Meinung/Dialog

Relevance and Rigour – What are Acceptable Standards and How are they Influenced?

Robert Winter

In June 2007, the 'European Conference on Information Systems' (ECIS) completed its first, 15 year long journey across most European countries in Switzerland. Upon completing its first round, ECIS 2007 was taken as an opportunity to reflect the state of the IS discipline in Europe: From 1993 through 2007, the number of IS researchers and programmes has grown significantly, the IS discipline has established itself in between computer science and business/economics, and information systems are now acknowledged as important innovation drivers and sources of growth for companies, government and society. On the other hand, the discipline is facing discontinuities such as the e-hype and the subsequent downturn as well as large-scale transformations such as global competition in IT service provision. Furthermore, IS research is constantly challenged by the co-existence of fundamentally different research styles.

The ECIS 2007 motto "Relevant rigour – rigorous relevance" reflected this challenge. While being rigorous, relevance must not be lost. While being relevant, sufficient rigour must be applied to create reliable, transparent results. As a follow-up of the keynotes, panels and discussions in St. Gallen, we have chosen "Relevance and rigour – What are acceptable standards and how are they influenced?" as a debate & dialogue topic for the WIRTSCHAFTSINFORMATIK community.

Colleagues from different IS research communities, different backgrounds and with different working styles have been invited to share their thoughts on the following issues:

- Is there a common understanding about minimal rigour requirements for the design of relevant IT artefacts? Is there a common understanding about minimal relevance requirements for rigorous IS research?
- How far can rigour be substituted by relevance, if at all? How far can relevance be substituted by rigour?
- What factors influence the positioning of such minimal requirements? Are there IS research scenarios where certain relevance and rigour proportions are undisputed?
- How far should cultural factors and research stakeholders (agencies, sponsors) be allowed to influence the rigour vs. relevance problem?
- How should research funding be implemented in order to support the desired proportion of rigour vs. relevance?

We present contributions by (in alphabetical order)

- Prof. Dr. Richard Baskerville (Department of Computer Information Systems, Georgia State University, Atlanta, USA)
- Prof. Dr. Ulrich Frank (Chair of Information Systems and Enterprise Modelling, Institute for Computer Science and Business Information Systems (ICB), University of Duisburg-Essen, Germany)
- Prof. Dr. Armin Heinzl (Chair of Information Systems I, University of Mannheim, Germany)
- Prof. Dr. Alan R. Hevner (Information Systems and Decision Sciences Department, College of Business Administration, University of South Florida, Tampa, USA; National Science Foundation, Arlington, USA)
- Prof. Dr. John R. Venable (Head of School, School of Information Systems, Curtin University of Technology, Perth, Australia)

Not surprisingly, all contributors agree that good research should aim at rigor AND relevance. In order to determine an appropriate combination and create appropriate results, different approaches are proposed. The authors discuss the questions to whom research should be relevant, how relevance is linked to our understanding of science and our conceptions of truth, and which combinations of rigor and relevance may be appropriate in different situations. A main challenge for the combination of rigor and relevance may be the choice of a research method fitting the individual problem. The restrictions of current approaches for measuring relevance and rigor are pointed out, and approaches to develop appropriate indicators are outlined.

If you like to contribute to this debate, please submit your position statement (two pages maximum) to the editor-in-chief of WIRTSCHAFTSINFORMATIK, Prof. Dr. Hans Ulrich Buhl, University of Augsburg, Hans-Ulrich.Buhl@wiwi.uni-augsburg.de.

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Forms of Design Research and the Role of Rigor and Relevance

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Rigor and relevance are two distinct and generally independent characteristics of scientific research. Research projects may exhibit positive attributes of either, neither, or both. Nevertheless, we often regard pressures to imbue research with rigor or relevance as oppositional: delivering research that conforms to the norms of science and scholarship versus delivering research with direct applicability to business and practice [RoMa98]. The degree to which either of these characteristics is a goal in the research project is a matter of research design choice, and the degree to which these are oppositional depends on any resource limitations that prevent servicing both.

Beyond the economic constraints, these characteristics are not really trade-offs or substitutes. Certain kinds of research goals may restrict the importance of one of these characteristics. For example, research intended to produce an abstract theory may have little possibility of developing a strong relevance characteristic. Such a research goal may limit its designers to a focus on the rigor characteristic. The design choice is an outcome of the research goal.

Certain research approaches have been lionized for their potential to develop both characteristics, e.g., action research and design research. Taking design research as an example, it is possible to set research goals for design research that focus on an abstract theory. Some work on design theory has proved worthy without actually developing technological artifacts [WaWE92]. It is not automatic that design research is necessarily more relevant than other research forms. The issues of rigor and relevance apply to design research.

We lack a common understanding about minimal expectations for rigor and relevance in design research. This should not be surprising since we lack a common understanding of exactly what constitutes design research. Does any design process that results in an artifact constitute design research?

There are at least three distinct forms of design research. These regard the relationship between the design process and the research process. The first of these forms might be called "designing with research". It can be represented as a setting in which a designer must conduct research in order to properly compose the design. For example, a web site designer might be compelled to research ambient intelligence and brand theory in order to create the best design (August de los Reyes, creative director for the Windows Platform Core Innovation team, http://www.microsoft.com/design/People/Detail.aspx?key=august). For researcher-designers, the purpose of the research is to produce good designs. The research domain is the same as the design domain.

The second of these forms might be called "research into designing". It might be represented as a setting in which a researcher is studying designers, design outcomes, design processes, etc. For example, Pelle Ehn illuminated designers as operating in one of three design worlds [Ehn97]. For these researchers, the purpose of the research is to illuminate the design activity. The research domain is the designer and the design process, and this is usually different from the design domain.

The third of these forms might be called "designing as research methodology". Design science fits nicely in this category. It can be represented as a setting in which the researcher is involved in "doing" design as a means of making both scholarly knowledge and design knowledge. For example, Vijay Vaishnavi studied the evolution of requirements by designing an operations support system [VaBK97]. For these researchers, the purpose of the research is to illuminate the research domain. The research domain is often the same as the design domain.

The first form is likely to emphasize relevance because of the necessity of producing an immediately usable design. The need for rigor is less important because scientific evaluation is not usually invoked in evaluating the design process. The second and third forms of design research are more likely to invoke the characteristic of rigor, because scientific evaluation is more usual. The degree of relevance depends upon expectations for an immediately usable design as one of the outcomes.

A preoccupation with scientific rigor does not necessarily promise improved design. Design is not a purely scientific process. Architectural research distinguishes between analytical design and generative design [GrWa02]. Analytical design involves propositional understanding and fits squarely in the scientific paradigm, aligning nicely with scientific rigor. In contrast, generative design involves subjective feelings and aesthetics. Generative design centers creativity and might be thereby marginalized when scientific rigor dominates an evaluation. Along with creativity, stakeholders in the "art" of design productions may slip to the margins, along with their important cultural and societal values.

Research funding complicates both rigor and relevance characteristics by emphasizing one or the other. Funding by industry groups or commercial companies tends to overemphasize relevance without regard to rigor. Funding by government research agencies tends to overemphasize rigor without regard to relevance. Remembering that not all research projects necessarily embody the goals for both characteristics, it is somewhat naïve to suggest that each should move to a middle ground. Besides, such a cultural change is difficult and slow. Where research goals invoke both characteristics, perhaps a more appropriate strategy would be to seek research support from multiple funding sources, providing a project with the ability to economically balance the characteristics of rigor and relevance in its outcomes.

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Relevance of Research Implies Relevance to Researchers

Ulrich Frank

In essence, the debate on rigour versus relevance is based on two assumptions that are barely challenged: a) Research in Information Systems (IS) lacks relevance. b) Rigour and relevance represent an antagonism.

ad a) Numerous authors have complained about the lack of relevance in IS research (for an extensive overview see [Scha07]). Mostly, relevance is related to the value, a research contribution provides to business practice, mainly by helping with solving critical problems. Missing relevance is regarded by many as the reason for the discipline's poor recognition in business practice and its insufficient exchange with IS professionals. Some authors relate relevance also to teaching. They criticize that IS research results are not suited and in fact not used for IS teaching. Further concerns are related to the presentation of research results in top-tier journals, which is regarded as repulsive to both students and IS professionals.

ad b) It is a common belief in IS that the widely disapproved lack of relevance is caused by the discipline's emphasis on rigour. It suggests the "conclusion" that relevance is sacrificed for high quality research. Such a conclusion is misleading for various reasons. Firstly, the modern conception of science and its appreciation in western societies is based on the conviction – supported by overwhelming evidence – that high quality research is relevant in many respects. Secondly, the rigorous application of a specific method is not sufficient for producing impressive research results – it may even impede them, if it is not appropriate for a particular research subject. In fact, many authors complain about the poor quality of IS research (for an overview see [Fran06]). So far, IS has hardly produced significant results – or, as Kavan puts it, its contributions are often "intuitively obvious" [Kava98].

Hence, the debate on the alleged conflict between rigour and relevance is an indication of the discipline's multiple failures – its failure to produce considerable research results, its failure to develop a coherent profile, which would foster a better identification of researchers with their discipline and its failure to develop an inspiring research agenda that is not determined by a widely unloved research method. It also reflects that IS is caught in a trap. Although the behaviourist paradigm is questioned by many, it constitutes an impression of scientific excellence, created through "compulsive handwashing in statistical procedures" [McCl85] and through prestigious journals. Hence, it helps to build legitimacy, which is especially important for a field that "continues to be haunted by feelings of inadequacy." [LyKi04].

Different from IS, relevance was never regarded as a problem in Wirtschaftsinformatik (WI). Often, research projects are focussed on developing artefacts, such as methods, models or architectures – in many cases in cooperation with companies. The evaluation of this construction-oriented approach in WI has an ambivalent outcome. On the one hand, there are clear indications of success: Relatively large amounts of industry funds demonstrate the appreciation of WI research in practice. This is also the case for the continuing high demand for WI graduates. Additionally, the topics of research projects can usually be related to the WI curriculum. This contributes to the unity of research and teaching, which is appreciated much at German universities. On the other hand, it is apparent that this kind of relevance is not sufficient. For establishing a convincing profile as a scientific discipline, WI needs to clearly distinguish itself from consulting or software companies. Such a profile would require a suitable methodological foundation.

In recent years, the so called "Design Science" paradigm has gained remarkable popularity. It promises to enrich the dissatisfactory research agenda of IS and to support WI with a suitable method. The guidelines proposed by [HMPR04] explicitly include the demand for relevance. In accordance with many other authors they suggest to regard a problem as relevant, if it actually exists in practice. This notion of relevance is certainly also compliant with many construction-oriented research projects in WI. Emphasizing that there is no necessary conflict between relevance and high academic standards is certainly a merit of Hevner et al. Unfortunately, they do not provide a convincing methodological foundation. Instead they mainly suggest applying the correspondence theory of truth – which is at the core of the behaviourist paradigm – to design-oriented research.

After recapitulating some aspects of the rigour vs. relevance debate, I would like to suggest a different viewpoint. It can be summarized in the following assumption: In order to be relevant, research needs to be relevant to researchers. This is not meant with respect to career considerations, but with respect to scientific interest and recognition – or, to put it simple: research is relevant, if it produces an outcome that is suited to make smart people smarter. Realizing such a request requires an appropriate conception of science – otherwise we could not differentiate the specific relevance of scientific research from any other form of relevance. I think that there are three essential postulates that characterize scientific knowledge: abstraction, originality and justification. Rigour would then refer to thoroughly testing research results against these postulates. Scientific recognition is not just aimed at describing single instances, e.g. a particular company or a specific information system. Instead, it is focussed on more general features or patterns that apply to a whole range of instances. Abstraction is not restricted to commonalities of actually existing instances – hence, relevance is not restricted to actually existing problems. Instead, abstraction can be aimed at transcending the world as we perceive it, resulting in the creation of not yet existing, but possible shapes of reality. A scientific contribution is linked to the claim of novelty: Only, if a research result is at least in part original, it may qualify as scientific. At the same time, originality is associated with the claim for superiority: New knowledge should be superior to existing knowledge. This requires comparability, i.e. among other things a precise language. Scientific justification is aimed a producing evidence for the truth of a proposition. However, there are different concepts of truth, which suggest different procedures to test the truth of a proposition. Furthermore, the construction of advanced IT artefacts and corresponding action systems can often not be tested against truth. Instead, there is need for other concepts, such as adequacy, to evaluate this sort of scientific knowledge. This recommends giving up the idea of one or two prevailing research methods and replacing it by the problem-specific configuration of research methods [Fran06].

It may seem that the proposed conception of relevance accounts for the perspectives of researchers only, excluding other stakeholders such as students or companies. This is not my intention at all. Firstly, the focus of our research – designing, applying and managing information systems – is application-oriented by definition. Secondly, I assume that emphasizing substantial abstractions is beneficial to both students and companies. Abstraction stresses the need for elaborate concepts that can be applied independently from particular technologies. It also recommends going beyond existing shapes of IT and patterns of their application. IS professionals, who are interested in considering essential aspects of their work as well as future challenges, should appreciate this. It also helps our graduates to act successfully in a world of ever changing fads and fancy labels. Of course, abstraction in our field does not mean to ignore reality. Only those, who have interacted with the research subject – IT artefacts and those who use them in practice – will be able to discover and evaluate general patterns of understanding.

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Rigour AND Relevance: Determining Positions on Two Independent Scales

Armin Heinzl

1. Is there a common understanding about minimal rigour requirements for the design of relevant IT artefacts? Is there a common understanding about minimal relevance requirements for rigorous IS research?

I personally do not see yet a common understanding about minimal rigour requirements for the design of IT artefacts. This is not surprising since constructive research, i.e. design science, is rather new to the field and represents only a small portion of international IS research. But I think Hevner et al. have addressed this topic three years ago quite well and created a better awareness. Thus, I believe that a basic common understanding will further develop. One challenge is to overcome the diversity of the IT artefacts. I think it makes a difference whether you want to evaluate the rigour of constructs, models, methods, and tools. Constructs and tools are probably most "easy" to evaluate. For methods and models, it will be more complex, of course. Furthermore, existing evaluation methods differ in terms of scope and effort for conducting them. Thus, I see the need for developing and using methods which offer a minimum level of standardization.

I also do not see yet a common understanding about minimal relevance requirements for rigorous IS research. According to my experiences, relevance issues have not been systematically addressed on a large scale. At max, you will be asked as a reviewer how relevant a submitted paper or project proposal is. But relevance needs to be addressed much more specific than a simple Likert scale. This is surprising since empirical IS research, for instance, has developed rigour standards over the past two decades. I interpret this pattern in a way that rigour was central and relevance was somewhat random. I personally know many colleagues who criticize this development. It is very important that we do not lose sight of the relevance criterion.

2. How far can rigour be substituted by relevance, if at all? How far can relevance be substituted by rigour?

The substitution line between rigour and relevance and vice versa is a strategic decision of every single researcher. To me, it is not a question of substitution on one scale, with rigour versus relevance as the antipodes. This would mean that rigour and relevance are mutually exclusive. They are not. A piece of research which is rigorous would be largely irrelevant. This is too simplistic. Rigour and relevance are more likely to be two independent scales. Both range from 'low' to 'high'. In this vein, a highly rigorous piece of research could also be highly relevant. For this reason, every researcher has to determine which position on these two scales she or he would like to opt for. A base researcher might go for high rigour and open relevance. An applied researcher may strive for high rigour and high relevance. Both positions are absolutely acceptable.

3. What factors influence the positioning of such minimal requirements? Are there IS research scenarios where certain relevance and rigour proportions are undisputed?

It is really difficult to answer what factors influence the positioning of such minimal requirements. I think the constituting factors of a discipline as well as the national research culture play a crucial role. If – for example – European researchers experience problems in publishing relevant research which is not considered as rigorous, they have two options: They could either try to adapt to external quality standards or they can attempt to implement their own standards. If the US community thinks that IS research is in crisis because practitioners say it is not relevant or because the IT in IS research is invisible, they could opt for more constructive and relevant research. Which force will become dominant will be driven by the fact how research output will be utilized ("bought") by the society and the corporate world. In this context, teaching and innovations are highly related to research output as well.

4. How far should cultural factors and research stakeholders (agencies, sponsors) be allowed to influence the rigour vs. relevance problem?

As indicated, cultural diversity will affect the rigour vs. relevance problem. It produces different variants of characteristic archetypes from which other cultures can learn. The foundation and formation of the IIITs (Indian Institutes of Information Technology) is one example, the development of the German "Wirtschaftsinformatik" another one. Both indicate how different cultures deal with the fundamental issue. Every national research culture has its own constituents that will determine the position of the relevance and rigor scales. I see culture not as an inhibitor of common standards but rather as good examples which will foster diversity ("new genes") and competition.

Research agencies should ensure that all fundamental archetypes will be addressed. I am quite satisfied with the process from a German perspective regardless of the fact, that the number of proposals increases and the likelihood of funding decreases as a statistical consequence. The German Science Foundation (DFG) focuses on foundational research. It ensures high rigour research regardless the practical exploitation of results. I call this archetype "very high rigour/open relevance". Only academics participate in the evaluation process. But standards between disciplines may vary significantly since every discipline has developed its own philosophy over the past 50 years. Nevertheless, the focus is on rigour and it is worth taken the effort of writing a proposal.

The German Ministry of Education and Research (BMBF) – for instance – focuses on applied research. Practitioners may be part of the review process as well. Companies have to participate in the funding of the research projects. They only will do so, if they consider the proposed projects as valuable which means, the projects must be relevant to them. I call this archetype "high rigour/high relevance". The European Union follows a similar model but focuses on cross-national enrichment and critical mass deployment.

Those academics that look for funds for their research from the corporate world follow the archetype "variable rigour/very high relevance". You will find high rigour when products and services of the corporate partner are directly affected. You may find a not so high rigour if speed matters. Nevertheless, the research subject will be of very high relevance. Otherwise, the companies will not have an incentive to contract directly with a professor or an institute.

As indicated above, as long as the entire spectrum is covered, any archetype can be potentially served. That means there are incentives in the research arena for stimulating diversity, competition, and quality.

5. How should research funding be implemented in order to support the desired proportion of rigour vs. relevance?

It can not be assumed that research funds will be equally distributed between the archetypes, for which I gave three examples. Depending on the research policies of a nation, the funds may be relocated between the models. Most important is a high quality and transparent review process. Furthermore, key stakeholders need to be integrated and frequently exchanged in order to avoid clan solutions. The more relevance shall be addressed, the more practitioners should be integrated.

Summing up, I am convinced that the relative position of researchers on the rigour and relevance scales can be best influenced through both, non-monetary and monetary incentives. The non-monetary incentives relate to the research paradigm in a discipline and the publication process which roots in its social and cultural environment. The spectrum of funding and publication opportunities should not be one-sided, but rather covering different profiles of the relevance and rigour scales. The quality of the feedback mechanism is also important. In case of transparent and constructive feedback, academics have strong incentives to write a paper or a research proposal since feedback helps to improve the own thoughts – no matter whether it gets accepted or rejected. Monetary incentives relate to direct funding and value increases through relevant and/or rigorous publications which make the researcher attractive for new appointments. (Re-)Building the consciousness around this issue – like this section does – is a necessary but not sufficient condition in order to (re-)vitalize the fundamental positioning problem on the relevant and rigour scales.

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Rigor and Relevance Viewed as a 2 × 2 Matrix

Alan R. Hevner

The design science research paradigm values the presence of both relevance and rigor as essential ingredients of an exemplary research project. While specific projects may vary in amounts of relevance and rigor, however measured, both must be present in some degree for the design research results to be considered valid and useful. For the purposes of this debate, let me briefly describe my definitions of relevance and rigor in design research and then state a position that calls for any design research project to have clearly articulated goals of utility in an application environment and contribution to a scientific knowledge base.

Consideration of relevance initiates design science research within an application context that not only provides the requirements for the research (e.g., the opportunity/problem to be addressed) but also defines acceptance criteria for the ultimate evaluation of the research results. Does the design artifact improve the application environment and how can this improvement be measured? The outputs from the design science research effort must be returned into the environment for study and evaluation in the application domain. The results of the field testing will determine whether additional iterations of design refinements are needed. The new artifact may have deficiencies in functionality or in its inherent qualities (e.g., performance, usability) that may limit its utility in practice. Another result of field testing may be that the requirements input to the design research were incorrect or incomplete with the resulting artifact satisfying the requirements but still inadequate to the opportunity or problem presented. By definition, effective design research must result in a design artifact that has measured utility in an application environment. Thus, relevance is assured in a successful design research project. Note that this requirement for relevance may not be a prerequisite for other research paradigms. For example, natural science research may study phenomena to understand its truth with no apparent relevance to any application environment.

Consideration of rigor in design research is based on the researcher's skilled selection and application of the appropriate theories and methods for constructing and evaluating the artifact. Design science research is grounded on existing ideas drawn from the domain knowledge base. Inspiration for creative design activity can be drawn from many different sources to include rich opportunities/problems from the application environment, existing artifacts, analogies/metaphors, and theories. Additions to the knowledge base as results of design science research will include any additions or extensions to the original theories and methods made during the research, the new artifacts (design products and processes), and all experiences gained from performing the research and field testing the artifact in the application environment. It is imperative that a design research project makes a compelling case for its rigorous bases and contributions lest the research be dismissed as a case of routine design. Definitive research contributions to the knowledge base are essential to selling the research to an academic audience just as useful contributions to the environment are the key selling points to a practitioner audience.

Drawing from the above descriptions of relevance and rigor, I posit that a design research project must embody sufficient levels of relevance and rigor to make a convincing case that 1) the resulting design artifact will have utility in the application environment and 2) the research will make a scientific contribution to the domain knowledge base. In support of the first requirement, I claim that design science research is essentially a pragmatic discipline. Pragmatism is a school of thought that considers practical consequences or real effects to be vital components of both meaning and truth. Design science research is essentially pragmatic in nature due to its emphasis on relevance; making a clear contribution into the application environment. In support of the second requirement, extending the content of the knowledge base is what separates design research from the practice of routine design. Together, it is the synergy between relevance and rigor and the contributions to both the application domain and the scientific knowledge base that define exemplary design science research.

In my current assignment at the U.S. National Science Foundation (NSF) I work with research proposals in the directorate of Computer and Information Science and Engineering (CISE). A majority of CISE research projects use a design science research paradigm. Since its beginnings in 1953, the NSF has struggled with distinctions between basic science and applied science in its awarding of research funds to academic researchers. Does the practical utility of a result necessarily make the research project applied science? Can a research project effectively balance goals of fundamental scientific understanding with considerations of the usefulness of the resulting artifacts? Should NSF fund primarily basic research, applied research, or some combination of the two? This debate has been strongly influenced over the past 55 years by NSF's relationships with the U.S. Congress as its funding source, industry as a collaborator and user of research results, and the general public for its social good.

A recommended book [Stok97] studies the history of this debate in NSF and suggests transforming the question from a one-dimensional picture (basic research ↔ applied research) to a two-dimensional, 2 × 2 matrix with "Considerations of use?" (roughly, relevance) on the x-axis and "Quest for fundamental understanding?" (roughly, rigor) on the y-axis. The upper right quadrant with high relevance and high rigor is titled Pasteur's Quadrant in recognition of his fundamental research in microbiology that had immediate and life-saving uses in the areas of food processing and preservation. While the upper left quadrant (high rigor, low relevance – named Bohr's Quadrant) and the lower right quadrant (high relevance, low rigor – named Edison's Quadrant) contain important research for funding, my contention is that true design science research belongs in Pasteur's Quadrant. Open questions remain as to how levels of rigor and relevance are measured in a specific research project and whether such levels can be predicted during research design or can only be judged on the basis of research results. These are importance issues for us in Information Systems to address as we strive to better understand how to perform rigorous and relevant design science research and how to attract external funding to our research.

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Relevance *vs.* Rigour or Relevance *and* Rigour? Contingence and Invariance in Standards for IS Research

John R. Venable

Introduction

Attendance to and standards for relevance and rigour are critical for any research field. Any field should strive to be both highly relevant and highly rigorous in its research. If a field, especially an applied field like IS, does not study topics that have some promise of applicability to practice (relevance), it is unlikely to be funded or listened to. If a field does not produce research results that are correct (truthful) and credible (or worse, publishes work that is later proven to be false), then it risks becoming untrustworthy and also not funded or listened to. Ultimately, achieving relevance and rigour affect the reputation of a field and its long-term existence. Having and diligently applying appropriate standards and guidelines for decision-making about acceptable relevance and rigour is important for any field. But what standards should we have for the field of IS or Informatics? What contingent factors, if anything, about our field influence acceptable levels of relevance and rigour in our research?

Background Matters

At the outset, it is worth considering what is meant by relevance and rigour in (slightly) more detail. It is first important to consider the question "Relevant for whom?" [CrYo07] consider this question in detail as it relates to IS practice, developing a framework of the ecology of practice, from the individual level through the societal/state levels. A second important question is "What are the characteristics of rigour and relevance?" [BeZm99] identified four aspects of relevance (to practice) for IS Research: interest, applicability, currency, and accessibility. [MaMa07] expand on the concepts of both rigour and relevance, identifying three characteristics of "good" research: Credible, Contributory and Communicable. I refer the reader to the above papers for the details.

However, besides relevance to IS practice, I suggest here that we should also consider relevance to other IS researchers, in the context of the continual, iterative process of building up reliable human knowledge. I further suggest that what is not immediately relevant to IS practice may be quite relevant to at least some IS researchers, as long as they can see the links to their line of research and some (future) relevance to practice. I also suggest that what is an adequate level of rigour for a practitioner may not be adequate for an IS researcher. A practitioner might be happy that there is an 80% chance that something highly relevant is true, whereas IS researchers usually insist on a much higher standard, e.g. 95% or 99%, where such measurement is possible. Thus, standards for both rigour and relevance depend (are contingent) to some extent on the reader.

Discussion

Whether considering research in design science, system development, IS management, empirical studies of IS practice, there is little or no common understanding, let alone standards about minimal requirements for either rigour or relevance in IS research.

What we have instead is general agreement that both rigour and relevance are desirable, but fairly widely varying, largely unstated (tacit), and vague criteria for both. There are hardly, then, "standards", except in the sense that they are individually held by the various gatekeepers (editors, reviewers, etc.) in our field. Our highest quality journals and conferences uphold higher standards of rigour and/or relevance, while lower quality research outlets may settle for less relevance and/or rigour. Different journals and conferences then develop different reputations for quality, which vary over time. For example, the Communications of the ACM has taken a stance of trying to improve its relevance, but has also suffered of late in various journal rankings. The Journal of Information Technology (JIT) and the Journal of Information Technology Theory and Application (JITTA) on the other hand have been steadily rising in rankings and citation analyses.

Many in our field also have a sense that these rigour and relevance must trade off against each other. I disagree. My position here is that the resources (including effort) required to pursue both highly relevant and highly rigorous research *may* limit the possibilities and force a trade off, *but not necessarily.* Achieving a high(er) level of both relevance and rigour is often possible through clever and artful research design, a task for which people vary considerably in their ability.

While high rigour and relevance are desirable, even with artful research design, resource constraints, the size and complexity of the research domain, or other factors may limit our ability to achieve them. The extent to which relevance can be substituted for rigour (and vice versa) is contingent on several factors, including the audience for the research and the "state of the art" of the area of knowledge.

Considering the audience, as noted above, a practitioner might accept relevant knowledge that has only an 80% probability of being true. This would especially be true for a highly novel and practicable piece of knowledge. Thus, for a practitioner (e.g., published in a practitioner journal or magazine), relevance can be substituted for rigour. Certainly there is little need to describe research methods and efforts to establish rigour in a paper for practitioners. However, I think there should still be some minimum (invariant) standards for rigour in what IS researchers publish. In particular, issues of validity and clarity of concepts are still essential. It would also be helpful for the researcher to assist the practitioner to judge the reliability of the knowledge.

For research published for researchers (in research journals), the opposite may be true. Rigorous establishment of knowledge (e.g. replication studies or theory testing research) may not be relevant to practitioners, but should be relevant to IS researchers. In research journals, clear rigour (including validity and clarity) are still essential.

In addition to the audience, it is also worth considering the "state of the art" of the area of knowledge to which a research contribution is being made. Research that is "theory building" in an area where knowledge and prior research is significantly lacking may tolerate significantly less rigour, in the hope that future research will improve on the rigour and test (and possibly refine and extend) the new concepts.

On the other hand, research that is "theory testing", especially in an area that already has significant research, is expected to be more rigorous. Such research is likely less relevant to IS practice because practice will put more emphasis on the newness of knowledge for it to be relevant. Theory testing research is still relevant to IS researchers, who are interested in improving the reliability of the knowledge so that they can reliably build other knowledge upon it. Note that when theory testing or replication studies disprove a previously published research result, it should be relevant to *both* researchers and practitioners.

Several research criteria can (or should, in my opinion) be considered as undisputed, absolute minimum (i.e. invariant) requirements, regardless of the above factors that influence minimum requirements. First, with respect to relevance, in an applied field such as IS, at a minimum, the topic and results should be such that they can be considered to be relevant now or they can potentially lead to relevant topics and results in the future. Basic research is the province of other fields. Second, with respect to rigour (and to some extent relevance), standards for validity of the research and its constructs, clarity, etc., should apply whether publications are targeted at practitioners or other researchers. Third, with respect to rigour, one clearly undisputed case is where human safety concerns are at stake (where death or serious injury are possible consequences). Like medical research, very high levels of rigour are required in that case.

Agencies and sponsors, such as governments, industry organisations, or businesses, have legitimate interests in pursuing particular topics (relevance) or in seeking particularly rigorous evidence in certain areas. Researchers are free to pursue or not pursue research and the funding that goes with it. Those funding or managing research are entitled to set a research agenda and may set targets for rigour or relevance and make decisions about proposed research accordingly. Where criteria for relevance (e.g. a topic) or rigour are set by research sponsors, researchers who agree to receive the funding should meet (or exceed) those criteria. Note that this does not guarantee that results will be publishable in the wider IS research community.

Summary

This essay has taken the position that there is no one desired proportion or balance of rigour and relevance and that the levels of relevance and rigour that the IS research community should require are contingent on various factors, such as the target audience of the resulting publication and the state of the art in the research domain. However, minimum, invariant standards of relevance (in any applied field such as IS), of validity and clarity about the resulting concepts and knowledge, and about rigour when there are safety concerns should always be met.

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Mitteilungen des GI-Fachbereichs Wirtschaftsinformatik

Neuer Arbeitskreis Energieinformationssysteme

Zur besseren Vernetzung der *Scientific Community* und des forschungsinteressierten wirtschaftlichen Umfelds im Themenbereich „IT in der Energiewirtschaft“ wurde kürzlich der Arbeitskreis „Energieinformationssysteme“ in der Gesellschaft für Informatik (GI) geschaffen. Hier bietet sich engagierten Arbeitsgruppen, aber auch einzelnen Akteuren, eine Plattform, um Problemfelder und spezifische Herausforderungen der Energiewirtschaft mit Konzepten, Methoden und Prototyprealisierungen aus der Informatik und der Wirtschaftsinformatik anzugehen. Der Arbeitskreis hat sich vor allem zum Ziel gesetzt, attraktive Fragestellungen aus dem Bereich Informationssysteme – also weniger die eher technischen Aspekte von Anlagen und Versorgungsnetzen, deren Verständnis aber unerlässlich ist – aufzugreifen und hinsichtlich der Anwendbarkeit in der Energiewirtschaft zu untersuchen. Ein breites Themenspektrum, wie zum Beispiel Daten- und Architekturmodelle, Anforderungsanalyse, Simulation und Entscheidungsunterstützung, jeweils mit Fokussierung auf die Energiedomäne, steht dabei voraussichtlich im Mittelpunkt der Arbeit.

Wenn Sie Interesse an der Mitwirkung im Arbeitskreis haben, so schauen Sie bitte unter http://www.energieinformationssysteme.de nach. Dort erhalten Sie nähere Informationen und Kontaktadressen. Als erste Veranstaltung in 2008 wird der GI-Arbeitskreis einen Track zum Thema „IT in der Energiewirtschaft“ auf der „Multikonferenz Wirtschaftsinformatik 2008“ vom 26. bis 28. Februar in München durchführen. Weitere Informationen finden sich unter http://www.mkwi2008.de, als Mailkontaktadresse zum Track steht mkwi08@offis.de zur Verfügung. Und auch für die WI 2009 in Münster ist eine Veranstaltung des GI-Arbeitskreises „Energieinformationssysteme“ in Vorbereitung.

Prof. Dr. Hans-Jürgen Appelrath
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Vorläufiger Sprecher des AK

Aus den Hochschulen

Dr. Carsten Felden, Jahrgang 1969, der bisher die Stelle eines Wissenschaftlichen Assistenten am Fachbereich Betriebswirtschaft der Universität Duisburg-Essen, Campus Duisburg, bekleidete, hat einen Ruf auf die Professur für Allgemeine Betriebswirtschaftslehre, insbesondere Informationswirtschaft/ Wirtschaftsinformatik an der Fakultät für Wirtschaftswissenschaften der TU Bergakademie Freiberg angenommen. Seine Forschungsschwerpunkte sind Business Intelligence, Business Process Management und IT in der Energiewirtschaft (http://www.wiwi.tu-freiberg.de/wi).

Dr. Jörn von Lucke, Jahrgang 1971, ist am 2007-01-22 vom Senat der Deutschen Hochschule für Verwaltungswissenschaften Speyer habilitiert worden und hat die Venia Legendi für die Fächer Verwaltungs- und Wirtschaftsinformatik erhalten. Der Titel seiner Habilitationsschrift lautet „Hochleistungsportale für die öffentliche Verwaltung“. Derzeit beschäftigt er sich am FOKUS Fraunhofer Institut für Offene Kommunikationssysteme in Berlin mit dem Sprachportal „Service 115“, mit der Umsetzung der EU-Dienstleistungsrichtlinie und ausgewählten Hochleistungsportalen für den öffentlichen Sektor (http://mitglied.lycos.de/ Lucke).

Frau PD Susanne Robra-Bissantz, Jahrgang 1965, die an der Universität Erlangen-Nürnberg im Fachbereich Betriebswirtschaftslehre die Stelle einer Akademischen Oberrätin bekleidete, hat den Ruf auf die Professur für Wirtschaftsinformatik, insbesondere Informationsmanagement an der Carl-Friedrich-Gauß-Fakultät der Technischen Universität Braunschweig angenommen. Ihr Forschungsschwerpunkt sind kundenorientierte Dienstleistungen im E-Business.

Dr. Thomas Schwotzer, Jahrgang 1969, der bislang als Leiter der Forschung und Entwicklung der neofonie GmbH in Berlin tätig war, hat einen Ruf auf die Professur für Webbasierte Anwendungen im Fachbereich Wirtschaftsinformatik der Fachhochschule Brandenburg in Brandenburg an der Havel angenommen. Seine Forschungsschwerpunkte sind mobile P2P-Anwendungen, Topic Maps und Semantic Web (http://fbwcms.fh-brandenburg.de/sixcms/ detail.php?id=3912).

Prof. Dr. Veronika Thurner, Jahrgang 1969, die bislang als Professorin an der Fachhochschule Landshut, sowie als Beraterin bei der ARS Computer und Consulting GmbH in München tätig war, hat einen Ruf auf die Professur für Softwareentwicklung für betriebliche Informationssysteme in der Fakultät für Informatik und Mathematik der Fachhochschule München angenommen. Ihre Forschungsschwerpunkte sind Modellierung von Geschäftsprozessen, Vorgehensmodelle und Software Engineering (http://www.cs.fhm.edu/~thurner).

Prof. Dr. Ulf J. Timm, Jahrgang 1966, der bislang bei der IBM Deutschland GmbH im Bereich Global Business Services – Strategy & Change als Managing Consultant tätig war, hat einen Ruf auf die Professur für Allgemeine Betriebswirtschaftslehre und Wirtschaftsinformatik im Fachbereich Maschinenbau und Wirtschaftsingenieurwesen an der Fachhochschule Lübeck angenommen. Seine Forschungsschwerpunkte sind Erfolgsfaktoren im (IT-)Projektmanagement, Electronic und Mobile Business sowie IT im Dienstleistungsbereich (http://www.fh-luebeck.de/content/04_03_02/4/577.html).