve as helpers to each other. I see people struggling to do the kinds of things that they have been trained in and vice versa. I struggle to do the kinds of things that they've been trained in. We should be able to learn from each other, if only we knew who those people were and where they came from.

And this brings me to my second comment, because in some sense, part of the problem for legitimacy from our work has to do with its invisibility. When I go back to my institution or other institutions, I have to hide under a basket the fact that I have a doctorate in history. This is not a legitimate sort of thing to have when you're in information systems. It is hard to know who is doing these kinds of things and where, maybe, you can find assistance within your own field. Referring to comments made by Heinz and Rob, if we can make more public what we do, who we are, why its legitimate, the outlets for it, and the ways in which we are doing something worthwhile, we will be better off. When we have to squirm in through the back door, we are buying into their sense that we're illegitimate and I'd like to see a much more *open* stance for what we do. One modest suggestion, along the lines of what Heinz was saying in terms of advertising and helping each other, by making public where we're coming from and what kinds of expertise we have, although it may not be considered the right kind of expertise, we could really help each other.

Kalle Lyytinen: Just a small comment on that. I think that one of the problems is that we are sometimes too shy in relation to these, what we call, rigorous disciplines. I've been working with speech-act models and speech-act theory for a very long time. I went to discuss this material with some linguists and they said that, gee, I was the first one who has ever found anything useful in it. So it shows that they can also learn from us.

Duane Truex (SUNY-Binghamton): I've been listening this morning and thinking about the whole conference and about the other IFIPS conferences I've attended over the last five years. I'm thinking about themes of power and structure that obviate against us doing some of the things that we think are important, about the political struggle that may be involved, about how we define the way in which we do research, about the types of methods and the quality of the methods that we use. Implicit in a lot of these thoughts is that we are actors, but actors, in effect, who are played upon rather than actors that have power. I can't help but remember some things that Tom Peters has said about

companies in the U.S. - that they, among other attributes, have a bias towards action. That bias toward action, it seems to me, is a part of what we're talking about because we are, in a sense, the problem, as well as also being the solution. The way that we act - as if we have power, as if we are providing relevance, as if we are using techniques that are rigorous - has a lot to do with how we are perceived. It is important for us to build a sense of community, to meet, to stand up, to take responsibility for sponsoring conferences, and that we say to the world, *Hey, this is important!* We are going to act, to take risks, and we are going to do these things as a community.

Jon Turner: Well, I want to thank you all. This has been a very stimulating session. I'd like to close with a little story; stories seem to be popular in this group, both as icons and as metaphors. This one is about the importance of culture, since a major theme of this conference has been research culture. I'm from New York City and as you probably know, one frequently hears automobile alarms going off. In the morning there are pieces of broken tempered glass on the sidewalks, and many cars are stolen. I don't know if any of you are on the east side of the hotel, but last night about 4:30 in the morning I heard a car alarm go off. I sat up in bed, and said, *My God, there go the ICIS proceedings*¹¹ (laughter). I'd like to thank our panelists, and thank you all, and Hans-Erik, our host, for an excellent exchange.

Endnotes:

1. Professor Jon Turner is Director of the Center for Research on Information Systems. For the past six years he has been Director of the Doctoral Program in Information Systems at New York University. He was trained in behavioral, computer, and management sciences.
2. Dr. Tora Bikson was trained in both behavioral science (psychology) and philosophy (logic). She is principal investigator on a number of National Science Foundation (NSF) and private grants concerned with the use of information tools in the workplace.
3. Professor Kalle Lyytinen's principal research interests concern theoretical issues in understanding and developing information systems. His main focus is the linguistic foundations of information systems and the development of design and analysis methods based on these notions. Professor Lyytinen was trained in computer science and business economics.

4. Professor Lars Mathiassen's research concerns information systems with primary emphasis on systems development. He has used action research to investigate, in cooperation with trade unions, the use and introduction of computer-based systems in organizations.
5. Professor Wanda Orlikowski does interpretivist research in information systems. She was trained in both the United States and abroad.
6. e.g., *Communications of the ACM*, *Management Information Systems Quarterly*, *Journal of Management Information Systems and Management Science*.
7. The session was held in a steeply-tiered theater.
8. WYSIWIS: What You See Is What I See.
9. *Grounded Theory*, Glaser and Strauss.
10. The reference is to a recent editorial in *Management Information Systems Quarterly*.
11. The evening prior to the conference, boxes containing Volume II of the Proceedings were stolen from Hans-Erik's automobile.