

Research to go: a Critique of Contemporary Relationships Between Information Systems Research and Modern Capitalism

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Abstract. The IS discipline is often portrayed as a research field in crisis. The persistent discussion of rigor and relevance together with a commercialization of the research conducted has led to the proposal of design science as a unified methodology for conducting proper research within IS. We argue that the commercialization of the IS field is highly problematic since it is strongly biased toward corporate interests and, furthermore, that design science actually reinforces the hegemony of modern capitalism. Instead we question if the IS field is in fact in a state of crisis and by introducing the concept of multiplicity provide an alternative foundation for IS research.

Introduction

Collaborative research projects between universities and the corporate world is becoming an increasingly popular model for research funding (Schrøder 2006). Our own experiences with co-funded projects lead us to consider this a construction which by no means is unproblematic. It permeates even the most practical aspects of our everyday life as researchers and has in our opinion led to

a problematic reorientation in academia which we set out to scrutinize in this position paper.

A consequence of this new orientation is a simplistic utilitarian understanding of scientific contribution, where corporate interests define what constitutes a valid research agenda. Furthermore, the implications of the incursion of corporate interests into academic territory are an uncultivated area that deserves to be addressed more tenaciously. The consequence is that academia renounces its privileged position to define and articulate the problems of investigation. This raises the question: Are we as researchers doing consultancy at the cost of academic substance and have we thereby lost ourselves in an attempt to make research sufficiently 'relevant' for private funding?

The information system research area has had a continuous and lively debate on what constitutes good research. In particular, discussion on rigorous methods and relevant research has had significant impact on the discussions produced within the field. Recently design science has been proposed as a unified method for information systems research in to order meet the criteria of rigor and relevance (Hevner 2004, Walls et al. 1992, March and Smith 1995, Walls and Widmeyer 2004). However, the very idea of a unified method for a research area that are characterized by its diversity and broad acceptance of variety is not likely to be unified by any particular method nor specific research agenda. Design science, we argue, can be seen as a problematic relationship forged between academia and the corporate world, and constitutes a leading example of the risk of research becoming a commodity. A challenge that academia is facing today and is in dire straits of ending up as.

As a reaction we argue that good IS research is characterized first and foremost by a reflexive and critical orientation that includes contemplation of the relationships between research, technology and society. The strength of the IS research field is its ability to include multiple research agendas and approaches and any attempt to reject this multiplicity by enforcing an archaic idea of one unified method for conducting proper research reflects a perverted one-dimensional perception.

The paper falls into three parts: We commence with a account of the relations between IS research and corporate interests where we draw on examples from a Danish research funding context. This leads us to a critique of the design science paradigm which in our judgement lends scientific legitimacy, in the form of a theoretical foundation, to research activities which are hard to distinguish from consultancy. We conclude by presenting an outline of an alternative theoretical foundation for IS research which allows for multiple understandings and enactments of the field.

Relations between IS research and Corporate Interests

Research is not only an intellectual endeavor merely delimited by the systematic creativity of the mind, but constrained by diverse practicalities which demarcates the preconditions of the research field in question. The funding opportunities constitutes one such rather significant restriction since they exert a profound influence on the very definition of what can or cannot be a research problem worthy of further investigation.

Most universities depend heavily on external funding whether in the form of governmental or private grants. As a result substantial amount of projects are funded by an intermixture of universities, governmental agencies, and businesses, and subsequently most IS research projects are funded this way. To obtain the requisite resources academia has to articulate research topics in a manner which meets the requirements of the public agencies and appears attractive to potential business partners. In a Danish context these two conditions tend to converge.

An example hereof is the NABIIT program (strategic research in nanotechnology, biotechnology, and information and communication technology) under The Danish Council for Strategic Research which belongs to the Danish Ministry of Science, Technology, and Innovation. As the name indicates are funds granted to research projects with *strategic qualities*. This special strategic quality is defined as the combination of relevance, effects, and quality of the research (Danish Agency for Science, Technology, and Innovation 2006).

The material from The Danish Council for Strategic Research does not elaborate on the exact meaning of the four concepts. But when examining the evaluation criteria with more scrutiny a strong bias towards the interests of the corporate world emerges which is demonstrated by the following excerpts from a call for applications:

Funding is awarded to those research activities within nanotechnology, biotechnology and ICT that will contribute to identifying and developing future opportunities for commercial innovation and solutions to societal problems (The Danish Council for Strategic Research 2008:5).

With innovation and subsequent marketing in mind, there is also a need for funding for minor or small-scale significant strategic initiatives that can promote the development of the interdisciplinary competencies of research institutions and businesses (ibid:5).

It goes without saying that competing IS researchers have to mold and shape their applications to fit the evaluation criteria. We argue that a most unfortunate implication is that the very definition of what constitutes a valid research agenda is delimited by commercial interests: the research project must be constructed in a manner with a view to how the findings and results can be transformed into concrete profitable products or services.

The 3gERP (3rd Generation Enterprise Resource Planning – Strategic Software for Increased Globalization) is a collaboration between Copenhagen Business School, Said Business School Oxford University, ESADE Business School Barcelona, Department of Computer Science Copenhagen University, and Microsoft Business Solutions. Apart from the contributions from the partners it is funded by the Danish National Advanced Technology Foundation which belongs to the Danish Ministry of Science, Technology, and Innovation. In the application the key success factors of the project are stated as follows:

The main objective of the third generation ERP (3gERP) project is produce the research breakthroughs that will enable the development of a much more comprehensive ERP-system [...] (3gERP:6).

This is a huge but not totally unrealistic challenge. If it can be achieved, it is not unlikely that MBS could achieve a market share of 25% [...]. Furthermore, a granting of this application will contribute to ensuring the continued Danish position as a market leader in developing ERP-systems for SMEs (ibid:4).

We are quite horrified that an increased market share to Microsoft Business Solutions can serve as an argument for public funding. When valid research agendas are articulated in terms of market share or positions in the market, and potential for innovation and subsequent marketing it has overwhelming consequences for the research field in question. A whole range of research topics are left out in the dark when academia presents the right to define the legitimate research problems to the corporate world. When utility and profit dominates the discourse it furthermore has massive implications for the ways that we go about doing research: our methods, theory building, and dissemination styles. Most significantly it has an undesirable effect on the direction of the IS research field as a whole, when academic argumentation is substituted with capitalistic merchantism as a guiding principle.

Design Science, the Submissive Servant of Modern Capitalism

The current state of the IS field is often described as being in some sort of crisis due to the heterogeneous or fragmented nature of the field (Hirschheim and Klein 2003). Though a variety of explanations for this state of crisis is offered, the solution for solving the crisis is often presented as being some sort of shared property e.g. a common body of knowledge (ibid), a uniform and rigorous research approach (Hevner et al. 2004) or something else that, so to say, can be the glue of the field. We concur that the IS discipline is fragmented, however, we reject that this is a cause of a crisis of the field. Instead of considering the IS field

as a fragmented discipline in crisis, we understand the field as being intransitive which of course could be considered problematic, but – as we will argue just might be the very strength of the field. By intransitivity we draw on Mol (2002) who argues that “[intransitivity] is not an assemblage of objects that rank from small to large. There is no framing [...] big enough to contain all others – and thus form a ‘whole’.” and continues “In a transitive world where scale was fixed and hierarchical in character, this would never be: that A included B, while B was also inside A. But in a world of objects enacted that we live in such things happen. It is even possible that objects include one another while, simultaneously, in several ways, they are incompatible” (ibid:120-121). From this outset along with the argument produced in the previous section we find it relevant to present a critique of one particular effort, which presents itself under the title ‘design science’, to give the IS discipline a unified and coherent identity. Furthermore by relating this research approach to the broader discourse of what we call ‘research to-go’ we argue that design science is an unreflective and dubious way of conducting IS research.

Originally coined by Simon (1969) design science, as we understand it, is an attempt to combine the design of IT artifacts i.e. constructs, models, methods and instantiations (March and Smith 1995) that is produced in order to solve some business needs (Hevner et al. 2004) with the analytical character of a scientific endeavor. Hevner et al. describes design science as “[...] a problem solving process. The fundamental principle of design science research [...] is that knowledge and understanding of a design problem and its solution are acquired in the building and application of an artifact. That is, design science research requires the creation of an innovative, purposeful artifact [...] for a specified problem domain. Hence, thorough evaluation of the artifact is crucial [...]. ” (2004:82).

In (ibid) it is clear that not everything counts as a valid problem for information research: “The objective of research in information systems is to acquire knowledge and understanding that enable the development and implementation of technology-based solutions to heretofore unsolved and important business problems” (ibid:84). This is problematic since business needs and problems are one-sided and one-layered. By one-sided we mean that ‘business needs’ infuses a very specific set of values while marginalizing others. By one-layered we mean a rejection of a complex understanding of the problem and how it relates to a social world. Thus, what counts as a valid problem worthy of scientific investigation is not determined by research, but instead given as fact, a point of departure from where research can be shipped out. It is part of a neo-managerial discourse that favors and reinforces modern capitalism by accepting that business problems are in fact the problems, and the only problems, that IS research should address. Efficiency and productivity becomes the epitome of research which we consider as a degrading and perverted orientation for the IS discipline. This is not to say

that IS research should not consider these two dimensions, but if reduced to them will enslave the discipline as a whole to narrow minded, short lived corporate interests.

As we have just argued one problem with design science is its bias towards corporate interests. However, the agony continues; not only is design science biased it also stipulates that all parts the of IS research discipline should conform itself to the regime of design science: “Given the artificial nature of organizations and the information systems that support them, the design-science paradigm can play a significant role in resolving the fundamental dilemmas that have plagued IS research: rigor, relevance, discipline boundaries, behavior, and technology” (Hevner et al. 2004:98). Design science can therefore be regarded as an attempt to create a unified nomothetic paradigm for the IS discipline. Put otherwise, design science is forcing a transitive ontology upon the whole of IS. The implicit assumption that technology per se is neutral, and that its use alone determines the character of the technology is at best naïve. Feenberg (2002) argues that “[...] sociotechnical transformation cannot be conceived in terms of instrumental categories because the very act of using technology reproduces what is supposed to be transformed. Hence the well-known limitations of liberal management techniques such as job enrichment and quality circles. This is the *paradox of reform from above*: since technology is not neutral but fundamentally biased toward a particular hegemony, all action undertaken within its framework tend to reproduce that hegemony” (ibid:63).

So far we have argued that design science embed a unified nomothetic science that support a capitalistic hegemony. The last point of critique we will present is on the technological determinism that is inherent in design science. By insisting on a technical solution design science inscribe a means-end rationality into the artifacts produced and thereby suggest that the technology per se causes any change. Technology becomes a tool that can - so to say - fix the problem, or put more bluntly *any* problem. Bijker (1992) criticizes this understanding of technology for not only being too simplistic but downright wrong. Instead Bijker suggest that the dichotomy between the social and technical world is abandoned by applying the concept of ‘technological frame’ which he describes as “[...] a hinge between the social impact and the social shaping perspectives on technology” (ibid:98).

To sum up the critique given above: we argue that design science is trying to establish itself as a unified nomothetic science for the entire IS discipline; design science affords technological determinism and, finally, design science marginalizes alternative understandings of what constitutes a valid problem and thereby reinforces the hegemony of modern capitalism. One thing that perhaps needs some clarification is that design science by no means is the only attempt to establish a research approach with such characteristics. The reason why we feel compelled to criticize design science in particular is its fairly strong presence

within the IS discipline. Habermas (1965) presents a broader critique of nomological sciences and technical determinism that is aligned with the critique given above: “[T]he positivist self-understanding of the nomological sciences lends countenance to the substitution of technology for enlightened action. It directs the utilization of scientific information from an illusory viewpoint, namely that the practical mastery of history can be reduced to technical control of objectified processes. The objectivist self-understanding of the hermeneutic sciences is of no lesser consequence. It defends sterilized knowledge against the reflected appropriation of active traditions and locks up history in a museum” (ibid:239). We concur with Habermas and suggest that instead of predicting and explaining phenomena science – in our case the IS discipline – should provide a platform for enlightened actions.

Multiple Enactments of Knowledge Production within IS Research

So far we have presented a critique of the discourse that permeates research labeled IS/IT. We have argued that IS research is formed after the funding structures of the field, and that these structures support the capitalistic hegemony over society. Design science, we argue, is a leading example of the entangled and problematic relation between corporate interests and IS research. The boundaries between research and consultancy are increasingly becoming blurred which pose the question of the justification of IS research. Is what we are trying to accomplish within the IS discipline merely to lend a helping hand to corporate interests, and is this enough to qualify as research? This form of vulgar pragmatism is in our view unsatisfactory as a foundation for IS research. In order not to end up as pessimists we will now introduce an alternative to the foundation offered by design science. Inspired by the Dutch philosopher and ethnographer Annemarie Mol (2002) we will introduce the concept of *multiplicity* in order to make room for the various research approaches that still are present within IS research.

The world, Mol (ibid) argues, is enacted in many different ways for different purposes. Though these enactments differ they are not unrelated. On the contrary; to quote Mol: “The drawing together of a diversity of objects that go by a single name involves various modes of coordination” (ibid:84). Thus, the singularity of ‘information technology’ as a research topic is multiple. As described earlier in this paper Hirschheim and Klein (2003) diagnose the IS research field as being fragmented. We think that a more appropriate way to describe the condition of field by is through the concept of multiplicity. IS research is conducted in a variety of ways, for different purposes, and by different people coming from different cultures and institutions. Therefore it is understandable that Hirschheim and Klein (ibid) describe the field as fragmented, however they miss a critical

point: all of research conducted within the IS discipline relates itself to information technology in one way or the other and can thereby be coordinated not in the sense of totality, but in partiality. It is possible to relate and coordinate particular pieces of IS research for some purpose it all depends on the purpose and background for doing so. In this way the IS research need not to present itself as a nicely ordered research paradigm, but can instead be seen as a field where temporal stability can be achieved.

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