15 CONFERENCE REVIEW: IS RESEARCH—A DOUBTFUL SCIENCE

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Introduction

Those that attended the conference were, generally speaking, sympathetic towards thinking about the methodologies that we use and were also aware of the wider societal and epistomological issues and implications.

As there was no consensus, it is difficult to summarize in a few words what was learned. However, we have now established a platform/basis for understanding, reflection, perfection and change. "In this way we may better transcend our present habits and liberate ourselves" (Habermas 1972).

From this point of view, the conference was extremely successful and I think that very few will go home and be the same. We have all learned something.

Problems Raised

One of the major difficulties was to actually identify what research is. It must be realized that we only call something creative if it differs marginally from something we already know (March 1972). Therefore, we can only appreciate/recognize small increments. Anything larger than this is called unscientific and is not recognized.

For example, in the field of medicine we may accept the credibility of acupuncture and perhaps reflexology, but what about voodooism or faith healing? And, within our own field, there is an increasing degree of uneasiness when we move from traditional research and protocol analysis to using phenomenology or, for example, using a feminist perspective.

Incidentally, the absence of a feminist perspective was noticed. Even though there were several excellent female speakers at the conference, they were only excellent within recognized scientific disciplines—they did not challenge or write off our traditional research as a masculine-based domain (which I would not have found unjustified).

But how do we define research? How does one distinguish between what does or does not constitute research? I would go so far as to say that research is whatever we choose to call research and is known/defined by the rituals that are associated with it.

One thing that is embarrassingly clear to me is that we are quite a long way from the scientific ideal of "advancement of knowledge." If that was the sole objective for those activities that we choose to call research, we would find it extremely difficult to justify what we do.

It was also made quite clear by Milton Jenkins that it is not possible to follow several objectives when carrying out research. I agree, one cannot write a textbook and carry out a solid piece of research at the same time. How can we ensure that ''advancement of science'' and ''writing a Ph.D.'' are not in conflict with each other? It seems to me that in many cases the "test" aspect (i.e., finding out whether the student is good enough) takes precedence over a true contribution to knowledge.

Furthermore, we define a number of activities as research (advancement of knowledge). But there are other objectives. The acid test is: "Would we carry out the research if we did not get rewarded?" Suppose that our name was not put on the front page of a paper—would we still write it?

These other objectives, for example, gaining a Ph.D., promotion, tenure, travel expenses, prestige and power, are better explanations of why we carry out what we choose to call research.

The Ph.D. student is not looking for a large gap in knowledge—at least not in the States. He/she is just looking for something which may qualify for a research project.

Finally, it seems to me that we use empirical, positivistic research paradigms not in the interest of advancing science, but in the interest of selecting the "good" (i.e., the ones that best represent ourselves) and obscuring/hiding the "bad" (i.e., the ideas that may represent a challenge to us).

These are some of the major problems that have been raised during this conference where our research has been under scrutiny. However, apart from just raising problems, I feel that we have come quite a long way towards addressing some of these problems.

Results Achieved at the Conference

Although the general theme of the conference has been concerned more with processes rather than results, I will summarize the results under three categories: (1) definition of the IS research field, (2) the problem of effectiveness in our research, and (3) the ethical choice.

(1) **Definition of the IS research field**. Despite the fact that:
- we have to look at approximately 600 journals for articles that are relevant,
- there are at least 150 journals within the field of IS,
- the cross-disciplinary nature of our field of research,
- the diversity of interests,

it seems that this conference has been instrumental in helping us to take a step forward in the direction of defining IS as something different from computer science or engineering. For better or for worse, it must be seen as a social science discipline.

However, we have to admit that we have still failed to solve or perhaps even address the most fundamental issues relating to the wider role of IS in society. But at least we are now closer to

- understanding our research field in its own right,
- developing theories, methodologies and tools relevant to addressing these problems.

One of the ways that this can be achieved, apart from the traditional methods, is by the use of metaphors (see Boland 1985) and we can recognize the power of metaphors such as

Burt Swanson's	—	racehorse
Milton Jenkins'	—	sick pony
Frank Land's	—	jungle
Heinz Klein's	—	roadmap
Kendalls'	—	strobe

In this way we add a dimension to our methodologies by including pictures (see also Checkland 1981)

(2) **The problem of effectiveness in our research**. One of the crucial issues in evaluating research methodologies is how effective are they, i.e., given certain research objectives, how can we accomplish these objectives in the most effective way?

Accordingly, several speakers at the beginning of the conference argued for

- agreement on the "best" (most effective) methodology,
- a common framework (i.e., the one that is the easiest to communicate),
- categorization of methodologies,
- authorization/acceptance of certain methods (this still takes place today, as certain methods are not accepted for publication in reputable journals).

However, the main conclusion has been that of methodological pluralism (as already supported by Kuhn 1970). It is important here to distinguish between "bad" and "good" pluralism. "Bad" pluralism is when we degenerate into absolute relativism whilst "good'' pluralism opens up new perspectives.

Heinz Klein said that we need roadmaps. Perhaps we do in certain areas, but perhaps we also need assistance/support in order to

- prevent us taking the wrong route or help us not to
- dig up the old road and build a new one.

Here a change in perspective might have been useful as a starting point.

Furthermore, I would like to make the point that it is important to see the concepts of effectiveness in a wider context, and we must be prepared to accept that wider definition. We should therefore—in the interest of advancement of knowledge —support pluralistic approaches. At Milton Jenkins' school, Ph.D. students were requested to read 140 articles within the IS research field. In my opinion, this makes for a counterproductive effect and leads to conservatism in relation to producing "new methods" or "new perspectives."

Contrast the above strategy with that of the famous Dutch economist and Nobel Prize winner, Jan Tingergen, who defined research as reading two articles and writing one. Perhaps this is conclusive to furthering true creativity.

(3) **New ethics for IS research**. We cannot just be concerned with the problem of effectiveness, we must also ask what our research is used for.

   Heinz once said when asked to comment on advancement in artificial intelligence, "A lot of people are worried about machines getting more and more like human beings. I am more worried about humans becoming more like machines."

   In the same direction, Fromm states, "Machines cannot love. At best they can exchange part of their consciousness and hope for a good bargain."

   With reference to the above two quotations, I want to make the point that over and above the effectiveness issue, there is the ethical issue. A list of some of the more important ethical questions is given below:

   • Do we realize that control of technology is control of others?
   • Do we start with the social problem or with the technological options?
   • Do we understand and perceive the organization as a social system and not just as an information processing system?
   • Do we realize the broader scope of our research objects, the organizational change and organizational structure?
   • Who do we work for, who is the client?
   • Do our methods support the elite or to they bridge inequalities?
   • Do we treat human beings as subjects or objects in a research project?
   • Do we support emancipatory structures and processes?
   • How can we make explicit the researcher's implicit assumptions?

   Only if we give some thought to these ethical questions in the selection of research methods can we gain the most effective solutions.

Conclusions

After having cleansed ourselves in the purification process of debating our strategies/ methodologies, it is my impression that change will occur. An example of this is that a large number of participants in the workshop rallied behind Hans-Erik Nissen in his attempt to continue the discussion and obtain viable suggestions and concrete examples of "good" research within the field of IS. It is to be expected that a book will be published in approximately two years' time setting standards for "alternative but nevertheless acceptable strategies for good research within the field of IS."

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