Towards a Pluralistic Conception of Research Methods in Information Systems

Frank, Ulrich

In: ICB Research Reports - Forschungsberichte des ICB / 2006

DOI: https://doi.org/10.17185/duepublico/47166

URN: urn:nbn:de:hbz:464-20180925-112122-3

Link: https://duepublico.uni-duisburg-essen.de/servlets/DocumentServlet?id=47166

Source: ICB-Research Report No. 7, December 2006; revised version

ICB
Institut für Informatik und
Wirtschaftsinformatik

Ulrich Frank

Towards a Pluralistic Conception of Research Methods in Information Systems Research

7

ICB-RESEARCH REPORT

ICB-Research Report No.7
December 2006
revised version

UNIVERSITÄT
DUISBURG
ESSEN

Die Forschungsberichte des Instituts für Informatik und Wirtschaftsinformatik dienen der Darstellung vorläufiger Ergebnisse, die i. d. R. noch für spätere Veröffentlichungen überarbeitet werden. Die Autoren sind deshalb für kritische Hinweise dankbar.

The ICB Research Reports comprise preliminary results which will usually be revised for subsequent publications. Critical comments would be appreciated by the authors.

Author's Address:

Ulrich Frank

Institut für Informatik und
Wirtschaftsinformatik (ICB)
Universität Duisburg-Essen
Universitätsstr. 9
D-45141 Essen

ulrich.frank@uni-due.de

Source of Supply:

Institut für Informatik und
Wirtschaftsinformatik (ICB)
Universität Duisburg-Essen
Universitätsstr. 9
45141 Essen

Email: icb@uni-duisburg-essen.de

ISSN 1860-2770

"… the object of thought becomes progressively clearer with this accumulation of different perspectives on it."

Peter L. Berger and Thomas Luckmann

"Innocence is the child, and forgetfulness, a new beginning, a game ..."

Friedrich Nietzsche

Abstract

There has been an ongoing debate about the use of research methods in Information Systems Research (ISR). Despite the international dominance of the behaviourist approach, there is a remarkable number of researchers who propagate alternative methods, such as hermeneutic approaches or the so-called 'Design-Science' approach. This report is based on the assumption that the variety of research topics and objectives of ISR cannot be covered in a satisfactory way by one method. Instead, it is claimed that there is need for the configuration of methods according to individual research designs. For this purpose, the report is focussing on the peculiarities of ISR in order to develop the requirements a research method should serve. Subsequently, existing methods, such as the behaviourist, the hermeneutic or the 'Design Science' approach are evaluated with respect to these requirements. To develop a common conception of science that fits all disciplines and can be adapted to serve the peculiarities of ISR at the same time, the report gives an overview of selected approaches in philosophy of science. Against this background, a conceptual framework is developed that guides the configuration of individual research methods. It includes the core concepts that constitute the parameters of configuration and supplemental criteria to support configuration decisions. Finally, the framework is adapted to serve as a conceptual foundation for the unified documentation of research results. It fosters comparability of research contributions across the spectrum of possible research methods and contributes to the reconstruction of scientific progress.

Table of Contents

Figures

Tables

1 Introduction

In Information Systems (IS), the so called positivist paradigm is still dominant as far as reviewing criteria in top tier journals are concerned. At the same time, there are a remarkable number of researchers and scholars who do not feel comfortable with the positivist paradigm. This dissatisfaction is accompanied by concerns about the usefulness of research results with respect to guiding decisions in practice ("rigour vs. relevance"). It is probably not too daring to state that the discipline is still in a state of search. It is searching for a coherent profile, which would foster a better identification of researchers with their discipline. It is searching for a more inspiring research agenda that is not determined by a widely unloved research method. It is searching for research results that are suited to improve the discipline's recognition in practice. For this search to be successful, it is required to develop alternative research methods that help to shake off those aspects of the positivist paradigm, which are perceived as a burden by many.

In *Wirtschaftsinformatik* (WI), the discipline of research on information systems in German speaking countries, the situation is similar and different at the same time. It is similar because WI is in need of supportive research methods as well. This is, however, for different reasons. In WI, research is mainly aimed at supporting the construction, deployment and utilization of information systems, including the design and test of innovative artefacts. This comprises the development of conceptual models, of information system architectures and of prototypes. This approach has rewarded the discipline with a respectable recognition in business firms – both for research results and for the qualification of its graduates. However, research in WI has often been performed in a rather pragmatic fashion with no explicit deployment of research methods. With the increasing globalisation of research on information systems, WI faces the challenge to compete in an international arena that is dominated by IS with its specific emphasis on a positivist research method. In this situation, WI has two choices. It can take IS as a paragon and – as a consequence – give up its previous profile or it can try to promote its own approach to become more popular on an international scale. While the latter alternative seems to be more attractive for the majority of researchers in WI, it comes with the challenge to support construction-oriented research with adequate methods. Without research methods, it would probably not be acknowledged as a serious contender.

Hence, it seems that both disciplines are in need for a more appropriate scientific foundation; however for different reasons. This report is aimed at developing a suggestion for such a foundation. Taken the diversity of research topics and goals in research on information systems, it is based on the assumption that there is no one specific method that would fit them all. Therefore, I shall advocate a pluralistic perspective, which regards research methods not as a corset that needs to be carried in order to satisfy prevailing expectations, but as an instrument that should support the process of gaining, assessing and comparing scientific knowledge. Therefore, this report is not targeting a particular method, but a framework that guides the configuration of research methods for certain purposes. In order to set the stage for developing such a framework, we will first look at two current conceptions of ISR: the one represented by IS and the conception, WI is characterized by.

2 Information Systems: Global Success, but State of Crisis

IS is emphasizing an approach to research, which is also common in parts of the social sciences and psychology. The behaviourist approach is aimed at adopting the method of the natural sciences. It is chiefly based on the assumption that the natural sciences' conception of research is superior also for the social sciences and psychology.

2.1 The Natural Sciences as a Model

The natural sciences are regarded as the prototypical model of science by many. In the English language, 'science' and 'natural science' are often used as synonyms. The natural sciences are focussing on empirical knowledge. Hence, scientific knowledge is related to the existence of the natural phenomenon, it describes. In order to foster an objective evaluation of knowledge, the natural sciences are committed to reproducible surveying and measuring methods. This implies to exclude bias caused by subjective perception and conceptualisation as much as possible.

Scientific knowledge in its ideal form is presented as a theory. While there is no common notion of a theory, we can regard it here as linguistic construction consisting of concepts and propositions (hypotheses), which serve to describe the invariant phenomena of a certain domain. Therefore, concepts and propositions of a theory ought to be general (or lawful) in the sense that they apply to classes of phenomena, not just to one concrete occurrence, and that they are valid not just for a certain period of time, but at best forever. Also, they should allow for being tested using reproducible procedures. Theories foster explanation and prediction. A (true) proposition representing a particular observation can be explained by deducting it from a general hypothesis. At the same time, a theory can be used for making predictions: Since it is supposed to identify general concepts and propositions that are invariant over time, it allows for deducting propositions that describe particular future states within a domain, the theory can be applied to. As a consequence, theories can guide the development of new technologies: They allow for explaining, why a technological artefact works and for predicting how it will react in certain situations.

While this brief description of theories[1] and their potential benefits applies in principle to any science, there is one big difference between the natural sciences and other disciplines. Only in the natural sciences, there is a substantial amount of theories that allow for comprehensive explanations and reliable predictions. Also, the natural sciences have produced a number of spectacular results that affected and facilitated our lives tremendously. Therefore, the natural sciences have achieved a recognition that is well above other forms of science. One further reason for this outstanding position is probably the fact that in the natural sciences mathematics plays a key role both for conducting research as well as for presenting research results. On the one hand, the use of mathematics fosters the application of sophisticated and reproducible testing procedures – such as controlled experiments, thereby contributing to especially reliable knowledge; on the other hand, it facilitates the application of research results e.g. for technology development. Last but not least, the use of mathematics contributes to the aura of objectivity and precision, the natural sciences are surrounded by.

Taking the impressive success story of the natural sciences into account, it does not come as a surprise that other fields chose them as a paragon. This is especially the case for large parts of sociology or psychology. The adaptation of the natural sciences to these fields is related to the notion of

[1] For a more elaborate attempt to conceptualize the notion of theory see 8.3.

behaviourism: It recommends restricting the study of social systems or of individuals to observable behaviour. Like the natural sciences, behaviourism is aimed at theories, which can be tested against reality – and which allow for explanation and prediction. For reasons to be analyzed later (see 4), IS, too, chose a behaviourist conception. It is focussed on creating hypotheses or theories about planning and deploying information systems for various economic purposes with special emphasis on identifying factors that determine successful action. To justify these hypotheses or theories, they are tested against reality through empirical studies. At first sight, this conception of science seems to fit well the subject and objective of ISR. Bright and well-trained people (scientists) observe how people deal with information systems - with special emphasis on criteria that are correlated with successful action. Based on that, they create new theories or improve existing ones. These theories can then be used to explain (and justify) singular phenomena or actions. As a consequence, successful action could be deducted from those theories – which includes the prediction of corresponding future states. According to this conception of research, the relationship of ISR to its application domain seems to be similar to that between the natural sciences and engineering. Hence, it may be regarded as a convincing approach to copy the success of the natural sciences to ISR. Nevertheless, its application turned out to be dissatisfactory.

2.2 Symptoms of Crisis

Despite the prominent role, IS plays in the international arena, there has been an ongoing debate that reflects serious doubts on the conception of IS. It is related to the diversity of the field, its success as an academic discipline and its prevailing method. While embraced by some ([Robe96], [LyKi04]), the diversity of the field has been criticized by others: Since it would allow for any topic, which is related to information systems somehow, it is suspected to compromise academic standards. In particular, some assume that diversity hinders a coherent profile, which would be required for achieving disciplinary progress. According to Benbasat and Weber, IS is in jeopardy to loose its identity. Therefore they demand for a unifying paradigm in order to "provide coherence to the IS discipline and to characterize the phenomena that make it different from other disciplines." ([BeWe96], p. 397) It seems that there is not much self confidence among IS researchers as far as the maturity and success of their discipline is concerned. According to Lyytinen and King "the IS field continues to be haunted by feelings of inadequacy." ([LyKi04], p. 221). These feelings can probably be contributed to the lack of significant scientific progress and the poor recognition, IS receives from its neighbouring disciplines. Weber even speaks of "hostile actions … undertaken from … colleagues from other disciplines against the IS discipline." ([Webe97], p. 13) Also in IS itself, there is not much confidence in the academic identity of the field: It is common to regard the relevant theories to originate in its so called reference disciplines – such as psychology or organisation studies (see e.g. [ScWa05]). The ongoing debate on the discipline's academic reputation – Lyytinen and King speak of an "anxiety discourse" ([LyKi04], p. 222) – has not produced a satisfactory result yet. The doubts remain. They are also related to the discipline's practical impact. While there seems to be a consensus that IS should foster the development and use of information systems in practice, many researchers argue that focussing on behaviourist methods produces results, which are of no value for decision makers in practice. This subject has been the core topic of the notorious "rigour versus relevance" debate for long (see, e.g. [Lee99], [Chatt01], [KGH+02]). It seems that most participants in this debate believe that applying a rigorous research method – such as a behaviourist approach – necessarily compromises the relevance of research results for solving problems that occur in practice. According to this common view, rigour and relevance are excluding one another. This results in a somewhat schizophrenic situation: While many IS researchers would prefer more practical relevance over the burden of a rigorous research method, they nevertheless focus on rigour, because that is the only way for making an academic career. According to this viewpoint, relevant research can be targeted only after career goals have been achieved: „Once tenure is out of the way, I would hope professors would be willing to interface closely with industry ..." (Hoving in [KGH+02]). There has been a plethora of publications and discussions throughout IS, which are aimed at challenging the behaviourist paradigm. It is remarkable that this discussion is not focussed on a topic that is being perceived as marginal. Instead, it is characterized by an often insistent and sometimes dramatic rhetoric (see, for example [BaLa89]). This debate gives the impression that IS has been in a continuous state of crisis. Furthermore, in recent years, external factors contributed to the discipline's unpleasant situation. The end of the first Internet bubble caused a substantial downturn of the demand for IS graduates[1]. As a consequence, the number of IS students has declined, which weakened the position of IS in the respective academic institutions ([Lang05], p. 54). In addition to that, with many companies not interested in IS graduates as much as in previous times, it became more difficult to get access to business firms ([Lang05], p. 54). This, however, is of existential importance for IS in order to gather the data, which are obligatory for a behaviourist approach.

While the ongoing debates on the discipline's profile can be regarded as an indicator of a vivid research culture, they are at the same time an indicator of doubt, discontent and even frustration, because the discussion has not resulted in a satisfactory perspective for the discipline so far. Therefore, one can speak of latent crisis, which is perceived more as a burden than as a challenge. A few years ago, Weber concluded with a harsh judgement after analysing the discipline's brief history: "I believe the IS discipline is still in a parlous state and that it will continue to remain in this state until the focus of the discipline's research changes." ([Webe97], p. 13). While one may agree or not with this judgement, it seems that not much has changed since 1997. There is some irony in this situation: Not only IS, but also its former model, the business schools, do not seem to be entirely in accord with choosing a behaviourist paradigm. Bennis and O'Toole, for instance, disapprove this orientation for its lack of appreciating other "forms of knowledge" ([BeOT05], p. 102) and its failure to support the solution of real world problems. Even in economics, which served in turn as a paragon for the business schools, there are doubts about a behaviourist approach as the leading paradigm. McCloskey, an uncompassionate critic of what he calls the „modernist methodology" in economics ([McCl85], p. 18) even speaks of „neurotic behavior ... such as its compulsive handwashing in statistical procedures." ([McCl85], S. xix).

[1] According to an unpublished survey carried out by B. Ives, undergraduate enrolments for IS majors have gone down up to 85 % (66 % on the average). Cited from [LoHi07], p. 90. Similar numbers are reported in [BeS+06]

3 Wirtschaftsinformatik: State of Comfort, but Need for Change

The emergence of WI was driven by a number of factors. The excitement that surrounded IT and its application in business inspired researchers with a background in Business & Administration or engineering to focus on information systems in organisations. At the same time, there was a growing demand in companies, both for fostering new ways of deploying IT and for graduates who combined IT and business skills on a professional level.

3.1 Research through Development?

The growth of WI was accompanied by increasing diversity: While some felt more committed to the design of IT artefacts, others put more emphasis on the design of corresponding organisational systems or on the investigation of new IT driven business models. However, apart from these differences, research in WI has mostly focussed on possible future uses of IT rather than studying current practice.

In the first decades of the discipline, there was hardly any discussion about gaining scientific credibility or about applying research methods. In the ninetieth of the last century, such a debate emerged – however, on a moderate level. While most WI researchers had not much interest in methodological questions and did not feel the need for justifying there work as scientific, some complained about the lack of a convincing methodological foundation (e.g. [Hein05]). As one consequence, action research was sometimes considered as a possible approach to complement the style of research projects in WI with a methodological foundation (e.g. [FrKl+98]). This offered, indeed, an attractive perspective. The problems created by interfering with practice could be turned into an advantage: Cooperation that is accompanied by reflecting upon the essential drivers of the social dynamics experienced in a particular project would allow for a deeper appreciation of the investigated action systems. Hence, research would be driven through development. While the idea of adopting action research as a major approach to research in WI was embraced by few and regarded by others merely as a clever vehicle for legitimizing development projects ex post as scientific, the majority of WI researchers seemed not to care much.

The evaluation of the "research through development" 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. Also, the topics of development projects can usually be related to the WI curriculum. Often, student assistants will participate in such projects. This contributes to the unity of research and teaching, which is appreciated much at German universities. On the other hand, the scientific outcome of WI has been – measured by standards common in the natural sciences – rather poor. The majority of WI scholars who participated in a recent series of peer interviews could not see any scientific contribution of outstanding importance so far [Lang06].

3.2 Changing Rules of the Game

At first sight, WI seems to be – different from IS – a discipline that is in accordance with itself and enjoys a satisfactory reputation both in its neighbouring disciplines and in industry. However, such an impression is misleading. So far, there has been no convincing answer to the question how WI could create a scientific profile that clearly distinguishes it from consulting or software firms. Such a profile would require a suitable methodological foundation. Action research, which was advocated by a few as a distinguishing feature, has hardly been adopted. Apart from that, the suitability of action research for this purpose is questionable, since it comes with a number of methodological problems (s. 7.2). Furthermore, the lack of common standards for conducting projects and documenting research results – especially in the area of construction-oriented research – impedes the comparison of research results and, as a consequence, the development of a coherent body of knowledge. While these problems may be regarded of mere philosophical relevance by some, the discipline is currently facing a challenge that cannot be ignored. For a number of years, it has been common sense in WI that research has to compete on an international scale: The search for new insights in the development and use of information systems cannot stop at national borders. However, going international does not only require publishing in English. Since IS, which is dominating the international scene, emphasizes a different paradigm, it is very difficult to get research results published in prestigious IS journals, if they were not obtained through a behaviourist approach. While this is bemoaned by some, there seems to be a growing number of researchers who adopt the model of IS. At the same time, the regulations that govern academic careers are in a process of change. Instead of writing a second thesis in addition to the dissertation, the so called "Habilitation", it will be required to qualify through publications – preferably in international journals. As a consequence, it can be expected that a growing number of young WI researchers will adapt to the IS paradigm. It is obvious that this kind of opportunistic action creates a clear threat to the identity of WI.

4 The Role of Legitimacy

Our brief comparison of the development and current state of IS and WI shows clear similarities and remarkable differences. Both disciplines emerged for similar economic and technological reasons. They share the same research subject and are both committed to supporting the development and use of information systems in practice. However, with respect to their actual research, they took different paths. How could this happen? Also, how could WI succeed in its environment without stressing a distinguished academic profile by adhering to specific research methods? And why would IS choose a paradigm, many researchers are not happy with? As an answer to this question, I would suggest that the course both disciplines took was not determined by rational decisions but rather by the need for gaining legitimacy – which was accomplished in different ways.

When WI emerged in the sixties of the last century, it was met with suspicion by its neighbouring disciplines, Business & Administration and Computer Science. But this was not only for a supposed lack of scientific credibility, but also for doubts concerning the usefulness and relevance of this new discipline. In both neighbouring disciplines there had been a strong, although not dominating tradition of collaborating with business firms. In large parts of the respective communities, receiving funds from industry was regarded as evidence for relevant research. Against this background, it is not surprising that research in WI often took place in joint projects with companies, which aimed at developing innovative corporate information systems and corresponding organisational settings – e.g. business processes. The funds that were generated by this kind of projects did not only help to gain reputation with universities, it also decreased the burden of ever tight budgets. Hence, the path, the discipline had taken was perceived as convenient and successful.

According to our analysis, choosing a behaviourist paradigm for IS does not seem to be a convincing decision. This may appear as irritating: Why would an entire community of intelligent, educated and experienced researchers deliberately take the wrong path? It seems that one answer to this puzzling situation can be found in the history of the discipline. First of all, not everything about a behaviourist approach is wrong. Its emphasis on the use of a precise language and comprehensible evidence for hypotheses is certainly a step forward compared to the old practices in business schools – and the early days of the IS discipline. In a critical essay on the state of the then evolving discipline, Dearden complains about a "mishmash of fuzzy thinking and incomprehensible jargon" ([Dear72], p. 90). Against this background, it is intelligible that early IS researchers made an effort to build a more solid, scientific foundation. It does not, however, explain why they chose a behaviourist approach for accomplishing this. It seems that this decision was mainly driven by political/institutional reasons. IS departments are usually part of business schools. From the beginning, IS was met with suspicion in this environment ([Lang05], pp. 17). This situation created a serious threat to the existence of IS. The business schools themselves had been under pressure before, especially from economics departments. They shared this faith with other applied disciplines, such as engineering and medicine: "As professional schools … were more and more absorbed into the general culture of the university, they hankered after academic respectability." ([Simo96], p. 112) As a consequence, business schools aimed at a more scientific profile - by choosing a behaviourist approach. Also, they established journals with elaborate reviewing processes. From a sociology of science point of view, it is pursuable that IS researchers as representatives of a young discipline were tempted to adopt this model of their more established neighbouring disciplines at business schools - rather than going for an own profile. As a result of this "race for credibility" ([Webe97], p. 2), the IS discipline accomplished to establish a few journals and a conference series ('ICIS'), which contributed to a scientific reputation.

Against this background, it is remarkable that the discipline has not changed its paradigm yet. It seems that, again, the main reason for this somewhat bizarre situation is probably the need for legitimacy. From the viewpoint of the discipline's peers, giving up the prevailing paradigm would not only jeopardize the discipline's reputation, it would also devalue their qualification as researchers, since it is based on applying the behaviourist paradigm. For those who are still at the beginning of their career, challenging the established rules implies a high risk of failure. This constellation facilitates opportunistic action. Therefore, even those who are not satisfied with the discipline's state contribute to its ongoing reproduction.

5 Information Systems Research: Science or Art?

Our brief overview of IS and WI reveals a sobering, if not frustrating result. On the one hand, there is IS, which chose an approach to qualify as a scientific discipline that seems to be consequent at first sight: It took the most accepted scientific paradigm – that of the natural sciences as a model. It also accomplished to establish peer reviewed journals that gained a high reputation within the discipline. In addition to that, English as the *lingua franca* of the academic world and the outstanding role of the US in international academia have contributed to IS being regarded as a model by respective research communities in many countries. However, despite its remarkable international success, IS is suffering from ongoing doubts about the adequacy of a behaviourist approach, from a lack of recognition and appreciation in business practice and – as a result – from difficulties with getting access to companies. Against this background, continuing the current practice of IS research is not an attractive option. On the other hand, WI is a discipline that seems to be more in accordance with itself. With its research activities it enjoys a remarkable recognition in practice. Also, there is a continuing high demand for WI graduates. However, WI has not achieved a convincing profile as a scientific discipline yet. This includes its differentiation towards consultancy firms as well as the lack of common research methods. Especially with the growing demand to compete on an international level, WI is forced to rethink its approach.

In this situation, WI has three main options to shape its future strategy.

- It could adopt the model of IS, ignoring the serious problems implied by a behaviourist approach.
- It could give up a scientific claim and establish itself as a more application-oriented discipline.
- It could stress a scientific claim by developing research methods that fit the specific peculiarities of ISR, including those of design-oriented research.

With respect to the seemingly dissatisfactory situation of IS, I will not consider the first option. In the remaining part of this section, we will first discuss the second option. Then, I will advocate the third option, a solution for which will be suggested in 9.

5.1 Information Systems Research as an Art?

It may seem strange to consider the conception of ISR as an art rather than a science. However, there are some authors who suggest that the idea of an art, a profession or an engineering discipline fits ISR better than the idea of science. Lee, for instance, recommends "architecture as a reference discipline for MIS" [Lee91]. Based on the implicit assumption that architecture is a well established and sound discipline, he suggests it as a model for ISR because of "strong parallels":

> "Both are design fields, both involve designers and users, and both employ a technology to develop an artefact to serve users. … In both architecture and MIS, the problem to be addressed is not a purely technical or technological one, but also one of individuals, groups, organizations, communities, politics, and cultures. In both architecture and MIS, the object of attention is not technology, but a socio-technical system." ([Lee91], p. 573).

To adapt the approach of architecture to ISR, he emphasizes the interest to develop "technical knowledge", to aim for "mutual understanding" with prospective users and to foster "social betterment and emancipation". While he remains vague in describing the principles or methods that would result form such a paradigm shift, he refers to two approaches that could serve as an intellectual foundation. One is action research, which apparently fits well the architecture metaphor. Its slogans such as "reflection in action" are similar to the "research through development" idea that is popular in WI. The second approach comes as a surprise: Critical Theory advocated by Adorno, Habermas et al. (see, for instance, [Habe81]), is characterized by an elaborate philosophical terminology that is all but suited to foster communication with practitioners. However, it comes with a concept of "discursive rationality", which could be used for justifying means and purposes of IT artefacts (see 6.2.3).

Bennis and O'Toole are not focussing on ISR, but on research in business schools in general [BeOT05]. They argue that the "scientific model" of research, i.e. the behaviouristic approach, is not of much help, when it comes to solve real world problems. As a remedy, they suggest the "professional model", which should be adapted from law or medicine, where it is common that researchers are practicing their profession at the same time. However, instead of discussing the implications for research, they mainly focus on the consequences for teaching. The proposals to conceive disciplines such as IS as an art or a profession are based on the assumption that there is need for systematically exploring the creation of artefacts or action systems – and that the model of the natural sciences is not suited for that. However, they lack a comprehensible concept of ISR as an art. Also, they assume that science is synonymous to the natural sciences.

From a different perspective, Lyytinen and King claim that the scientific model should not be regarded as mandatory for ISR. For them, it is the main objective of an application-oriented discipline to produce "strong results" as "provably valuable consequences of research and instruction with the society as materialized in artifacts, behaviors, and expectations." ([LyKi04], p. 228). They suggest a utilitarian notion of knowledge: No matter how it was produced, it is "strong", if it proves to be valuable. Also, they do not consider theoretical knowledge to be superior: "There is nothing special about theory: it is merely one component in the process of developing useful knowledge." ([LyKi04], p. 231) Such a conception of knowledge and devaluation of scientific knowledge implies the question, how IS could preserve a profile as an academic discipline. It seems that Lyytinen and King do not care much about this question. Instead they suggest a "market of ideas" as "the real center in the IS field", "in which scholars (and practitioners) exchange their views regarding the design and management of information and associated technologies in organized human enterprises."[1] ([LyKi04], p. 236). The market metaphor may be a useful orientation for the exchange of IS with practice. However, it does not provide an orientation for the field of ISR itself.

5.2 Reasons for Pursuing a Scientific Claim

While one may be satisfied with the conception of ISR as an art, I prefer to advocate a scientific approach. Note, that this does not imply a conception that corresponds to that of the natural sciences. Instead, I regard the idea of science as more general, not just restricted to one specific form. Against this background, I will elucidate my preference using three hypotheses:

Analytic superiority: While I am not advocating a positivist, all too optimistic assessment of the capacities of science, I assume that a scientific approach to the solution of complex intellectual problems is in general – not always – more promising than any other approach. While this is a consensus as far as the description and explanation of natural phenomena is concerned, I do not see a convincing reason why this argument should not be applicable to designing socio-technical systems.

[1] This suggestion reminds of a proposal made by Feyerabend, who once recommended organising science as a "supermarket of ideas" [Feye78]

Functional complement: While sophisticated professions like those that are dealing with the development of information systems may deploy methods that are similar to those in scientific ISR, there is still one major difference: In business firms, it is mandatory to care for profits, which, among other things, usually implies to take into account limited resources and time restrictions. In science on the other hand, there is an appreciation of reflection and knowledge as a value on its own (Aristotle). Also, it is associated with the respect for emancipation and rationality. I assume that a scientific perspective is a valuable complement to professional action: It offers the chance to provide ideas and insights, which are not feasible under the pressure of everyday business.

Contribution to ethical and moral progress: The idea of science is directly related to the idea of freedom, emancipation and rationality. I assume that science – mainly through academic education – can foster the dissemination of these ideas and thereby contribute to more liveable societies.

5.3 Peculiarities of Information Systems Research

A proposal to conceive ISR (which includes construction-oriented research) as a science is certainly not sufficient. In order to develop a specific scientific conception, we first need to consider the characteristic features of ISR. ISR is focussing on information systems in organisations. This includes the development, introduction, management and maintenance of information systems as well as their contribution to an organisation's success. It is widely agreed that ISR is an application-oriented discipline. Therefore, it should support people in practice with designing, introducing, managing and maintaining information systems. In other words, ISR should produce knowledge that helps with developing and deploying information systems successfully. Pursuing this objective recommends taking into account the following peculiarities:

Design and Analysis of possible Worlds

Different from the social sciences, ISR is not only aimed at studying existing action systems. In order to provide guidance for decision making and planning in practice, it makes sense to also consider technological innovations and their potential for designing action systems. Different from computer science, this is not just about designing artefacts. Instead, it includes new ways of organizing work, of cooperation and coordination. Therefore, we can speak of designing new *possible worlds*, which serve as an orientation for planning the future. While these possible worlds may be very valuable for inspiring decision makers in practice, they come with a serious challenge: A straightforward scientific evaluation of these scenarios is hard to accomplish. An empirical test, whether a possible world is suited to fulfil certain requirements would require to implement it first. Usually, this will not be feasible.

Against this background, one could refer to the natural sciences and the role of engineering disciplines. However, different from the natural sciences, ISR lacks an "interface" such as the engineering disciplines that would turn theories into technology or more concrete principles of successful action. One further reason for this analogy to fail is that ISR itself takes a perspective that is similar to engineering disciplines, since it is focussed on artefacts, not on nature.

Contingent Subject

The subject of ISR are information systems and the context they are used in, hence an artefact and a social system. Different from the subject of the natural sciences, the artefact is a human (or social) construction. It results from technological, economic and political decisions, which will usually reflect some sort of arbitrariness. Hence, the artefact is contingent in the sense that it does not have to be like it actually exists. It could well be different – with respect to e.g. its architecture, the concrete functions it provides etc. This aspect becomes evident with technological progress. Therefore, focussing on observable artefacts – which is implied by an empirical conception of research – takes the risk to neglect possible not yet existing superior forms of information systems. With respect to technological change, its results may be too late for being applied: By the time a study has been completed, the analysed artefact may already be replaced by advanced technology.

Success in dealing with information systems depends on the context such as the organisational setting, decision makers', developers' and users' qualification, experience and motivation etc. Similar to the artefact, this context is contingent, too. Many of the aspects that constitute the context are not necessarily restricted to the features that can be observed at a certain point in time. They could be different, e.g. if people had received a different kind of education and professional training. One may claim that contingency is a characteristic feature of any social system or of man-made artefacts as well, for which Simon assumes "an air of 'contingency' in their malleability by environment" ([Simo96], p. xi). In the context of information systems, one could speak of amplified or mutual contingency: The contingency of the artefact causes contingent (re-) actions of the context, which in turn will influence the development of future artefacts.[1]

As a consequence, patterns of successful action, which might be discovered by empirical research, are contingent themselves: There may well exist other, more successful patterns of action that are enabled by other artefacts and contexts. However, they are likely out of consideration because they do not happen to exist.

Lack of theoretical Foundation

So far, ISR has not produced well established theories, which can be seen as a further indicator of the subject's contingency. Instead, ISR makes use of theories, which originate in other disciplines, such as sociology, psychology or economics. In WI, concepts and formal theories from computer science are being used, too. The already mentioned survey among senior scholars of IS and WI supports this assumption ([Lang06], [Lang05]). A "collection of theories used in IS" referred to on ISWorld [ScWa05] provides further evidence: None of the presumably most used theories originated in the discipline itself. This lack of own theories is sometimes contributed to the youth of the discipline. While this can hardly be refuted, it is nevertheless obvious that the subject of ISR hinders the discovery of common patterns, which could evolve into theories.

Insufficient Comparability of Results

The design of artefacts, such as modelling languages, reference models or system architectures or possible worlds, as it is common in WI, produces complex research results. They are determined by numerous assumptions and design decisions, which are concerned with the selection of foundational concepts, the evaluation of design goals and many design details. As a consequence, discriminating between two artefacts or possible worlds is a tremendous challenge. Usually, a differentiated comparison with competing results does not happen. This may also be contributed to the complexity of artefacts, which does not allow for a comprehensive documentation in common publication formats. The insufficient comparability of results is a serious problem, since the idea of science implies effective competition and the striving for progress over existing knowledge. Progress, however, requires discriminating successfully between competing knowledge proposals. As a further effect, this situation prevents the evolution of a coherent body of knowledge. In IS, which is focussed on empirical research, the variety of existing artefacts and action systems is a threat to comparing research results, too: Two empirical studies focussing the same research question will usually take place in settings, which differ in various aspects.

[1] With respect to actors in a social system, Luhmann speaks of „double contingency", which results from contingent expectations that two interacting people face (see [Luhm84], pp. 148).

6 Background: Philosophy of Science

For developing a framework to support the configuration of research methods, one should be aware of basic conceptions and schools of thought in philosophy of science. For this purpose, we will first look at the most common theories of truth and then proceed to an overview of important schools in philosophy of science. Considering these also serves to get a better idea of the methodological background of current ISR and to find out whether they are sufficient to satisfy the proposed requirements.

6.1 Concepts of Truth

The creation of scientific knowledge is associated with the claim for truth. In order to judge whether a proposition is true or not, one needs a concept of truth (for an overview, see [Lore96a]). Such a concept depends on fundamental epistemological and ontological assumptions. *Realism* expresses the ontological assumption that the world has an objective existence. With respect to its epistemological meaning, it can be differentiated in *naïve* and *critical* realism. Naïve realism assumes that we perceive the world as it is, whereas critical realism is more sceptical as far as human perception is concerned: It may be limited and deceptive. Therefore critical realism recommends carefully analysing possible sources of bias. *Idealism* – which exists in various flavours [Schw84] - is combining ontological and epistemological assumptions. Kant's conception of idealism does not deny the objective existence of reality. However, in order for us to conceive reality, it is not sufficient to rely on its mere existence ("Ding an sich", "thing on its own"). Instead, we need ideas (concepts) to perceive and structure reality in a way that it becomes what it appears to be. Hence, idealism assumes that reality is an idealistic construction: Our perception of it is determined by our ideas. *Solipsism* is a radical form of idealism [Schw95]. The ontological solipsism denotes an extreme flavour of solipsism: It assumes that only the self exists, whereas everything else (people, reality) exists as a mental idea only. The epistemological solipsism is more moderate. It is still based on the outstanding importance of the self as the only unquestionable existence - as it is expressed in Descartes' famous statement "cogito ergo sum" - and hence our only reference when it comes to get an image of the world. However, it does not deny the existence of other human minds: We cannot exclude it, but we cannot detect it with final assurance either. *Constructivism* resembles idealism in so far as it presumes that it is indifferent towards an objective existence of reality (ontological position). Instead, it assumes that our idea of reality is a construction. The social constructivism (see [BeLu66] for one of the most influential contributions), which has rather a focus on everyday knowledge than on scientific knowledge, emphasizes that our perspective on reality is to a large degree influenced by processes of socialisation and the language we speak: Language is not only the prerequisite for successful communication, it also serves as a structure for categorizing our experience. Hence, it can be regarded as a template for forming our knowledge. According to the social constructivism, this influence of language and society on individual (and social) knowledge is pivotal. Therefore, it regards knowledge as a product of social construction. This is different from idealism, which presumes a more independent individual mind. The so called radical constructivism ([Glas95], [Foer03]) is conceiving the human brain as a self-organizing system, which is autonomous in the sense that its structure and operations are independent from any input from the outside world. This assumed capability of the brain to reproduce itself is referred to as "autopoiesis". Hence, it is the state of the brain that constitutes our image of reality: It provides us with an organisation of our experiences rather than with a representation of the actual ontological reality. According to this view, our perception of reality is like a dream that constructs models of a world, the existence of which is not accessible by us. In other words: Perception reflects state changes within our cognitive system, hence biological or chemical processes – not necessarily the detection of a real phenomenon. Thus, human beings are regarded as "molecular machines" [Matu98] which are loosely coupled through language, the evolution of which is conceived as a result of the brain's adaptive capability. Language – or culture - is regarded as the medium, in which any human being is determined to live ([Mat90], p. 290).

The assumptions made by the radical constructivism will be judged by many as indeed radical if not strange. What are its possible implications for our course of thought? The already mentioned ontological position suggests indifference: Since reality is not accessible to us, we can neither confirm nor reject its existence. At first sight, such an ontological position is rather frustrating, if not destructive for the idea of science, which is based on the idealistic assumption of creating superior knowledge. However, the radical constructivism does not regard science as a useless venture. Instead, it recommends a more relaxed conception of science: Scientific recognition results from the application of certain methods, which determine the cultural homogeneity of a research community rather than reflecting an objective reality [Mat90]. This would, from an epistemological point of view, suggest a relativistic position. This is an important conclusion, which, however, does not require the biological and cognitive assumptions made by the radical constructivism. The social constructivism allows for the same conclusion.

Concepts of truth – sometimes also referred to as theories of truth - are aimed at a useful notion of truth that is compliant with the ever preferred ontological and epistemological assumption. According to the *correspondence theory* of truth, a proposition is true, if it fits the part of reality, it describes. Apparently, the correspondence theory of truth is based on realism. An interpretation of the correspondence theory according to the naïve realism, however, is contradicted by obvious evidence: Perception of reality varies, not only on an inter-personal level, but even on an intra-personal level. Therefore, a correspondence theory of this kind could hardly be used for testing "the" truth of a proposition. Hence, the correspondence theory of truth is usually related to critical realism: It assumes that a correspondence between a proposition and the described part of reality can be detected. However, since perception as well as technical procedures may be biased, perceived or detected correspondence is not a proof of truth. The *coherence theory* of truth suggests that a proposition is true, if it is consistent with an existing set of accepted ("true") scientific and colloquial propositions – thus contributing to a coherent body of scientific knowledge. Hence, the coherence theory of truth suggests testing potential new knowledge against accepted wisdom. Such an approach corresponds to the common procedure of judging the truth of propositions: Only if they make sense in the light of existing knowledge, they are considered as possibly true. The correspondence theory is indifferent towards realism: It does not exclude it, but it would also be compatible with a more idealistic viewpoint or a constructivist position. Finally, the *consensus theory* of truth emphasizes discursive human judgement as the key to an acceptable notion of truth. If a group of scientists after thoroughly considering and discussing a proposition comes to the joint conclusion that it is true, the proposition is regarded as true: "the opinion which is fated to be ultimately agreed to by all who investigate, is what we mean by the truth, and the object represented in this opinion is the real." ([Peir31], p. 407) The consensus theory does not necessarily exclude critical realism: Those who participate in a discourse on judging the truth of a proposition may well have a look a reality. However, it does not depend on realism. Therefore, it could also be combined with the social constructivism.

Apparently, none of these three concepts of truth is entirely satisfactory. As long as it is left to individual judgement only, the correspondence theory is not appropriate for distinguishing science from other cultures. The coherence theory of truth would not allow for new superior knowledge that is not compliant with existing convictions. This would apparently challenge the scientific claim for superior knowledge. Also, it does not offer an answer to the question, how coherence with existing knowledge should be tested. The consensus theory reflects the reasonable idea that truth in science requires a certain degree of approval by others. However, it does not inform about the requirements to be fulfilled by those who are regarded as qualified for this kind of discursive judgement. Also, it does not tell what happens, if two different groups reach contradictory conclusions.

This brief evaluation of theories of truth suggests two conclusions:

- Only if they are applied with a sceptical attitude towards their inherent limitations, they comply with the idea of science.
- Apparently, the theories of truth do not mutually exclude one another. Combining them can help with overcoming specific weaknesses and hence may contribute to a more appropriate, multi-perspective concept of truth.

6.2 Selected Approaches

In philosophy of science, there are some approaches or "schools", which propose guidelines for scientific research. In addition to basic ontological and epistemological positions, they suggest high-level principles for testing and – sometimes – describing research results. They also include guidelines for the demarcation of science from other parts of society as well as for the social/political responsibilities of scientists. The four approaches that are considered below were selected because of their outstanding relevance in philosophy of science.

6.2.1 Logical Positivism

Logical positivism, also referred to as logical empiricism, was created by a group of philosophers in Vienna ('Vienna circle', among others: Schlick, Carnap, Neurath). They were motivated by the refusal of large parts of traditional philosophy, which they judged as 'meta-physical'. Instead, their approach was aimed at making physics a model for any science. There are two main basic assumptions that characterize logical positivism: Firstly, one can speak of (scientific) knowledge only, if it is either based on immediate experience (empirical evidence) or if it can be logically proven. In other words: Scientific knowledge is either empirical or analytic. Secondly, the formulation of knowledge requires the application of a precise formal language. For the latter, logical positivism built on so called modern logics as it had been developed by Wittgenstein and Russel. According to logical positivism, science is aimed at developing theories, the propositions of which need to be *verified*. For this purpose, two kinds of languages are differentiated: The *theory language* serves to describe theories as general laws. It is a formal language, which allows for expressing logical propositions. The *observation language* serves the representation of sensuous perceptions. Its terms refer directly to observable objects and events. Since the theory language allows for building formal structures only, logical positivism proposes rules of correspondence to associate terms of the observation language with terms of the theory language. In other words: The terms of the observation language are used for the interpretation of theories expressed in the theory language. Any empirical term or proposition qualifies as scientific only, if its semantics can be reduced to something observable. Testing a theory requires deducting its implications in terms of the observation language and then testing the corresponding propositions of the observation language against reality. The truth of a proposition in the observation language is defined by the procedures suggested to test it against reality. A complete test would imply to take into account every possible instance of the terms mapped to the observation language, which would either result in an infinite regress or in the inductive fallacy. However, logical positivism allows for verification through a limited set of observations. In order to protect a verified theory against refutation, it can be supplemented with constraints on its application domains ('ceteris paribus clauses').

From an ISR perspective, logical positivism is relevant for its influence on current research in IS. After the Nazis forced the protagonists of the Vienna circle to flee the country, some of them continued their work in the US and had a substantial impact on the further development of American philosophy of science ([Steg78], p. 349). Also, logical empiricism recommends behaviourism in psychology ([Carr96], p. 284): Only if one restricts research to observable facts, it is possible to define procedures for empirically testing corresponding hypotheses. Against this background, it does not come as a surprise that the dominating paradigm in IS is referred to as positivist. At the same time, there is a clear difference between current research in IS and the recommendations of logical empiricism: It is not common to use logics for representing theories.

In contrast to the claim of objective knowledge, the verification of a hypothesis faces the problem how to decide about a sufficient level of evidence. While this can be regarded as not satisfactory from a philosophical point of view, it is characteristic for current practice of empirical research: It is regarded as adequate to check a hypothesis for an assumingly representative selection - with a confidence level that is generally accepted as being sufficient. With respect to the evaluation of possible future worlds, logical empiricism would not consider them as acceptable scientific knowledge as long as they do not allow for being tested through observations.

6.2.2 Critical Rationalism

Critical rationalism, sometimes misleadingly referred to as positivism, represents a stream in philosophy of science, which was founded by Popper and further elaborated by many others. Popper's conception of scientific research was motivated by his admiration of the natural sciences and by his rejection of logical positivism, which assumes that scientific knowledge is always empirical – i.e. that there is no *a priori* knowledge. In contrast to logical positivism, Popper assumed that the creation of empirical knowledge depends on already existing conceptions or theories. Therefore, he was a vigorous opponent of induction as an epistemological instrument. Hence, Popper is a follower of Hume, who was the first to point at the inductive fallacy: It is not possible to prove the truth of a general proposition by a set of true sentences about corresponding instances. He is also a follower of Kant who claimed a conceptual *a priori* of any experience: observations without concepts are "blind" (Kant, KrV, B741, A713). Against this background, Popper developed guidelines for scientific research. His conviction that there is no study of reality without a theoretical background results in the recommendation to create hypotheses or theories first – instead of collecting empirical data for inductive reasoning only. The acknowledgement of the inductive fallacy implies that there is no final confirmation or even a proof of empirical hypotheses. Popper turned this limitation of scientific knowledge into a virtue: According to critical rationalism, it is mandatory that scientific hypotheses cannot be proven. Instead, they should be expressed in a way that offers a good chance for refutation. This implies the use of a precise language and a high level of information content. The more precise (and comprehensive) the language and the higher the level of information content, the lower is the range of possible interpretations, hence, the better the chance for successful falsification. Information content corresponds to abstraction: the more general a proposition, the higher its information content. Testing hypotheses requires their confrontation with reality through reproducible observations or experiments. Critical rationalism favours critical realism. Therefore, testing itself is subject to critical examination. Furthermore, only those propositions are accepted as scientific, the truth of which can be tested against reality. This excludes value judgements and – important for ISR – constructions of possible future worlds. Note, however, that both value judgements and constructions may be backed by hypotheses with empirical content, i.e. propositions with a claim for truth.

On an abstract level, the course of research outlined by critical rationalism starts with the creation or selection of hypotheses that claim to describe lawful patterns of a certain real world domain. Subsequently, these hypotheses are used for predicting the outcome of a set of observations or experiments. Testing these predicted outcomes against reality should be aimed at refutation. As soon as a test in a single case disproves the prediction, the corresponding hypothesis is regarded as falsified and would be eliminated. As long as any attempt to falsify a hypothesis has failed, it is regarded as preliminary confirmed. Hence, critical rationalism suggests an indirect justification through failed falsification. This process is called "critical appraisal". According to critical rationalism, the growth of scientific knowledge follows a Darwinian scheme: The body of scientific knowledge comprises all hypotheses that have not yet been eliminated through falsification attempts. Critical rationalism is aimed at explanations: Empirical propositions are explained by deducting them from preliminary confirmed theories.

While critical rationalism corresponds obviously to the natural sciences, Popper and his followers recommend it for the social sciences as well. This has caused many controversial debates, especially in the so called positivist dispute in sociology ([AdDa+76], [Dahm94]). It included a discussion of the question, whether social systems are essentially different from natural phenomena and therefore require a dedicated research approach as well as the question, whether and how value judgements should be incorporated into scientific reasoning. Apart from the details of this discussion, the question remains, whether critical rationalism is suited for ISR. While it can be regarded as a foundation of behaviouristic research, the rigorous application of the falsification tenet is hardly compatible with socio-technical action systems that are subject of ISR. As a consequence, probably all empirical hypotheses in ISR would need to be eliminated. It would not be sufficient to back a hypothesis by testing it successfully in several cases, instead, it would be required to confirm it in all cases. A further problem related to the application of critical rationalism is its lack of a precise conception of core terms. The concept of a theory is restricted to a set of propositions with the claim for general validity, which allow for refutation. It does not tell how general a proposition needs to be or how precise its terms need to be to qualify for being part of a theory. Also, critical rationalism does not tell much about relationships between theories: Is a theory, which can be deducted from other theories (which can be reduced to these), still a theory? With respect to ISR this is an important question, since one could assume that most hypotheses in ISR could be reduced to theories in reference disciplines. The concept of scientific progress proposed by critical rationalism seems to be rather narrow. Especially for the evolution of a new discipline, the development of a sound and useful terminology is a key criterion for indicating the progress of the field. Last but not least, critical rationalism restricts science to empirical hypotheses that can be tested against reality – in other words: Only propositions that satisfy the claim for truth according to the correspondence theory of truth qualify for being scientific. Such a restriction does not necessarily exclude the design of possible future worlds from the body of scientific knowledge. The designs could be implemented and then the core hypotheses underlying the design could be tested. However, often the implementation of a construction in a realistic setting is no option, since it would require too much resources and time. While blueprints of future worlds cannot entirely be justified through empirical studies, Albert suggest catering for a more substantial evaluation by bridging the proposed scenario to propositions with an empirical content. Among other things, this so called "Brückenprinzip" (bridging postulate) ([Albe91], pp. 76) implies to show the feasibility of a possible world.

While critical rationalism excludes value judgments from scientific knowledge, its proponents nevertheless propose an orientation for the development of societies: The "open society" [Popp71] is characterized by liberalism and reasonable individuals (see also [Popp84], p. 23, [Albe78], p. 12, [Albe72], p. 119). It seems to be a projection of critical rationalism to society in general. The impact of critical rationalism on ISR is difficult to judge. In WI, critical rationalism is probably well known. However, the model proposed by critical rationalism is hardly been followed. In IS, some descriptions of so called quantitative methods include explicit references to Popper (e.g. [StGe+04]). However, it seems that the details of critical rationalism are not much appreciated:

Usually, Popper is regarded as a positivist, which is – at least without further comments – inappropriate, since critical rationalism comes with an explicit anti-positivist claim.

6.2.3 Critical Theory

Critical theory, also referred to as 'Frankfurt school', has evolved in sociology and social philosophy. Adorno [AdHo69] and Habermas [Habe81] are among its main proponents. It is based on the assumption that a so called positivist approach – such as critical rationalism is considered to be – is not suitable for the social sciences. According to critical theory, it would not be sufficient for the social sciences to merely aim at a description of existing societies, since that would only contribute to an affirmative reproduction of the given social situation. Instead, the social sciences should also outline a vision of possible alternatives in order to foster social change. If not only the description of factual social systems, but also the proposal of possible future options is regarded as scientific, there is need for criteria, which allow for an acceptable evaluation of competing proposals. This also implies the need for a general orientation to guide the development of society. Both aspects are intended to be covered by the concept of *discursive rationality*. It includes both, so called theoretical discourses and practical discourses. Theoretical discourses are aimed at judging the truth of propositions – based on the consensus theory of truth. Practical discourses, on the other hand are aimed at judging the appropriateness of norms for social action. For a discourse to be rational, it has to fulfil a number of requirements [Habe81]:

- The participants have to be knowledgeable.
- The discourse has to be open (nobody who is affected is excluded) and free of mastery (all participants are equal).
- The participants need to believe in the chance to accomplish a consensus.
- They should not make assertions that are not in accordance with their convictions.
- They should be willing to discuss their subjective world views, preferences and interests – and take into account those of others. Based on that, they should aim at a trans-subjective perspective.

These principles apply both to theoretical and practical discourses. They are also essential features of an enlightened society that serves as an ideal orientation. Different from critical rationalism, the justification of a proposition is not merely an act of testing it against reality, but occurs during a process of collective learning. Also, critical theory objects against a strict separation of an "objective" observer and a research subject in the social sciences. Instead, it emphasizes the relevance of interaction and collective learning. Different from critical rationalism, it is aimed at understanding social phenomena rather than explaining them ([Habe81], pp. 159; see also 7.2).

For ISR, critical theory offers an interesting perspective: It would allow for evaluating designs of possible worlds by applying the tenets suggested for discursive rationality. Apart from the social and political visions that accompany critical theory, such a procedure is similar to the idea of peer reviews. Despite this potential advantage, critical theory implies a number of challenges. Firstly, it is not clear how to guarantee that the participants of a rational discourse apply the prescribed rules in satisfactory way. Who is supposed to make these decisions? Also, critical theory has no concept of a theory that would allow for discriminating theories from other linguistic constructs. This includes the lack of any guideline for the language to be used with scientific reasoning. From an ISR point of view, two aspects are relevant: The language used by the proponents of critical theory is very much focussed on sociology. It gives the impression of a jargon reserved for a certain community only. Furthermore, critical theory is suspicious against technology, which is regarded as reification of teleological rationality (referring to the concept of "Zweckrationalität" that was introduced by Max Weber), hence consolidating existing patterns of power.

Also, it has no explicit concept of scientific progress. Of course, the discrimination between competing knowledge proposals could be done in a discursive way. What happens, however, if two different communities produce contradictory judgements concerning the truth or appropriateness of a statement? This problem does not disqualify any consensus concept of judgement. However, it can contribute to the evolution of ideologies represented by different schools of thought.

In general, the impact of critical theory on ISR is marginal. Some descriptions of so called qualitative methods include references to critical theory, which makes sense with respect to the role hermeneutics plays in critical theory. Considering the reservation critical theory shows towards technology, it is remarkable that it has been adopted by some to enrich construction-oriented research. Dietz draws on Habermas' conception of communication to develop and justify a method for designing information systems ([Diet99], [Diet06]). Lee, too, suggests making use of critical theory as a foundation of a design-oriented approach to ISR that takes architecture as a model ([Lee91], see 5.1).

6.2.4 The Erlangen Constructivism

Different from critical theory, the so called Erlangen constructivism - not to be confused with the radical constructivism - claims to be appropriate for all scientific disciplines. Its main focus, however, is on so called cultural sciences, hence disciplines, which focus on human culture and its products. These include, among others, sociology and economics. The Erlangen constructivism was created by the mathematician and philosopher Lorenzen and the philosopher Kamlah ([Lore74], [LoKa84], [LoSc73]). It is based on the assumption that science is not just dealing with a given nature, but with cultural constructions, which are constituted through action systems and language. This is the case for the subject of the cultural sciences, but in general for any science with respect to its instruments (technical language, measuring tools). In order to promote a more objective, scientific appreciation of the phenomena to be studied, Lorenzen and Kamlah suggest a reconstruction of the constructions. It is aimed at justification by going back to shared elementary experiences, memorized as actions and speech acts (pre-scientific apriori). In order for these reconstructions to qualify as scientific, the Erlangen constructivism proposes a formal logic-based language ("Orthosprache"), which is assumed to be a better foundation for reasonable discourses than the natural language.

According to the Erlangen constructivism, the justification of an empirical theory takes three steps [Thie84]: (1) establishing a shared set of basic, pre-scientific assertions to secure a common foundation; (2) establishing a shared set of assertions concerning the procedures and tools to access and measure the studied phenomena; (3) building a deductively established theory. To accomplish satisfactory results, the Erlangen constructivism proposes the concept of *reasonable consultation* ("vernünftige Beratung"), which resembles the idea of rational discourses that is suggested by critical theory. It includes the following tenets:

- The participants should be knowledgeable.
- They should have the ambition to accomplish a consensus.
- They should be honest and not withhold relevant information.
- The consultation should take place in an easy atmosphere – free of mastery
- The participants should aim at "trans-subjectivity" – by identifying and trying to overcome subjective viewpoints.

Reasonable consultations are not only recommended for deciding about the truth of an empirical proposition. They are also regarded as suitable for judging moral statements in a scientific way. This is accompanied by the vision of a society that is called a "free monodoxy" ("freie Monodoxie, [Lore74], p. 12): Different from a free, pluralistic society, it is characterized by unity, which would be accomplished through reasonable consultations.

With respect to ISR, the Erlangen constructivism offers the option to evaluate constructions of possible future worlds: They would be evaluated in reasonable consultations that would have to take into account the interests of all affected parties. However, it inherits all the burden of a consensus-oriented concept of justification. Similar to critical theory, it does not tell how to decide who is qualified for participating in such consultations – or how to deal with the social fact that qualified people may not be interested in participating. Also, a comprehensible reconstruction of statements down to elementary statements that reflect shared experiences is probably too costly in most cases. There is no explicit notion of a theory, except for its deductive construction. Also, the idea of scientific progress it not dealt with explicitly.

The introduction of an explicit language for scientific reasoning is an important contribution, since language is pivotal for research. As far as the IT artefact is concerned, a formal language comes with a number of advantages. There are, however, numerous formal languages for this purpose already, which have the advantage to reflect the peculiarities of existing technologies (of course, this comes with the problem that they to some degree depend on these technologies and are hardly suited to overcome them). Nevertheless, it is questionable whether a formal language can be expressive enough for describing research in ISR. It is indicative that the proponents of the Erlangen constructivism do not use the "Orthosprache" in their own publications on philosophy of science.

In general, the Erlangen constructivism is hardly known in ISR. In WI, there are only a few, however elaborated approaches to adapt core ideas to ISR. Wedekind and Ortner, both at the computer science side of the WI spectrum, suggest applying the idea of criticizing existing language use through reasonable consultations in order to reconstruct it as a reference language, which corresponds to the "Orthosprache" suggested by the Erlangen constructivism. The reference language is then being used for developing information systems ([Ortn05], [WeOr80]). It is remarkable that this approach is very similar to current ontology based approaches to information systems development, however with a more solid philosophical foundation.

6.3 Comparison and Implications for Information Systems Research

Based on the overview of selected approaches in philosophy of science, we need to analyze how they compare with respect to the requirements for ISR proposed above. With respect to supporting the design and evaluation of artefacts in ISR, critical rationalism is only of limited use: Firstly, it does not consider the design of artefacts as part of scientific research. At best, it is regarded as a – tautological – transformation from existing theories. Secondly, it considers only propositions with an empirical content – which can be tested applying the correspondence theory of truth – as scientific. Therefore, the evaluation of an artefact would be restricted to those aspects to empirical hypotheses regarding the effects of its use. This, however, would require the implementation of the artefact or of relevant parts. Critical theory allows for supporting the evaluation of artefacts through rational discourses. However, not only that it leaves the problem of deciding how to meet the requirements of rational discourses, it does not take into account the peculiarities of ISR, since it is mainly aimed at the social sciences. Especially, it does not take into account the importance of (semi-) formal languages that may be required for the adequate description of information systems. With its concept of reasonable consulting, the Erlangen constructivism provides support for an evaluation of artefacts, too. Different from critical theory, it proposes the use of a formal language. Although the 'Orthosprache' is not intended for the description of information systems, it could be used for the specification of information system concepts. Tailoring of research methods is out of the scope of all three approaches, since they come with the claim to be the best choice for the disciplines they intend to be applied in. Also, none of the approaches provides comprehensible guidelines for the documentation of research results. While all of them suggest the development of theories, they lack a precise concept of a theory. All approaches foster cross-disciplinary cooperation by focussing on concepts that are intended to apply not only to one discipline. In other words: They suggest a cross-disciplinary meta-terminology. On the other hand, none of the approaches provides any concrete support for cross-disciplinary research in the context of ISR.

Apparently, none of the approaches provides a sufficient orientation for ISR. With respect to the evaluation of possible future worlds, there is, on the one hand, a clear difference between critical rationalism and the other two approaches: While critical rationalism would exclude such judgements from scientific research as long as they cannot be tested against reality, both critical theory and the Erlangen constructivism recommend a procedure, which could be used for developing judgements without empirical justification. On the other hand, all approaches share a similar vision of a better society. Popper's "open society" emphasizes individual freedom and responsibility, which are core elements of rational discourses and reasonable consultations. The main reason for critical rationalism to exclude non empirical propositions is probably its vision of a better society: to protect science against its ideological misuse. Critical theory and the Erlangen constructivism probably have a similar motivation, which, however results in the opposite recommendation – based on the conviction that a discursive process that is intended to meet high scientific standards is better suited to produce satisfactory results than decisions that take place in a less sophisticated environment. Critical rationalism stresses the correspondence theory of truth, while the other two prefer the consensus theory. However, that does not mean that these viewpoints are entirely incompatible. I do not see any evidence for critical theory or the Erlangen constructivism to explicitly exclude the correspondence theory for testing certain empirical propositions. They only claim that the correspondence theory is not sufficient. At the same time, critical rationalism does not entirely exclude the consensus theory: Its proponents are struggling for convincing others, hence to accomplish a consensus – even though this is meant to happen on a meta layer (in philosophy of science) only.

To summarize: None of the approaches in philosophy of science is suited to satisfy the specific requirements of ISR. Even selecting specific features from particular approaches would not allow for an adequate configuration. This is probable the case, because all approaches were created with specific fields – such as the naturals sciences or the social sciences – in mind. However, a few approaches to adopt critical theory or the Erlangen constructivism as a foundation for ISR demonstrate that it is possible to supplement basic concepts of philosophy of science with interpretations that account for the peculiarities of ISR. For the further course of our study, there are mainly two conclusions. Firstly, science requires an accepted conception of justification. In the ideal case, justification goes back to a concept of truth that is shared by all observers. In many cases, the correspondence theory of truth should satisfy this requirement – no matter whether this kind of commonality is only a construction or not. Secondly, in those cases where the correspondence theory does not work – as with possible worlds in ISR, there seems no better choice than a rational process, where the concept of rationality is associated with rules for communication. This is similar to evaluation in practice: If measurable indicators are missing or regarded as not sufficient, the focus is often on rules to guide the evaluation process.

7 Research Methods in Information Systems Research

Philosophy of science remains on a rather abstract level. For this reason and for not considering the specific peculiarities of ISR, it is not sufficient for providing comprehensible guidance to particular research projects. There are a number of specialized research methods in ISR, which are aimed at offering more concrete guidance. In general, a method is aimed at providing support for solving a class of problems. A problem is constituted by a certain domain and a set of objectives. In addition to this common view, I would suggest regarding a method as composed of a linguistic structure and a corresponding process. The linguistic structure is given by a specific terminology or a terminological framework, which serves to structure the problem domain according to the actual objectives. The process describes in more or less detail the steps to be taken for solving a problem. Describing a step will usually refer to the terminology, hence, it is not independent of the terminology. In addition to linguistic structure and process, a method may include criteria to evaluate competing solutions. Examples for methods in the scope of ISR are software development or requirements analysis methods.

Based on this conception, a research method would consist of a terminology that includes terms to characterize the underlying ontological and epistemological position as well as generic notions of research, such as 'explanans', 'explanandum', 'theory', 'hypotheses', 'confirmation' etc. These concepts should include criteria to support the evaluation of research results. Based on the core objectives of research, which depend on the epistemological position, the process describes how to proceed in order to accomplish these goals. Note that 'method' should not be confused with 'methodology' (which happens frequently): A methodology denotes a *study of methods*.

This report is aimed at an approach that guides the configuration of research methods for ISR. Therefore, we will first consider existing methods. This overview is restricted to methods, which have been adapted to IS, because, so far, WI lacks specific research methods.

7.1 The Behaviourist Approach

Behaviourism was created in psychology in order to establish an appropriate scientific foundation, which takes the natural sciences as a model. It suggests reducing the analysis of psychological phenomena to observable behaviour. Hence, internal mental states such as fear or anger are excluded from immediate detection, since they are not accessible from the outside. Therefore they can be analyzed only indirectly, by studying the effects of external stimuli on observable behaviour. For its limitation to observable facts, behaviourism corresponds to positivism. Following the natural sciences as a model, behaviourism is aimed at theories, which allow for *explaining* observable behaviour. In IS, the behaviourist approach is the dominating paradigm.

A behaviourist approach to ISR suggests selecting or creating hypotheses about certain aspects in the domain of interest, which are subsequently evaluated through empirical studies. This requires hypotheses to be conceptualized in a way that allows for detecting and measuring it – often referred to as *operationalisation*. According to the claim for explanation, this implies to differentiate dependent ("explanandum") and independent variables ("explanans"). Such a conceptualisation should promise a high degree of *validity* (do the variables actually represent the subject of analysis?) as well as a high degree of *reliability* of the measurement procedures (can the relevant aspects be measured exactly in a reliable, i.e. reproducible, way?). A hypothesis is being evaluated by applying it to a set of single cases. The resulting propositions are then tested against reality. This requires collecting data representing the tested cases. This should be done without affecting the subject of analysis. Usually, it is not an option to collect a complete set of data. Instead, it is regarded as sufficient to collect representative data sets. For this purpose, various data collection techniques, such as observation, interviews and document analysis, can be deployed. Data collection is followed by data analysis. It includes the application of deductive and inductive statistical procedures, which requires representing the collected data using an appropriate level of measurement. For this reason behaviourist research is often referred to as "quantitative" research as opposed to "qualitative" research. However, such a distinction is misleading, since "quantitative" and "qualitative" are orthogonal predicates. In addition to testing hypotheses according to the correspondence theory of truth, it is also common practice to relate them to comparable work in the field, hence, to make use of the coherence theory of truth.

The methodological background of the behaviourist method is hard to judge. There is an obvious correspondence to critical rationalism (see 6.2.2), which proposes expressing and testing hypotheses through empirical studies. This impression is confirmed by a report on „quantitative, positivist research methods in information systems", where Straub et al. refer explicitly to Popper [StGe+04]. However, at the same time, it is my impression that critical rationalism is not well known in IS. Straub et al., too, promote a misleading understanding of critical rationalism: They assert that Popper was a representative of logical positivism, ignoring the fact that Popper was a determined opponent of logical positivism. It fits the impression of an inappropriate appreciation of Popper that publications on the behaviourist research method often do not take account of the critical rationalism's claim for falsification, which recommends to formulate hypotheses that show a high information content and – as a consequence – a high risk of refutation. Also, the behaviourist approach's regular use of statistical procedures contrasts Popper's opinion. With reference to the problem of inductive reasoning, he regards testing hypotheses by using statistical procedures as a problematic option ([Popp82], p. 213).

The following evaluation of the behaviourist method is differentiated in general methodological aspects and other aspects, which result from the peculiarities of ISR. From a general philosophy of science perspective, it is remarkable that the behaviourist approach does not include an elaborate concept of theory. Instead, it seems that a theory is regarded as a system of generalized propositions, which is appointed as theory by a community of scientists. While this reflects a common practice in science, it does not allow for a precise identification of a theory. This includes the question, whether a system of propositions that can be deducted from (or reduced to) more general theories, may still qualify as a theory (for a discussion of reductionism see [Wolt95]). One further epistemological problem is related to the interpretation of statistical correlation: There is no comprehensible justification for a correlation coefficient to be regarded as sufficient. It is rather a matter of convention. With respect to the peculiarities of ISR, a behaviourist approach shows four serious drawbacks: *insufficient consideration of contingency*, *lack of orientation for decision makers* in practice, *promotion of trivial results* and *unacceptable effort*.

Insufficient Consideration of Contingency

The specific kind of contingency, ISR is confronted with, refers to yet evolving patterns of deploying information systems as well as to future development of information technology. This includes the task to develop and analyze possible future worlds (see 5.3). The assumption that the subject of ISR, namely information systems and patterns related to their development and use, is contingent is questioning a basic supposition of empirical research in a behaviourist style: the existence of general, invariant patterns of action, which can be represented by theories.

Challenging this supposition has two implications. Firstly, it recommends rethinking the concept of a theory. Secondly, it reduces the relevance of testing hypotheses based on the correspondence theory of truth. Testing a hypothesis refers to existing patterns of action in the context of information systems only. This is especially relevant for testing hypotheses, which describe successful patterns of using information systems ("best practice"). While they may be confirmed, it could be inappropriate to conclude that they describe generic patterns of success. Instead, there may be other patterns of action that have not evolved yet, but which are clearly superior (see 5.3). One might claim that hypotheses are not necessarily restricted to already existing artefacts and contexts. While this is true, hypotheses of this kind are not compatible with the behaviourist paradigm, since it requires testing hypotheses against the actual world (correspondence theory of truth), not against possible worlds. This indicates that aiming at theories in ISR that apply to the standards, which are common in the natural sciences, is a frustrating endeavour. This judgment is backed by the actual history of ISR. In a recent study [Lang05], renowned IS scholars confirmed that so far, IS has not produced any remarkable theory (see also 5.3). Of course, such a lack of success in the past does not prove the impossibility of creating powerful theories in ISR in future times. However, together with the problems caused by the presumed contingency of the subject, they constitute a strong argument against the adequacy of a pure behaviourist approach.

Lack of Orientation

An application-oriented discipline is aimed at providing guidelines for successful action in practice. There are at least two reasons why a behaviourist approach in ISR is not a convincing choice in this respect. The first reason is related to the arguments already discussed above: By its very nature, behaviourist research is focussed on existing artefacts and corresponding patterns of action. Hence, it is characterized by a retrograde perspective thereby fading out options for future action, which are potentially more attractive. So, even if research would result in "theories" that are suited to explain patterns of action related to information systems in the past - which is very unlikely to happen, they might not be sufficient to cover alternatives superior to current best practice. With respect to the high dynamics of change in information technology and its application, results of this kind of research will usually not be suited to provide guidelines for innovative practitioners. The second reason is related to the specific way, behaviourist research is performed. As a consequence of the effort to copy the model of the natural sciences, ISR is focussed on concepts that allow for being measured in a valid and reliable way. Measuring is a prerequisite for producing input for statistical procedures – the higher the level of measurement, the better. Research of this kind takes the risk either to leave out relevant aspects only because they do not allow for measurement or to focus on hypotheses that are all but interesting, but allow for measurement. Hence, it seems that in many studies, the application of complex statistical procedures is contrasted by rather poor insights. Often, this aspect is referred to as "rigour", which is associated with "precise", "objective" and "scientific" by some. There has been a long discussion in IS, which is based on the assumption that rigour of this kind prevents research results from being of value for supporting decision making in practice ("rigour vs. relevance"). However, such a viewpoint is misleading. There is no necessary contradiction between so called rigour and relevance. However, if rigour becomes an end in itself without caring about elaborate insights, it is likely that results are not relevant for successful action in practice.

Promotion of Trivial Results

A behaviourist approach requires focussing on those aspects of information systems and their use, which allow for specifying and testing hypotheses. This includes conceptualizing hypotheses in a way that allows for measuring the outcomes of corresponding tests. Also, it requires preparing the resulting data for statistical procedures. If this is regarded as a necessary constraint for research to be accepted as scientific, it contributes to fading out important aspects of the subject, although they might be intellectually inspiring. Even more, it may foster conditioning of researchers in a way that their interest is primarily directed towards research questions that allow for applying a behaviourist approach. Hence, the main driver for research would not be the intellectual interest in sophisticated knowledge, but the effort to satisfy the formal requirements imposed by a behaviourist approach. As a consequence – this is an assumption, I would propose - the vast majority of behaviourist research in ISR does not provide interesting new insights into the subject[1]. Instead, it fosters the accumulation of trivial results – a fact that is distracted from by the use of sophisticated statistical procedures.

Unacceptable Effort

A behaviourist approach depends on getting access to actual information systems and related action systems. Due to the complexity of the subject, the description of a system implies a remarkable effort. If one, for example, intends to analyze the influence of professional training on the quality of IT management, a plethora of aspects need to be recorded. They include social, economic, psychological and technological aspects. While this means a lot of work even in the case of a single organisation, it implies a level of effort that is hard to afford for many research institutions, if a representative selection of enterprises needs to be studied. However, a representative representation is mandatory for behaviourist research in order to allow for generalisation. As a consequence, empirical research in IS is often compromised: A thorough exploration of the subject is replaced by questionnaires and structured interviews, which are designed not to exceed the researchers' and the questioned persons' time and budget. Therefore, the required effort is a serious threat to behaviourist research: It may prevent interesting studies or it may contribute to research designs, which simplify the considered subject too much. This problem becomes even more evident, if a behaviourist approach is used for testing hypotheses about innovative ways of designing and deploying information systems. Such an approach would require implementing the innovative socio-technical systems in a scale sufficient for testing them ex post. In most cases, the time and effort required for this kind of research will make it a no-option.

To summarize, we can conclude that restricting ISR to empirical research in a behaviourist style would be a serious mistake. That does not mean, however, to exclude any kind of behaviourist research from the ISR agenda. In 9.3, criteria will be suggested that help to decide whether a behaviourist approach is suitable.

7.2 Hermeneutic Approaches

Hermeneutics can be regarded as an art, or study of interpretation. Hermeneutic approaches are based on the assumption that the peculiarities of the research subject, the humanities are dealing with, requires a specific method. This assumption has been applied to the social sciences as well. On the one hand, it is justified by pointing at epistemological deficiencies related to a behaviourist approach. According to this argument, social systems are characterized by phenomena, which cannot be analyzed comprehensibly by considering only aspects that qualify for statistical analysis of data. Originally, hermeneutics is a method for interpreting written text. Hence, its adaptation to the social sciences is focussed on the interpretation of actions that are expressed through language. Different from behaviourist research, the access to reality happens through interacting with actors in the actual domain of interest. The actors' actions and utterances made during the course of a research project are subject to thorough interpretations. Interacting with actors in the research fields contrasts the idea of objective research that is promoted by the behaviourist approach. Hermeneutics is motivated by the conviction that the researcher's involvement creates the chance to enrich research results by fostering a more authentic perception of social reality. Hence, hermeneutic research is aimed at *understanding*.

[1] At least, it has not produced original theories so far (see above).

In contrast to explanation, understanding implies the idea of intentionality, which is emphasizing the relationship between thoughts and experiences on the one hand and the subjects of thoughts and experiences on the other hand: These relationships are characterized by personal perception, experience and conceptualisation. They are objectified in language. Language is the pivotal object and instrument of hermeneutic analysis. "Language also typifies experiences, allowing me to subsume them under broad categories in terms of which they have meaning not only to myself but also to my fellowmen." ([BeLu67], p. 53). The meaning of linguistic expressions cannot be explained in an extensional fashion – by enumerating all their characteristic features. Instead, they feature what one could call inherent semantics: Their meaning depends on recalling certain associations to thoughts and experiences ([Habe84], p. 311). Berger and Luckmann stress this aspect, too: "Language originates in it and has its primary reference to everyday life; it refers above all to the reality I experience in wide-awake consciousness … which I share with others in a taken for granted manner." ([BeLu67], p. 53). According to hermeneutics, researchers are able to appreciate the studied actors' world views, motives, attitudes etc. in a meaningful way only by referring to their own experiences ([Wrig74], p. 20). The late Wittgenstein illustrates this thought with the following image: "Even if a lion could talk, we would not understand him." ([Witt58], p. 223[e]). Hence, the hermeneutic approach emphasizes the researcher's role as a human being. This is a precondition for studying social phenomena and at the same time an obstacle to objective truth, which is – according to hermeneutics – impossible anyway. Against this background, the basic idea of a hermeneutic approach to research is to make interpretation a conscious, reflexive process that is sometimes referred to as "hermeneutic circle" [Gada72]: By reflecting the parts of a phenomenon, he may get a better, revised understanding of the whole, which in turn may guide a deeper appreciation of the parts and so on.

While hermeneutics has various roots and flavours, e.g. the tradition of interpreting theological documents or philosophical phenomenology, its adoption in the social sciences is often related to critical theory (see 6.2.3). There is, however, no common hermeneutic method in the social sciences. One approach, for instance, builds on transcriptions of conversations, which are to give an authentic representation of certain social settings (see e.g. [Welt83]). Other approaches recommend the use of images and analogies to everyday life in order to foster the appreciation of the researcher's insights ([Morg86], [Weic79]). In ISR, hermeneutic approaches are certainly not mainstream research. However, in IS they have gained remarkable attention, often referred to as 'qualitative' or 'interpretative' research. Usually, they are suggested to overcome some limitations of behaviourist research (e.g. [Wals95], [Trau97]). To contribute to a hermeneutic method for ISR, Klein and Myers proposed a guideline that includes seven principles for conducting and evaluating "interpretative field studies" [MyKl99]. The "fundamental principle of the hermeneutic circle" recommends the reflexive use of a circulatory process of interpretation (see above) with special emphasis on the relationship between the subject of study and its context (part and whole). It is related to the "principle of contextualisation", which suggests to reflect upon the social and historical background of the studied phenomenon – in order to gain a better understanding of its emergence. In contrast to behaviourist research, the "principle of interaction between the researchers and the subjects" recommends interacting with people in the domain of research. It is based on the assumption that interaction fosters a more differentiated appreciation. The "principle of abstraction and generalisation" is especially important: It recommends aiming at more general knowledge by abstracting from the studied case. This is supposed to be accomplished by applying the first two principles. The "principle of dialogical reasoning" proposes that a researcher should reflect upon the specific assumptions underlying his perspective on the domain of research. This includes identifying relevant prior knowledge or prejudgement and evaluate their effect on the process of interpretation – in order to overcome this source of bias as much as possible. The "principle of multiple interpretations" is also aimed at fostering a less biased interpretation. It suggests taking into account multiple viewpoints and the related interpretations to foster a trans-subjective perspective. Finally, the "principle of suspicion" – which refers explicitly to critical theory - demands a critical attitude towards the perception and conceptualisation of social settings: They may be implied by distorted communication, which is an effect of ideology and various forms of domination.

The recommendations made by Klein and Myers are certainly helpful for developing an elaborate conception of hermeneutic research in ISR. However, their guidelines are somewhat diluted by concessions made to the behaviourist paradigm: Often, they speak of "data", a term that is not characteristic for a hermeneutic approach. The so called 'grounded theory' recommends integrating behaviourist research with a hermeneutic approach [GlSt67]. It suggests preparing the collection of data through achieving an explorative understanding first. Interpretations are to be tested against further data.

Action research is a further approach that builds on hermeneutics. It gained remarkable attention, mainly in the UK [BaWo98]. Action research is based on the assumption that processes of change are especially well suited to gain a deep understanding of social systems. Therefore it proposes that researchers participate in processes of change. Hence, it promises a deeper understanding and fostering progress in practice at the same time. Obviously, this supposed coincidence fits the 'research through development' approach often used in WI very well.

With respect to the peculiarities of ISR, a hermeneutic approach offers interesting prospects. It does not believe in invariant laws of social action. In other words: It assumes that social systems are contingent. Therefore, it is aimed rather at idiographic than at nomothetic knowledge, i.e. at a differentiated appreciation of single cases. It seems plausible that interacting with practitioners fosters the recognition of aspects relevant for adequate understanding. This is especially the case for action research, which allows studying how people think and act in the context of developing information systems. Also, it supports the dissemination and evaluation of possible new worlds within development projects. The principles suggested by Klein and Myers contribute - despite their partial overlapping – to conducting and evaluating hermeneutic research. With respect to the objective of this research report, grounded theory is relevant, too, since it suggests the integration of behaviourist and interpretative research, which is similar to configuring a research method from behaviourist and hermeneutic elements.

Despite its promising perspectives, a hermeneutic approach suffers from severe epistemological problems. They can be illustrated by four interrelated obstacles: *vague conception of "scientific" understanding*, *lack of standards for presenting and justifying interpretations*, and *dilution of scientific competition*, *lack of concepts for analyzing the IT artefact*. Additionally, there is one more possible threat that is related to the specific *Weltanschauung* of researchers who prefer a hermeneutic approach: *reproduction of cultural chasm*.

Vague Conception of "Scientific" Understanding

While the assumption that there is no objective truth of interpretations concerning social phenomena seems plausible, it has its back side, too: There is no conception of research that is aimed at understanding. This concerns the subject of research: Is it sufficient to focus on single cases with supposedly idiosyncratic features? Also, it is not clear when a satisfactory level of understanding is achieved. It is certainly not convincing to leave this decision to the taste – and ability – of single researchers. Guidelines as the one suggested by Klein and Myers provide some support. However, without comprehensible standards, there is the clear danger of producing interpretative research, which is not more than an informed description of a case study.

Lack of Standards for Presenting and Justifying Interpretations

The idea of understanding is related to the notion of intentionality. That means that certain terms used in interpretations cannot be defined in an extensional way. Hence, understanding them depends on comparable experience as human being. This thought implies two challenges. Firstly, the result of an interpretation has to be presented in terms that transport the intended meaning. Without knowing the potential readers' background, this is hard to accomplish. A narrative style may be appropriate for this purpose. However, it requires the skills of a sensitive writer and an idea of scientific relevance. Otherwise, it can produce narrative descriptions of insignificant "stories" observed in case studies. Understanding idiosyncratic cases only does hardly qualify as scientific. However, it is not clear how understanding of single cases can be abstracted to more general knowledge. Even more important is the problem that occurs with the justification of generalized understanding. The idea of an adequate interpretation can be related to the consensus theory of truth and to the coherence theory of truth. Both come with specific obstacles (see 6.1).

Dilution of Scientific Competition

Without standards that guide the justification and comparison of interpretations, hermeneutic research is likely to produce schools of thought. With each school claiming the exclusive ability for the appropriate interpretation of social reality and developing its own terminology or jargon, an exchange of ideas across schools or even a constructive competition are seriously obstructed. While one could regard such a situation as an expression of a pluralistic conception of science, I regard it as a serious threat, since it tends to replace the free competition for superior knowledge by ideological thinking.

Lack of Concepts for Analyzing the IT Artefact

Hermeneutics are focussed on humans, their thoughts, world views and their interactions within social systems. While this is an important part of the subject studied in ISR, it does not include the IT artefact, such as software systems, conceptual models etc. For this part of the ISR subject, it is not sufficient to aim at interpretations. Instead, it is required to develop and use concepts that allow for a comprehensible description of IT artefacts. This may include the adoption of computer science terminology. Hermeneutics does not provide any support for the development of an adequate ISR terminology to conceptualize the IT artefact.

Reproduction of Cultural Chasm

A hermeneutic approach to research is motivated by the supposed peculiarities of the humanities or the social sciences. Therefore, it is regarded by some as a characteristic feature of their conception of science – in contrast to the natural sciences – and their identity as scholars of the humanities. Sometimes, this attitude is accompanied by a clear mistrust against the natural sciences and technology. In an autobiographic statement, Georg Henrik von Wright stresses this position:

> "Though in my youth I had been a positivist of a sort, I had never shared the belief in 'progress' through the advancement of science and diffusion of knowledge which has been the ethos of the positivist tradition. My humanist attitudes had been connected with a pessimistic view of reform and a skeptical view of the implications of science and technology for society."[1]

[1] taken from the Pegasos website (http://www.kirjasto.sci.fi/gwright.htm) on 12-29-06, which refers to "the intellectual autobiography of Georg Henrik von Wright" published in: Schlipp, P.A.; Hahn, L.E. (Eds.): The Philosophy of Georg Henrik von Wright. 1989

In ISR, such an attitude constitutes a severe problem. It is one of the core objectives of ISR to foster communication between business people and IT specialists, who are often separated by a cultural chasm that causes friction and a waste of resources. Hence, ISR can be regarded as a moderator between those who develop IT artefacts and those who deploy them. Obviously, a hostile attitude against technology would be contra productive. Note, however, that this attitude is not necessarily related to hermeneutic approaches. However, the dominance of the positivist paradigm may have fostered the frustration of those who feel committed to a hermeneutic approach. As a consequence, some may tend to denigrate positivist research ([Webe04], p. III). To summarize, a hermeneutic approach to ISR offers some promising perspectives. Taking advantage of them recommends taking into account the specific pitfalls as well. This requires reflecting upon standards and adequate actions for promoting competition.

7.3 "Design Science" in Information Systems

The dissatisfaction with behaviourist research in ISR has not only inspired the adoption of hermeneutic approaches. Furthermore, it attracted attention towards construction-oriented research as it is common in WI (see 3.1). It is based on two assumptions: Firstly, the design of artefacts can be a sophisticated task that contributes to the development of new knowledge on a scientific level. Secondly, the scientific design of artefacts is supposed to require a specific research method. Probably the most prominent proponent of establishing research disciplines that do not follow the model of the natural sciences is Simon. He differentiates between the natural sciences and other sciences that deal with man made artefacts. He does not call those an art, but the "sciences of the artificial" [Simo96]. However, his proposal is focussed on research on artificial intelligence. Also, he does not take into account essential epistemological questions, e.g. how to achieve a satisfactory scientific justification of a particular design. In IS, March and Smith are among the early proponents of so called design-oriented research [MaSm95]. They did not, however, attract much attention. This changed clearly with an article by Hevner, March et al. that was published in the most respected IS journal [HeMa+04]. The article presents a framework that is supposed to guide conducting, presenting and evaluating design-oriented research. To emphasize a scientific claim, they speak of "design science".

Hevner et al. propose eight guidelines for design science projects. The first principle stresses the goal of design-oriented research: It should produce a "viable artefact" such as a "construct, a model, a method, or an instantiation." The problem targeted with an artefact should be relevant (guideline #2). According to Hevner et al. a problem is regarded as relevant, if it actually exists in praxis and is given a high priority. Any artefact developed in design science should be thoroughly evaluated (guideline # 3). This includes demonstrating its "utility, quality, and efficacy … via well-executed evaluation methods." The fourth guideline stresses the contribution to the scientific body of knowledge. It should be "clear and verifiable … in the areas of the design artefact, design foundations, and/or design methodologies." Guideline #5 demands the application of "rigorous methods" for constructing and evaluating the designed artefact. Guideline #6 ("design as a search process") suggests to "utilize available means to reach desired ends while satisfying laws in the problem domain." This guideline seems to be somehow redundant: How would you build an artefact without using "available means" and without reaching for "desired ends". The final guideline focuses on the dissemination of research results. It proposes to communicate research results effectively "both to technology-oriented as well as management-oriented audiences."

While the guidelines presented by Hevner et al. have contributed to a better reception of design-oriented research in IS, they suffer from various flaws and misconceptions (for a comprehensive analysis of the design science conception see [Zele07]). Those include an *insufficient conception of* *a scientific foundation*, a *mechanistic world view*, the *lack of appropriate concepts for describing IT artefacts*, and the *lack of accounting for possible future worlds*.

Lack of Accounting for Possible Future Worlds

A pivotal motivation for designing artefacts within ISR is to inspire decision makers in practice by demonstrating how possible future worlds could look like (see 5.3). However, Hevner et al. propose focussing on those problems only that are already perceived as relevant in practice: "Artifacts must accurately represent the business and technology environments used in the research, information systems themselves being models of the business." ([HeMa+04], p. 87). This contributes to an affirmative character of research, which is focussed on reproduction rather than on innovation. It also promotes conformist behaviour: Only topics that are regarded as relevant by others, are suitable. At the same time, this proposal contradicts another statement made by the authors: „… a theory of design in information systems, of necessity, is in a constant state of scientific revolution." (p. 81).

Insufficient Conception of a Scientific Foundation

On the one hand, Hevner et al. claim that a behaviourist approach is not sufficient. On the other hand, they do not reflect on its basic ontological and epistemological assumptions. Therefore, it is not surprising that they do not aim at developing a conception of science that fits the specific needs of a design-oriented approach. Instead, they take essential aspects of the behaviourist approach as a model for design science as well: Results produced by design-oriented research are to be tested against reality, which corresponds to the claim for empirical studies in behaviourist research. This ignores the need for developing future worlds (see above) as well as the problem that innovative artefacts may need some time to get accepted in practice. Hence, it leaves the evaluation of research results to practice – although the adoption of artefacts in practice may depend on various factors including marketing and political circumstances. The lack of a differentiated comparison of the specific epistemological requirements related to behaviourist research and design science has one further effect, which is relevant for the objective of this report: Without knowing these differences, it will not be possible to develop a solid foundation for integration or configuration.

In addition to empirical tests, Hevner et al. suggest other "rigorous" evaluation methods. For this purpose they refer to procedures used in system development – such as the application of metrics and test routines. These can be applied either to a single application of an artefact or to a field study, which includes several cases. In addition to that, evaluation can be supported by creating and discussing possible application scenarios. The state of the art ("existing knowledge base") is a further reference for evaluating an artefact. The main purpose of judging an artefact is related to its quality for being suitable to fulfil the intended purpose. Furthermore, it should be suited to enhance the discipline's knowledge base. However, it is not clear how this can be tested. An artefact should allow for solving problems that had not been solved so far (p. 81). But it remains unclear what that implies from a scientific point of view. As an example, Hevner et al. refer to expert systems. They claim that at first it was uncertain whether they were feasible at all (p. 84). However, from the viewpoint of formal logics, the feasibility of expert systems was no question. A similar assessment applies to other examples given by the authors (p. 87).

Mechanistic World View

The proposal made be Hevner et al. is based on the assumption that there are precise definitions of requirements to be met by artefacts. Also, it includes the presumption that there is a – somehow given – space of possible solutions. For analyzing this space, the authors propose "heuristic search" strategies: "Heuristic search strategies produce feasible, good designs that can be implemented in the business environment." (p. 88). Such a view is misleading for two reasons. Firstly, it ignores completely the fact that requirements are contingent. Secondly, it is certainly not true that applying heuristic search strategies will always produce satisfactory results as suggested by the authors.

Lack of Appropriate Concepts for Describing the IT-Artefact

ISR deals with IT artefacts, which requires a corresponding terminology. This is especially the case for design-oriented research, because it cannot abstract from the construction of an artefact. Hence, it needs concepts that allow for the adequate description of design decisions and of relevant features of the artefact. Various statements give the impression that Hevner et al. are not too familiar with information systems design. Referring to Simon, they suggest focussing on means, ends and "laws existing in the environment". "Ends are represented using a utility function and constraints that can be expressed in terms of decision variables and constants." (p. 89) While this terminology is common in artificial intelligence, it is not appropriate for designing information systems. Also, they request a "formal" representation of the artefact (p. 82). They do not explain what they mean by "formal". However, it seems that they think of a formal specification. While this is certainly nice to have, it will often not be feasible with respect to limited resources. It should be mentioned that this request contradicts a statement, they made earlier: "Such artifacts are represented in a structured form that may vary from software, formal logic, and rigorous mathematics to informal natural language descriptions." (p. 77)

To summarize: The article by Hevner et al. is one of the rare and therefore important contributions to the discussion on enhancing ISR with design-oriented research. However, it does not provide a convincing methodological foundation. Note that this is not to be mistaken as a verdict against design-oriented research in general. It only implies that more work is needed to establish a scientific conception of design-oriented research.

7.4 Requirements for Achieving a Scientific Foundation

None of the methods proposed for ISR is convincing. This result is a further challenge to the conception of ISR as a scientific discipline. Therefore, advocating a scientific model remains merely a confession as long as there is no convincing orientation for a scientific approach to ISR. In order for ISR to be conceived as a scientific discipline, a number of requirements need to be satisfied:

R1 - Generic conception of science: Apparently, it is not satisfactory to merely copy the model of the natural sciences. However, if one does not assume that science is restricted to the approach taken by the natural sciences, there is need for essential features of scientific research, which could then be adapted to the specific requirements of particular disciplines – such as ISR. If this is not possible, it will make no sense to pursue a scientific profile.

R2 - Support for the evaluation of artefacts and outlines of possible future worlds: If design-oriented research is regarded as an important part of ISR, there is need for criteria that can be applied to evaluate and compare corresponding research results. This includes taking into account the peculiarities of the artefacts that are subject of ISR: information systems, which are constituted through language. Among other things, there is need for adequate concepts to describe and analyze information systems.

R3 - Support for tailoring research methods: Since the research methods considered in section 7 focus on different aspects of the same research subject, one can assume that they can enrich one other. Therefore, it is required to support the configuration of research methods that fit the peculiarities of a particular research project. With respect to the diversity of ISR, this should cover a wide field of topics, including cross-disciplinary research. Method configuration should be based on a unifying generic conception of science (R1). Furthermore, it requires common concepts for describing the essential features of the various methods to be integrated – in other words: There is need for a common meta model.

R4 - Support for the documentation of research results: With respect to the diversity of the field and the ever growing flood of publications, there is need to guide the documentation of research results. The documentation should be aimed at fostering the access to particular research results and at improving their comparability. This would contribute to the recognition (or reconstruction) of scientific progress and to the evolution of a coherent body of knowledge.

8 A Generic Conception of Science

A conception of science, researchers from most, if not all disciplines can agree on, is not only required for justifying ISR as a scientific discipline. Furthermore, it would serve as a common foundation for the configuration of individual research methods (R3). By emphasizing common roots, it could also contribute to overcoming the cultural chasm and the mutual suspicion that compromises the potential of multi-perspective approaches. In this sense, Weber suggests to overcome existing differences in research perspectives: "It is time for us to move beyond labels and to see the underlying unity in what we are trying to achieve via our research methods. The commonalities in my view are compelling and paramount." ([Webe04], p. xii). However, he does not provide an elaborate description of what these commonalities might be – except for suggesting that all researchers, independent from the method they prefer, "… are concerned with trying to enhance their understanding of the world (whatever the world might be)." ([Webe04], p. vi).

8.1 Common Grounds: Essential Characteristics of Scientific Research

Similar to terms such as 'Organisation' or 'Management', science denotes both, an activity and an institution. The activity is aimed at producing knowledge that qualifies for being "scientific". It is distinguished from other activities that are aimed at knowledge creation by the procedures it applies and by its results, i.e. the specific qualities of the knowledge it produces. The institutional aspect of science is, different from 'organisation' or 'management', a more abstract concept. While people can become official members of an institution like a university, science is usually regarded rather as a virtual community of those, who feel committed to certain ideas and rules. Science in this sense can be conceived as a specific "Weltanschauung" or way of life (see for instance [Mitt82]). It is characterized by the creation of new, superior knowledge and – often, but not necessarily - by the claim that this knowledge serves as an orientation for successful action. Also, science is related to objectivity and marks a clear contrast to subjectivity or belief. However, other groups or institutions in society claim to provide superior knowledge as well. Therefore, it is required to have a closer look at features of science that allow for distinguishing its results from the "dreams of a ghostviewer" (Kant).

Our brief analysis of ontological and epistemological positions as well as of selected schools in philosophy of science (see section 6) has shown that there is a remarkable diversity: There are different concepts of truth and justification and – as a consequence – different conceptions of science. These differences have inspired the construction of research methods, which are currently used within ISR (see section 7). But only if there are common grounds of different schools and flavours of science in ISR, it will be possible to configure specific research from existing elements: Configuration in this sense implies integration, which in turn requires common concepts. It seems that these commonalities exist. They are independent from a particular epistemological approach or research method. On the one hand, they are related to characteristics that distinguish scientific knowledge from other forms of knowledge and on the other hand, they characterize the process or context of scientific research. Firstly, we will have a look at three essential postulates that are characteristic for the results of scientific research, i.e. for scientific knowledge: *originality*, *abstraction* and *justification*.

The Abstraction Postulate

The term 'abstraction' designates both a process and the result of this process. It is at the core of scientific research. Scientific recognition is not just aimed at describing single instances, e.g. a particular firm or a specific information system. Instead, it is focussed on more general features or patterns that apply to a whole range of instances. The act of scientific abstraction requires a purpose, e.g. fostering the recognition of certain phenomena. Abstraction is aimed at concepts that should serve this purpose. These concepts are an abstraction (as a result of the process). The process of abstraction includes three different activities. One is aimed at the identification of common features, which are shared by a targeted set of (recognition) objects. Hence, it is a generalisation over sets of objects resulting in classes (or concepts). We can call it an abstraction *to higher level concepts*. The second activity is aimed at selecting those features that are essential for the purpose the abstraction should serve. If, for instance, a generalisation results in the concept of a 'business software developer', one would then have to decide, which features of people who belong to this class can be neglected, because they are of no relevance for the given purpose. Hence, we can call it abstraction *from the irrelevant*. The third kind of abstraction is related to the selection and conceptualisation of features. We can call it abstraction *from the possible*. Take, for instance, 'age' and 'formal qualification' of the concept 'business software developer'. Both features can be conceptualized by defining the range of possible values (as well as the type of the value). The corresponding concepts – as well as the resulting concept of 'business software developer' – are not only an abstraction over existing instances, but define a class of possible instances. Hence, an abstraction to commonalities results in concepts that allow for abstracting from – actual and possible – variety. Note, that the three kinds of abstraction are so tightly intertwined that they are often not distinguished consciously. They are different faces of the same process.

For the course of our study, it is important that abstraction is not restricted to commonalities of actually existing instances. 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. This aspect of science corresponds to the original meaning of the Greek *theoria*: outlook, contemplation. Hence, the demand for abstraction recommends taking into account the possibility of other worlds as well. In the natural sciences scenarios of possible – not (yet) detectable – worlds are aimed at developing more powerful theories that may contribute to a more comprehensive explanation of natural phenomena and their evolution – as well as to the actual construction of worlds that one would not have thought to be possible. Consider, for instance, the theory of special relativity. It includes statements about worlds that are possible through high velocities – producing effects that are beyond our experience (and maybe even beyond our imagination). Typically, these possible worlds exist in parallel to the observable world. Hence, they may provide an explanation for phenomena in the existing world, which may be regarded as a special case of a more general possible world. In the social sciences, the existence of possible worlds is more apparent: Factual social systems are characterized by specific cultural, historical and economical factors that might not only change in time, but also between different groups or societies in the existing world. In addition to that, other worlds are possible through individual and social change. In application-oriented disciplines that deal with a contingent subject, focussing on observable phenomena only fades out possibly superior alternatives and may therefore contribute to naturalistic fallacies (see 7.1). In logics, the concept of possible world has been introduced to deal with the restrictions of the dualistic concept of truth ('tertium non datur') – or in other words: to deal with contingency. While a proposition in a particular possible world needs to be either true or false, it can be assigned a different truth predicate in another possible world. Based on this conception, modal logics allows for assigning confidence levels of truth – depending on the number of worlds, for which a proposition is true.

The Originality Postulate

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. It seems this assumption is valid for all directions of philosophy of science. There may be a difference with respect to the effect of culture on the validity of scientific knowledge: While critical rationalism is aimed at objective knowledge that is independent from the background of the interpreter, hermeneutic approaches stress that interpretation is a process, which relies on the cultural background of the participants. This does not, however, refute the hypothesis that the claim for originality is characteristic for the idea of science. Whether or not a thought is original cannot be decided for sure: Even if one could ask every living person on earth, it would not be sufficient, since it is possible that somebody who does not live anymore had developed this thought already. As a pragmatic consequence, only knowledge that was published or made available somehow is taken into account for judging the originality of a statement.

The Justification Postulate

Propositions can be abstract and original without being scientific. For instance: "All dogs are reborn as human beings". For a hypothesis to qualify as scientific, it needs to be supported by convincing reason. As a default, scientific justification is aimed a producing evidence for the truth of a proposition. This requires a concept of truth as well as procedures, which allow for testing it – preferably in an inter-subjective way.

With respect to the lack of a common notion of truth (see 6.1), it is questionable whether justification can serve as a generic characteristic of science. There is a clear difference between a justification procedure that makes use of the correspondence theory of truth and another one that is based on the consensus theory. In the first case, a proposition is to be tested against observations of reality. In the second case, its truth is evaluated in a merely discursive procedure. However, a closer look at the corresponding schools reveals that these justification procedures are not mutually exclusive. There is no doubt that empirical evidence, i.e. the application of the correspondence theory of truth, will usually serve as a good argument in discursive justification procedures. At the same time, critical rationalism depends also on judgements, which cannot be justified through empirical evidence. This is e.g. the case for the indispensable decision whether the (preliminary) confirmation of a proposition is sufficient (How many cases would one need? What is the minimum correlation coefficient required?): "Confirmations should count only if they are the result of risky predictions ..." ([Popp63], p. 35) – which requires evaluating whether a prediction is risky enough. As a consequence, Popper asserts that sometimes comparing the refutability or the level of confirmation of two propositions cannot be performed in an objective fashion ([Popp82], p. 213). While critical rationalism does not regard a discursive approach as sufficient to produce a scientifically convincing solution to this evaluation problem, its proponents are practicing discursive evaluations of their base judgements, e.g. for their conception of science or of an "open society". Since there are no convincing alternatives for justifying statements that cannot be tested empirically, I do not see any reason for excluding discursive judgements – as long as they satisfy certain requirements like the ones proposed by critical theory or the Erlangen constructivism – from the conception of science. In order to avoid terminological confusion – and friction between proponents of different schools, it might be helpful to differentiate between truth and adequacy.

Furthermore, we can assume that both, proponents of the correspondence theory and the consensus theory will not entirely refuse to apply the coherence theory of truth. While coherence is probably not regarded as sufficient by all schools considered in this report, it provides – for pragmatic reasons – an important criterion for scientific reasoning: A proposition coherent with a given body of knowledge can be regarded as plausible until further evidence rejects this assumption.

8.2 Core Elements of Scientific Culture

In addition to these features that are characteristic for scientific knowledge, there are other features essential for the idea of science. They are related to the preconditions, preferences, commitments and instruments of science – in other words: to its culture. The culture of science is mainly characterized by *critique, freedom*, *language* and *transparency*. These features cannot be separated from originality, abstraction and justification. Only, they stress a different perspective and additional aspects, which are relevant for a suitable conception of science.

The Critique Postulate

The idea of science is characterized through significant criticism or scepticism – against any statement that is not justified sufficiently. Critique is essential for all schools of philosophy of science we considered in 6.2. The founders of two schools, critical rationalism and critical theory, regarded it as so characteristic that they made the attribute 'critical' part of the designation. At the end of his "Critique of pure Reason" Kant emphasized the outstanding relevance of critique for scientific discovery – and for the satisfaction of human reason: "Only the critical path is still open." (translated from [Kant76], B 884) Emphasizing critique as part of a scientific culture has a number of implications. It does not only recommend a sceptical attitude against commonly accepted wisdom and against research results. Furthermore, it comes with the claim that nothing is excluded from critique – as long as critique is backed by conceivable reason. Hence, critique may also be directed against the conception of research methods or basic epistemological and ontological assumptions.

A commitment to critique also recommends exposing research results to critique – by making all underlying assumptions explicit and by using terms that allow for testing them. Protecting statements against critique, e.g. through "ceteris paribus" clauses should be avoided unless it serves a reasonable purpose – such as simplifying a complex matter in order to foster its analysis. At the same time, it would hurt the claim for critique, if a particular research method is prescribed in an apologetic way. In order for critique to work, it is required that it is not only possible, but also appreciated: Critique should not be regarded as a personal offence, but rather as a contribution to the evolution of knowledge.

The Freedom Postulate

Critique as a pivotal driver of knowledge implies the idea of freedom. On the one hand, freedom refers to the lack of dominion: Everybody is free to express his judgement – as long as he is able to support it with conceivable reason – without the threat of sanctions. In an ideal conception of science, there is no force. No viewpoint is preferred over others because of the position of those who represent it. There is only the "peculiarly forceless force of the better argument" (translated from [Habe81], pp. 52). On the other hand, freedom also relates to emancipation, which means to free oneself from previous worldviews, hence to transcend the world as it is and to look curiously and in an unprejudiced manner for more appropriate ways to conceptualize and organize the (social) world. Emancipation of this kind is a remarkable challenge: Scientists are often influenced by a paradigm, which is related to certain perspectives on and conceptions of their research subject. On the one hand, the familiarity with a paradigm fosters research productivity, on the other hand, it contributes to a *deformation professionelle*, which would be a threat to the demand for abstraction. Especially in disciplines that deal with contingent subjects, this implies the danger of being caught by the trap of the naturalistic fallacy: the factual appearances – of contingent subjects – have a deep, however subtle impact on the perceptions and conceptualisations that guide research approaches. As a consequence, factual appearances, although at least in part contingent, are taken for granted.

The Language Postulate

Language is at the core of science. It is the pivotal instrument of scientific studies and the medium to present and evaluate research results. While there is no doubt that not any kind of language qualifies for being used in science, there is no consensus on how an adequate language for science should look like. An ongoing debate is focussing on the question whether science requires the use of formal languages. While some schools of philosophy of science, such as logical positivism or the Erlangen constructivism propose the use of a formal language for the presentation of research results, critical rationalism does not recommend it explicitly (see 6.2). Only the critical theory and – usually - hermeneutic approaches do not regard formal languages as appropriate. It is characteristic for a widespread comprehension of the natural sciences that research results are achieved through the use of mathematics, which requires deploying a formal language for representing the domain that is being studied. In praise of mathematics, Hilbert takes this thought to an extreme: He suggests that any scientific study, provided it is mature enough for developing a theory, depends on the use of mathematics ([Hilb70], p. 156). This judgement is contrasted not only by those who do not agree (like proponents of hermeneutic research), but also by the vast amount of actual research in many disciplines that does not use formal languages. Nevertheless, one can conclude that a language used in scientific research should be as precise as possible. This is an implication of the demand for critique: The smaller the range of possible interpretations, a proposition leaves, the better the chance to refute it.[1] This aspect allows for distinguishing between 'strong' and 'weak' abstractions: An abstraction that applies to a wide range of instances, but makes use of vague concepts can be regarded as weaker than another one that applies more precise concepts.

Against this background, it is not possible to identify a specific language that is characteristic for science in general. However, with respect to the crucial role, language plays in scientific research, a conscious use of language is characteristic for science. That recommends thinking about the proper language, e.g. formal vs. informal, for specific research questions – and to justify one's choice with comprehensible reason. It also recommends reflecting upon the epistemological impact of language. This is related to the demands for abstraction, critique and freedom: Research depends on language. At the same time, language determines the access to the research subject and its conceptualisation. Although we are able to reflect upon language, for instance by distinguishing between object and meta level language, our ability to speak and understand a language is commonly regarded as a competence that we cannot entirely comprehend ([Lore96b], p. 49). Therefore any research that either aims at analysing a language and its use or at inventing new "language games" (i.e. artificial languages and actions built upon them), has to face a subtle challenge: Every researcher is trapped in a network of language, patterns of thought and action he cannot completely transcend - leading to a paradox that can hardly be resolved: Understanding a language is not possible without using a language. At the same time, any language we use for this purpose will bias our perception and judgement – or, as the early Wittgenstein put it: "The limit of my language means the limit of my world." ([Witt81], §5.6).

The Transparency Postulate

Developing an argument to justify a certain thought will usually include suppositions, some of which seem to be so plausible that one tends to take them for granted. The transparency postulate demands for a more careful and restrained argumentation. It can be derived from the justification postulate and the critique postulate. The transparency postulate recommends making all non-evident suppositions within an argument explicit. This includes e.g. hypotheses that influence the design of a field study, hypotheses that guide the specification of requirements to be met by a design artefact

[1] This corresponds to Popper's demand for high information content, any scientific proposition should fulfil.

etc. These hypotheses can be introduced as working hypotheses without further test of their truth. Alternatively, their validation may be part of the research as well.

8.3 Theories and Hypotheses

Theories are often regarded as the core outcome of scientific research. They are closely related to the concept of explanation. There is no uniform notion of an empirical theory. Against the background of the essential characteristics of scientific research discussed so far, I suggest the following idealized concept:

A theory is comprised of a set of consistent propositions, which fulfil the following requirements:

- The set is minimal in the sense that none of the propositions can be omitted without compromising the substance of the theory.
- The propositions are expressed in a precise language that fosters inter-subjective comprehensibility.
- The propositions need to satisfy the claim for abstraction. Hence, they apply to classes of phenomena, not just to particular instances.
- They allow for refutation with respect to an accepted concept of truth.
- They include a description of the domain, they apply to.
- They are generic in the sense that they cannot be deducted from other known propositions.
- They are justified through an accepted justification procedure, which is aimed at an accepted concept of truth.
- They have been confirmed in a number of cases.

Note that this concept is not precise enough to clearly discriminate a theory against other linguistic constructions. It is not specified, what confirmation in a number of cases means. While the concept recommends the use of a precise language, it does not imply the use of a formal language. Therefore, it may be difficult to show that a proposition cannot be deducted from known propositions. This is the case, too, for showing that the set of propositions is minimal. Hence, it is recommended relaxing this concept for pragmatic reasons. All demands that cannot be fulfilled for sure should be regarded as an orientation. For instance, the set of propositions should not include any proposition that is clearly redundant. Or: It should not be apparent that a theory can be deducted from other theories. In other words: A theory should appear to have an explanatory power of its own – which does not exclude that it will be reduced to other more powerful theories later on. This pragmatic relaxation of the concept applies to judgements concerning refutation and justification, too. To allow for a differentiated contemplation of theory proposals, I suggest differentiating between empirical theories and hermeneutic theories. Empirical theories allow for testing them through empirical studies, while hermeneutic theories are rather aimed at the correspondence theory of truth or the consensus theory of truth. I will refer to an empirical theory as theory, to a hermeneutic theory as interpretation. Note that this is an analytic differentiation. It is possible that an empirical theory makes use of an interpretation or the other way around (see 9.1).

The *scientific explanation* of an actual or potential empirical phenomenon is aimed at deriving it from a theory. This may be an elaborate, highly confirmed theory or a theory draft that has been applied to a few cases only. A *hypothesis* is a proposition that is introduced explicitly as a fallible assumption and that has not been confirmed to a satisfactory degree. It will usually refer to classes of phenomena. A *working hypothesis* is a hypothesis that has not been tested to a noteworthy degree so far. The propositions that constitute a theory can be regarded as hypotheses. However, once a theory has been confirmed to a higher degree, its propositions are often regarded as true - for pragmatic reasons.

9 Multi-Perspective Information Systems Research

The analysis of research methods in ISR has revealed particular strengths and weaknesses. That confirms the assumption that motivates this report: Due to the diversity of research topics and objectives in ISR, mono-paradigmatic research is not sufficient. At the same time, it is not a trivial task to combine different research methods in order to provide adequate support for a particular research project – mainly for the epistemological and cultural chasm that separates them. The occasional claim for a pluralistic use of methods is usually not aimed at the configuration of methods but at replacing the dominance of the behaviourist paradigm in IS with a more liberal use of methods (see e.g. [KaDu88], [Gabl94]). Falconer and Mackay analyse some attempts to integrate "positivist" and "interpretative" methods [FaMa99]. They come to the conclusion that these attempts must fail as long as a researcher does not manage to apply a "neutral viewpoint" for selecting the appropriate method. They do not, however, guide this selection. Mingers suggests a pluralistic approach to combining IS research methods [Ming01]. It is based on concepts proposed by Checkland for the so called soft systems methodology [Chec81]: "Developing on work of Checkland (1981) we can conceptualize a research situation in terms of three notional systems: a researching system (RIS), a research-content system (RCS), and an intellectual resources system (IRS)." (p. 250) However, this approach remains dissatisfactory, because it is restricted to suggesting a framework for structuring research projects. Niehaves proposes a framework for "multi-method information systems research" [Nieh05]. It includes epistemological and ontological aspects, which are condensed into a set of questions that are supposed to guide researchers with comparing different research methods. While this may serve as an inspiration for selecting methods, the framework does not directly support the configuration of methods.

9.1 A Framework to Support the Configuration of Research Methods

Configuration is based on selecting and possibly adapting elements out of a given set of elements. The selected elements are then combined in order to serve a certain purpose. With regard to the configuration of research methods, there is need for a unifying conception of research method, which includes core concepts and the corresponding variety to choose from. It should cover all types of ISR and should describe integrity constraints that apply to the configuration of a particular research method.

The notion of research method suggested in section 7 suggests that a method should provide concepts to structure research projects as well as a process that guides the execution and documentation of a project. As core concepts to structure any kind of research project, I suggest to differentiate between the generic *epistemological contribution*, abstract and concrete *knowledge contribution*, its *representation*, the *criteria used to justify* the knowledge contribution as well as the corresponding *justification procedure*. I assume that there are two kinds of generic epistemological contributions only: *construction* and *critique*. Construction is focussed on the creation of new knowledge. In general, it is related to a research subject. The characteristic research subject of ISR is constituted by socio-technical systems, which are comprised of one or more information systems (i.e. artefacts) and the action system(s) they are embedded in. The term 'construction' emphasizes the fact that scientific knowledge is not to be confused with the description of concrete instances of some class; neither does it emerge from discovering new phenomena, from ingenious ideas only. Instead, there is always need for developing concepts, which are suited to express new insights on a high level of abstraction. Concepts can be used for developing theories or interpretations. They are aimed at fostering superior descriptions of the factual world (including existing artefacts), its possible states and – may be – possible state changes. Hence, we can call these generic knowledge contributions *abstractions of the factual*. On the other hand, concepts can target results of intentional human action. This applies to descriptions of interesting possible worlds, which can be realized through human action only, as well as to the description of how to accomplish them. We can call these constructions *abstractions of the intentional*. They include descriptions of artefacts and corresponding action systems, which have not been realized yet. Furthermore, constructions can be aimed at concepts that support the development of constructions. Examples would be modelling languages or problem solving methods. Note that there is no exact borderline between abstractions of the factual and those of the intentional. In physics, for instance, abstractions of the factual are constituted by powerful theories that do not only explain existing shapes of reality. Instead, they also cover a range of possible other states. Therefore, an artefact designed by an engineer can be explained by a corresponding theory. However, it cannot just be derived from the theory: The design of an elaborate artefact requires an intentional act that is directed against certain objectives (and additional intellectual achievements). This intentional act is out of the scope of physics. Nevertheless, abstractions of the intentional produced by an engineer are based on abstractions of the factual originating in the natural sciences. In ISR, the situation is different. Usually, there will be no theories available that would allow for explaining an abstraction of the intentional designed in a research project. Nevertheless, existing abstractions of the factual (e.g. concepts, hypotheses and 'weak' theories) have to be taken into account as well. This is related to the claim that the proposed design is not only feasible but also applicable. For instance, a reference object model that was designed for a certain domain, for instance, may come with the claim to be based on an improved organisation of the corresponding action system. Nevertheless, it will usually also include abstractions of existing action systems. As a consequence, constructions in ISR may be abstractions of the intentional and of the factual at the same time.

Critique[1] represents a generic epistemological contribution that is aimed a challenging or evaluating given knowledge contributions – resulting in new knowledge contributions such as refutation or confirmation of abstractions of the factual, the evaluation of abstractions of the intentional as well as challenging previous critique. Note that the distinction of construction and critique is made for analytical reasons: Usually, they cannot be separated completely. Critique will usually include constructions that substantiate the intended judgement. With respect to the claim for originality, any construction includes the implicit or explicit critique of competing constructions.

All research contributions are represented through language. This could be the natural language, a formal language or a modelling language. With respect to the subject of ISR, the construction of a formal language or a modelling language could be a knowledge contribution, which would require a representation using a meta level language, e.g. a meta meta model. Any research contribution needs to be justified. This requires selecting appropriate justification criteria, such as truth (for abstractions of the factual) or adequacy (for abstractions of the intentional). Since there is no unified concept of truth, the preferred theory of truth needs to be selected. Adequacy refers to the purpose, a construction is to serve. It may include assumptions that come with the claim for truth. The purpose, an abstraction of the intentional is supposed to satisfy, does not have to be justified. This is a consequence of the claim for freedom and the appreciation of scientific knowledge as an end in itself. However, in an application-oriented discipline such as ISR, it is a good idea to provide reason for the implied purpose as well. This could be done by pointing at the actual expression of a corresponding demand in practice. Note, however, that this would not mean to provide evidence for the truth of a purpose – only for its relevance for certain groups. In addition to selecting justifica-

[1] This meaning of the term should not be confused with critique as an essential feature of a scientific culture (see 8.2).

tion criteria, it is required to decide for testing procedures that correspond to these criteria. For instance, an experiment could be used, if one selected the correspondence theory of truth.

Figure 1 depicts a rudimentary meta model of the concepts to be taken into account for designing research methods. They should apply to any scientific discipline. Note that this is not a formal model that could be used to determine the set of possible instantiations. It is only intended to provide a guideline for instantiations that allow for a more elaborate description of options to design a research project in a particular discipline.

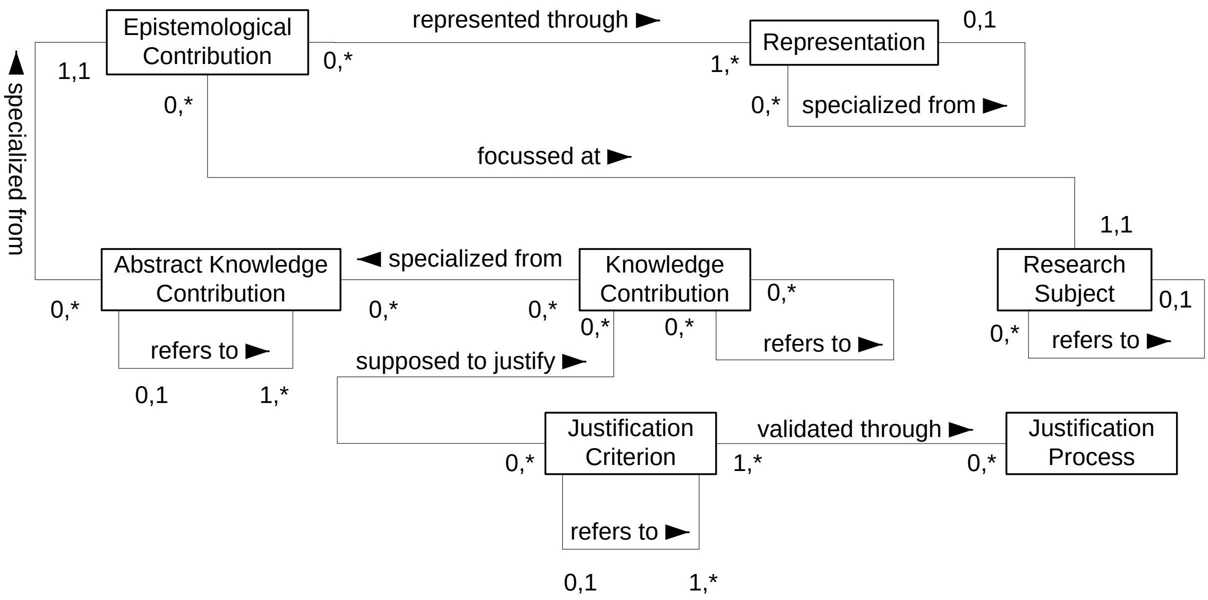

Figure 1: Meta model of Research Method

Figure 2 shows a conceptual model that is a first instantiation of the meta model. It comes with the claim to be suitable for any kind of scientific research. Colours are used to indicate the meta concepts, the concepts in the model are instantiated from.

Within the model, the generic epistemological contribution critique is not differentiated into much detail. This is not necessary because critique is aimed at constructions and – as a consequence – at the corresponding concrete knowledge contributions. Additionally, critique makes use of the same justification criteria that are possible for the constructions it targets. Note that critique can focus on previous critique. Therefore, it can aim at the refutation of a refutation, hence resulting in a confirmation. Abstractions of the intentional are primarily justified by showing their adequacy. Adequacy relates the abstraction of the intentional, e.g. a design artefact, to the purpose it should serve. It may, therefore, refer to hypotheses, which in turn may refer to the justification of the purpose, e.g. by claiming that it reflects actual requirements. They may also reflect assumptions on how a certain feature of the abstraction of the intentional is suited to serve the given purpose. Also, hypotheses can be used for an interpretation, i.e. the outcome of a hermeneutic reflexion on a phenomenon. Hence, they can represent both, an abstraction of the factual and an abstraction of the intentional. A prototypical implementation, sometimes referred to as 'proof of concept', can be used as an indication of adequacy. However, without embedding it within an action system – e.g. within a case study, it only demonstrates technical feasibility, which is often not a pivotal issue.

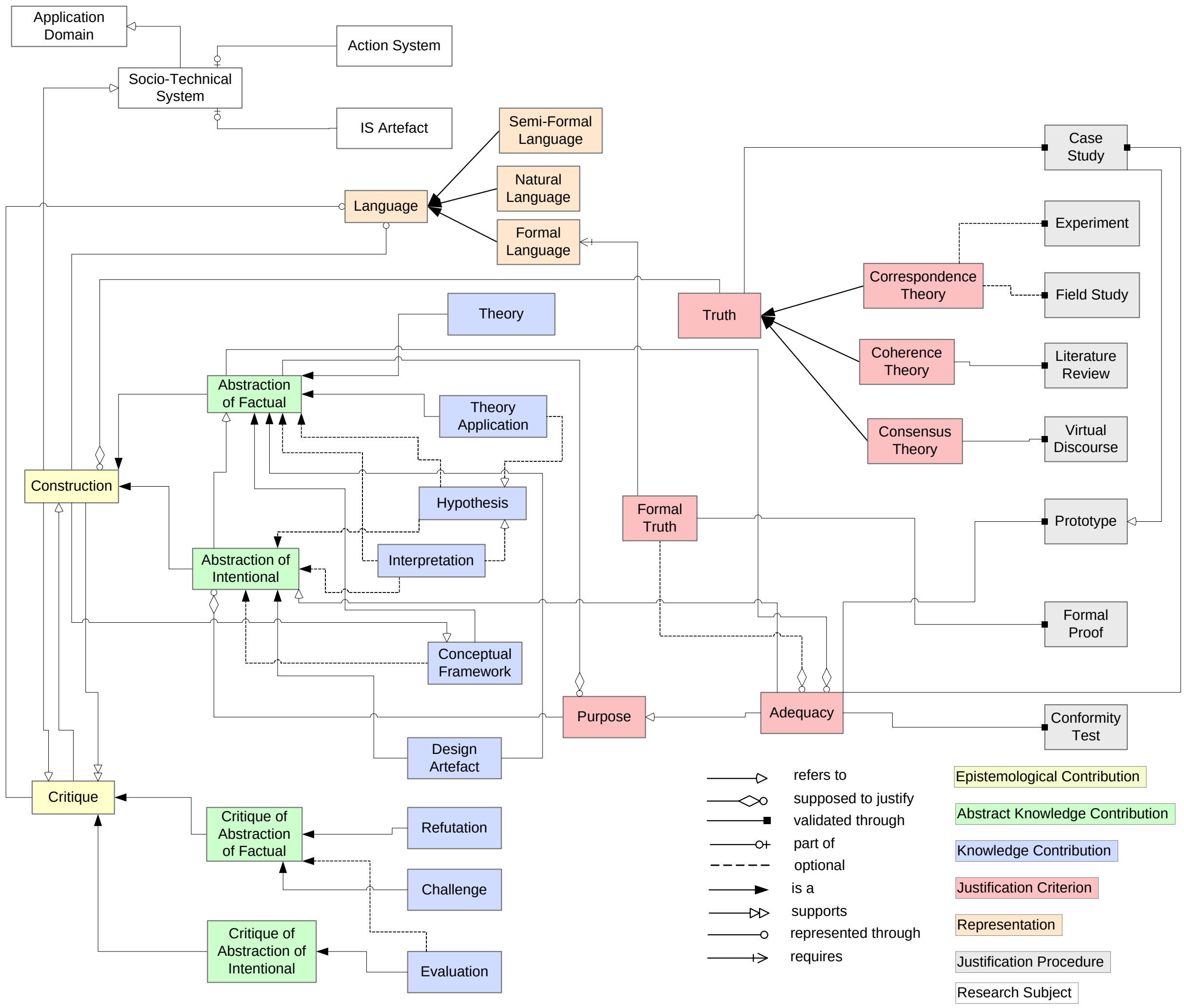

Figure 2: Conceptual Model to Guide the Configuration of Research Methods

A theory application is aimed at explaining a phenomenon by applying a theory that was originally created to other domains. In other words, it is aimed at extending the domain, the theory can be applied to. This requires projecting/adapting the propositions of the theory to the studied subject. It is supplemented by testing the truth of these propositions. Conceptual frameworks are an important part of research, both with respect to abstractions of the factual and abstractions of the intentional. They provide the abstractions that serve to conceive or to structure the research subject. In other words: They provide the concepts that serve as core instruments for scientific recognition and its dissemination. Giddens underlines the pivotal relevance of conceptual frameworks especially for the social sciences:

> "... the discovery of 'laws' ... is only one concern among others that are equally important to the theoretical content of social science. Chief among these other concerns is the provision of conceptual means for analysing what actors know about why they act as they do ..." ([Gidd84], p. xix)

Conceptual frameworks can also be aimed at guiding problem solving in practice by providing an appropriate structure of the problem domain. Note, that there is no clear borderline between a design artefact and a conceptual framework. A conceptual model, which may be at the core of a design artefact, can be intended as a conceptual framework. This is, for instance, the case for the conceptual model presented in Figure 2. Often, however, a conceptual framework is to serve a purpose that is different from that of a typical conceptual model: It should guide research rather than the construction of artefacts or possible worlds. Formal truth can be used as a criterion both for justifying certain properties that relate to the adequacy of a designed artefact. This requires the artefact to be described in a formal language. Then it is possible to use formal expressions for describing properties, the artefact should have or must not have. Showing the truth of these expressions requires a formal proof.

The three concepts of empirical truth portrayed in 6.1 can be tested with different procedures. The correspondence theory of truth requires a confrontation with reality, which could be achieved either through a field study or an experiment. A case study is a further option. However, it is suited only to demonstrate preliminary evidence. The same restriction applies to case studies that are used to demonstrate the adequacy of abstractions of the intentional.

9.2 Applying the Framework: Examples

As a consequence of its wide scope, the conceptual model presented above remains rather abstract. The following examples are intended to demonstrate its application in a more comprehensible way. They serve to reconstruct prototypical research types already common in ISR (see section 7). The instantiation in Figure 3 represents – in an idealized way – research that pursues the behaviourist paradigm. All concepts that are not required are omitted. Ideally, a behaviourist approach in ISR is focussed on certain phenomena in a particular class of socio-technical systems, which are not explained so far to a satisfactory degree. In order to develop an explanation, one or more theories are applied to the phenomena resulting in a set of hypotheses, which describe characteristics of the class of socio-technical systems. Consequently, the hypotheses are tested against reality referring to the correspondence theory of truth. This test is typically performed as a field study. For this purpose, the hypotheses need to be operationalized, which often includes their (partial) formalisation. In case, there is no appropriate theory available, a study may focus on formulating and testing hypotheses only.

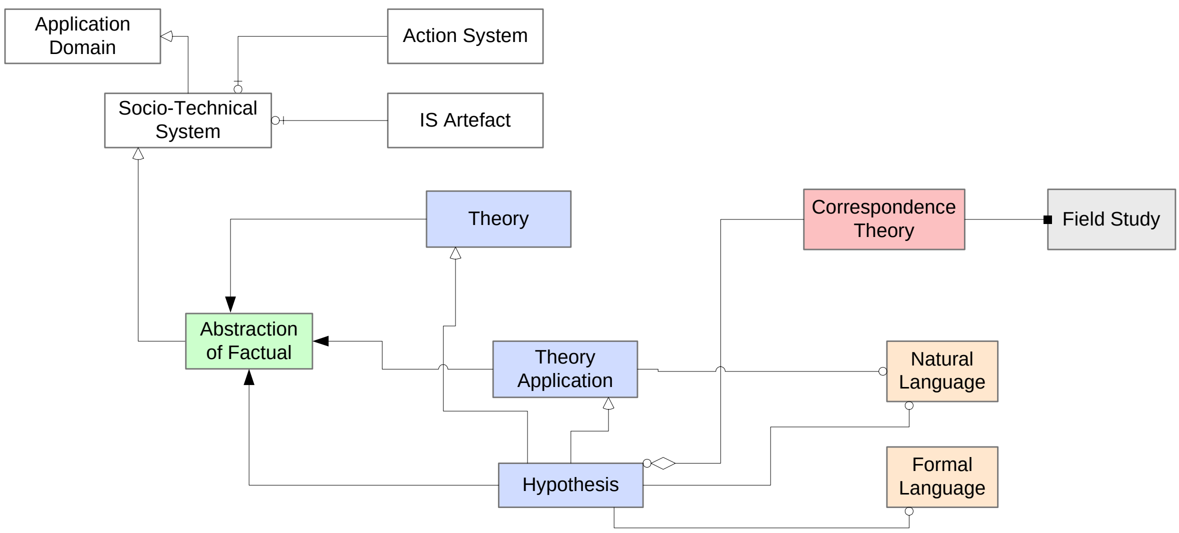

Figure 3: Conceptual Model of Idealized Behaviourist Approach in Information Systems Research

Hypotheses of this kind are intended to describe generic patterns with the observed domain that cannot be explained by any existing theory. Instead of applying an existing theory, it is – of course – possible to develop a new theory. In ISR, however, this seems to be extremely rare. It seems unlikely that a concrete behaviourist research project fits this idealized model. Often, the discussion of a theory as well as the introduction of corresponding hypotheses is accompanied by an interpretation. This interpretation may be justified by applying the coherence theory of truth, tested through a literature review, or the consensus theory of truth, tested through a virtual discourse with the prospective readers. Furthermore, it is possible that a conceptual framework is developed in order to structure the studied domain with respect to the theory that is to be applied. Figure 4 reflects these thoughts. It reconstructs a particular study [BaKo+04], which investigates, if and how the Internet improves business performance. The authors refer to the so called 'resource based view' theory, which suggests that competitiveness of a firm depends mainly on the value of its resources. To identify the relevant resources, they refer to the concept of 'net-enabled business transformation', which is comprised of improved financial and operational performance enabled by taking advantage of the Internet. Against this background, they develop a conceptual framework – which they call "conceptual model" ([BaKo+04], p. 589). It includes the core concepts identified by the authors to analyse relevant resources and their effects. Based on these concepts, they formulate a set of hypotheses that are applications of the resource based view to the business value of changes enabled by the Internet, e.g. "Higher levels of systems integration within a firm will be positively associated with greater levels of customerside online informational capabilities." The operationalisation of theses hypotheses includes the formalisation of some performance measures, such as revenue per employee, percentage of total business transacted online etc. The hypotheses are then tested through a field study. Both for the justifying the conceptual framework and for motivating their hypotheses, the authors draw intensively upon existing work (coherence theory of truth). Furthermore, wide parts of the paper include interpretations, which are either supported by referring to the literature or by performing a virtual discourse (consensus theory). Different from the empirical field study, these aspects of developing evidence are not treated explicitly as part of a research method. They are rather applied in an *en passant* style. Figure 4 shows the conceptual model that describes the corresponding research approach. The concepts that constitute the 'official' research method, i.e. a behaviourist approach, are framed with thick lines.

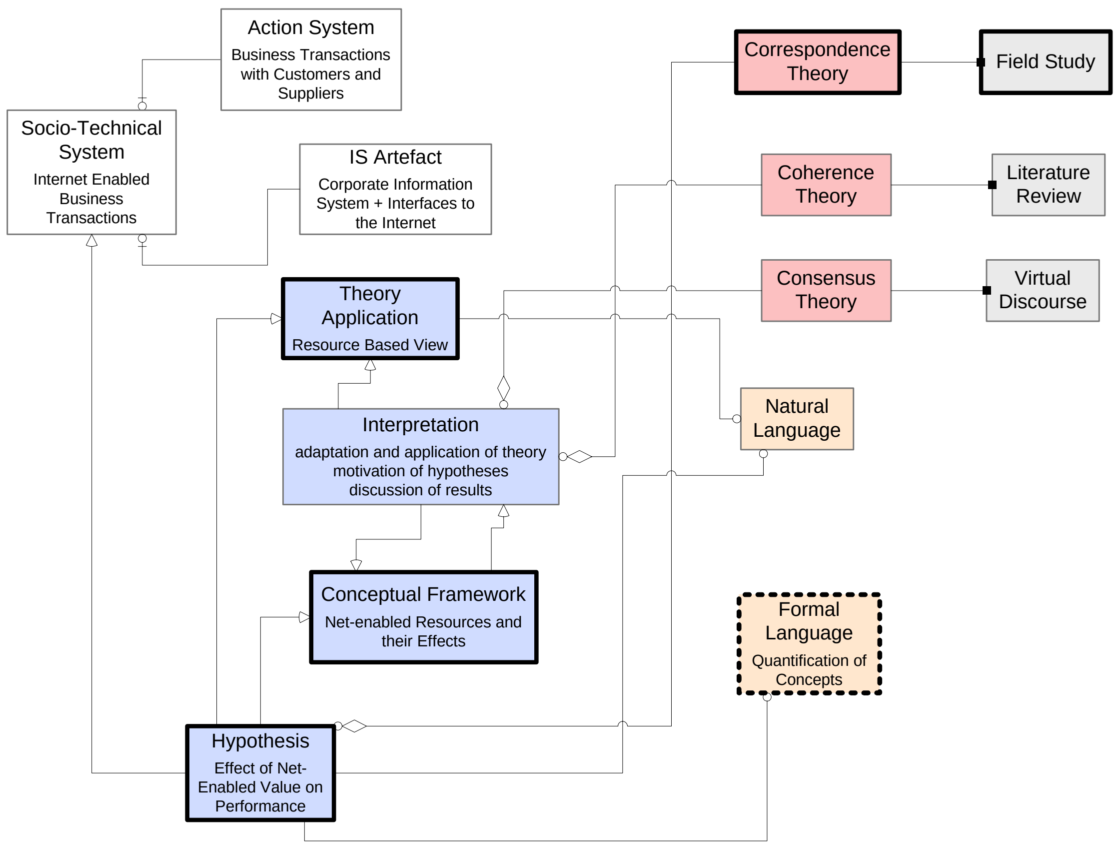

Figure 4: Model of a Particular Approach to 'Behaviourist' Research [BaKo+04]

Note that the concept "Formal Language" is represented by a dotted line to express that it does not indicate the explicit use of a formal language, but only the formalization required for applying statistical procedures.

It is arguable, whether the resource based view qualifies as a theory (see 8.3). However, this is not an issue here. The model serves only to show how the concepts as they are being used by the authors can be reconstructed using the conceptual model presented in Figure 2.

The next examples are aimed at 'design-science' research. The model in Figure 5 shows an idealized configuration for this kind of research. It is typically aimed at the development of a design artefact. Abstractions of the factual, such as theory applications, interpretations or hypotheses may be used to provide justification for the purpose of the artefact. Note, however, that this is not an 'objective' justification, which would be associated with the idea of truth. Instead, pointing at actual problems or requirements serves more as a motivation for selecting a particular purpose. Adequacy connects purpose and features of the design artefact. To evaluate, whether and how well the artefact satisfies the purpose, i.e. certain requirements, it may be an option to refer to theory applications, interpretations or hypotheses. Note, however, that testing hypotheses according to the correspondence theory of truth is out of the scope of an idealized design-science project. Instead, hypotheses and interpretations can be evaluated by applying the consensus theory or the coherence theory – which includes referring to other studies that focussed on the correspondence theory. If requirements as well as the design artefact were expressed in a formal language, it is possible to prove the adequacy of the artefact. Both, the artefact and the supporting hypotheses, theory applications and interpretations refer to socio-technical systems. Note, however, that these are different abstractions.

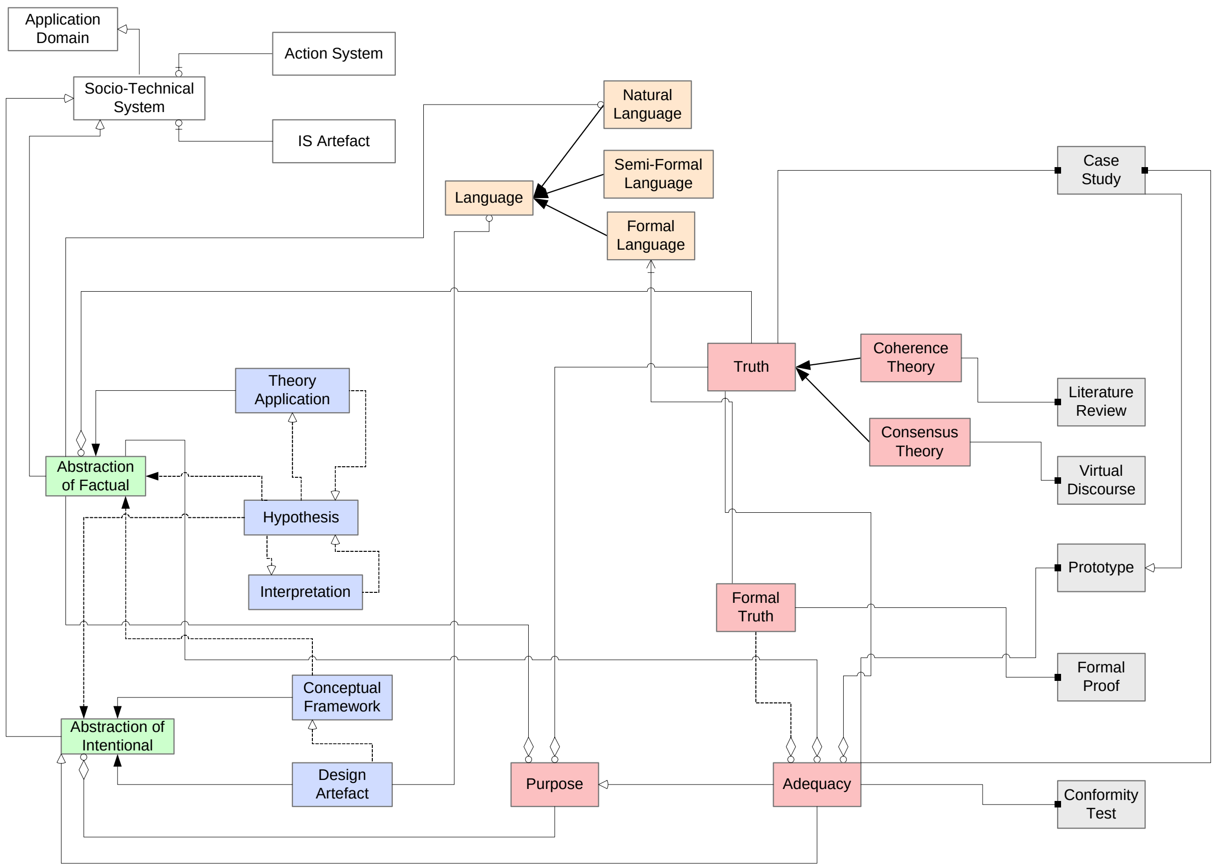

Figure 5: Conceptual Model of Idealized Design Science Approach in Information Systems Research

The artefact as an abstraction of the intentional refers to an intended possible world. This possible world may include elements of the factual world. On the other hand, hypotheses, interpretations and theory applications, which may be used to substantiate the design artefact, rather refer to abstractions of factual socio-technical systems.

Following this idealized model, the next example shows a concrete design-science project, which has been evaluated by Hevner et al. [HeMa+04] already. Van der Aalst and Kumar present a language to model workflows [AaKu03]. Its design draws upon concepts of Petri nets. It is specified as an XML DTD. Furthermore, the paper presents the architecture of a workflow management system, the schema of which can be defined using instances of the proposed DTD. The purpose, the language should serve is not clearly expressed. The authors criticize existing languages for not supporting inter-organisational workflows and for insufficient scalability. The article presents core concepts of the language. The corresponding DTD is presented in the appendix. Figure 6 shows the reconstruction of the research design presented in the paper. It sticks out that there is a clear lack of justification. This is the case for the purpose, the language should serve as well as its adequacy. The purpose is described only vaguely – by claiming the benefit of inter-organisational workflow management. There is no explicit test of the artefacts' adequacy. Only with considerable goodwill, the prototype can be regarded as an attempt to show adequacy.

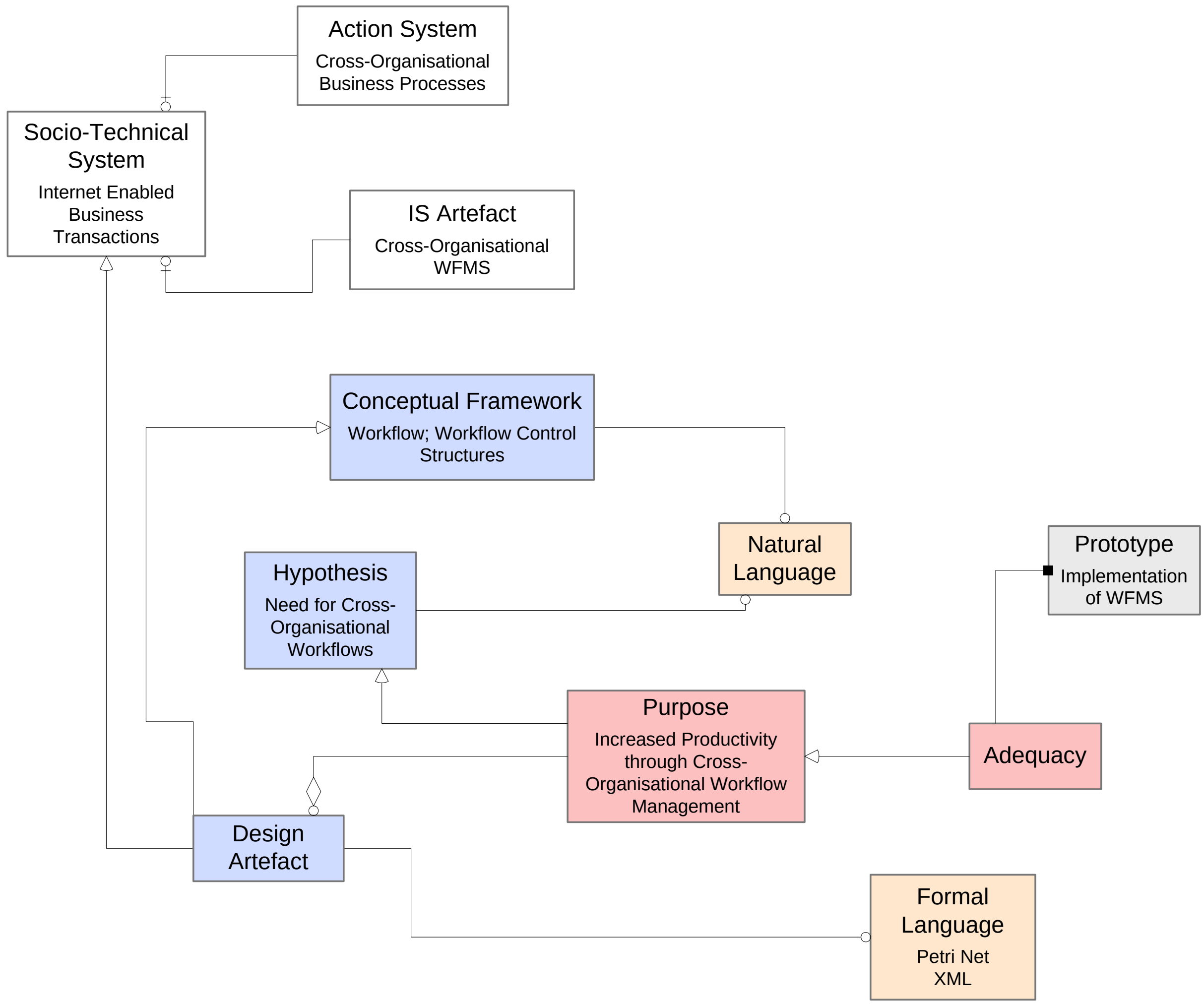

Figure 6: Model of a Particular Approach to 'Design-Science' Research [AaKu03]

The two examples illustrate the following: Research that is supposed to make use of one research method only, such as [BaKo+04], may also use elements of other research methods. This may, however, happen implicitly and without following an appropriate procedure. The explicit configuration of methods would foster more conscious and elaborate research designs. Especially the second example illustrates that the application of the framework supports the assessment of the methodological foundation of a research project (e.g.: is anything missing?).

9.3 Guidelines for Configuration Decisions

The conceptual model depicted in Figure 2 shows elements of research methods and how they could be combined. It does not include, however, criteria that would help with the selection and proper configuration of these elements. The criteria that are suggested for supporting configuration decisions are based on the essential postulates for scientific research (see 8.1 and 8.2). These features are refined with respect to the peculiarities of ISR. At first, we will look at the selection of primary knowledge contributions. During the configuration process, they may be supplemented by secondary knowledge contributions.

General Criteria

The configuration of a particular research method implies selecting the research subject and specifying the primary research objective. This includes selecting the intended knowledge contribution. The topic should fit the disciplinary focus. This demand makes sense in general: It emphasizes the need for pertinent competence. Regarding the remarkable bandwidth of topics in ISR, it is important to account for this postulate in order to prevent the impression of a "free for all" discipline. As a default, the research subject should be comprised of an action system and a corresponding information system, which is comprised of IS artefacts. The action system should be located in one or more goal-oriented organisation, typically business firms. That means that characteristics of both systems and their interaction should be part of the research subject. Also, as a default, the research goal should be directly related to the analysis, development, use and maintenance of information systems with respect to a managerial or economic perspective. In addition to that, a research focus should be avoided that clearly belongs to other disciplines. This would, for instance, exclude studies of "sexual harassment via e-mail" [Sipi97], the assessment of a firm's Web presence [VeAg02], or the design of an optimisation model for "profit maximization in multi service networks" [SaHe03].

The selection of the primary knowledge contribution is tightly interrelated with the selection of the research subject and the research objective. It is aimed at deciding whether the intended research should primarily focus on a certain abstraction of the factual, on a specific abstraction of the intentional or on critique. In general, the intended knowledge contribution should satisfy the demands for originality and abstraction. As elucidated in 8.1, there is no definite procedure to decide whether a prospective research result qualifies as original. Nevertheless, the claim for originality can be supported by a study of related work. The intended knowledge contribution should allow for a clear and comprehensible distinction towards existing contributions. This is a consequence of the critique postulate, which demands to make decisions explicit in order to allow for criticizing them. However, the level of innovative distinction is difficult to judge. In the end, the main criterion is a research result's potential for *surprise* and *inspiration*. Only, if there is a chance to surprise members of the relevant scientific community, a contribution is original. Inspiration is stressing a different aspect of surprise. It is related to new concepts or viewpoints, which are unusual, but suited to enrich the knowledge of other scientists in the respective field. It can, for instance, be inspiring to apply a certain meta theory, such as general systems theory. Judging the potential for surprise and inspiration can be done through introspection, relying on own experience and knowledge of the field – of course, this is not an objective assessment.

Abstraction is hard to judge, too. Statements about single instances are certainly not satisfactory. A knowledge contribution should always be built on concepts that allow for its application to a number of cases. However, it is not possible to develop a definition of the required level of abstraction that would allow for a clear judgement. This imperfection should not be mistaken as a reason to totally neglect the issue of abstraction. Instead of judging the level of abstraction, it should at least be described in a comprehensible way. This includes a demarcation of the intended and – may be – the potential application domains as well as the prerequisites to be fulfilled in order to apply the results. For a knowledge contribution to comply with the justification postulate, it needs to be backed by at least one adequate justification criterion and a respective justification procedure. Table 1 summarizes the general criteria. Originality and abstraction will be resumed in some cases for discussing decisions on specific knowledge contributions

	Criterion	Check	Comment
Topic	research subject	action system + information system (artefacts) within goal-oriented organisation (or group of goal-oriented organisations)	Typically, the notion of information system will focus on computer-based information systems.
	research goal	analysis, development, use and maintenance of information systems with respect to a managerial or economic perspective	This demands to check goals for their potential economic implications.
	responsibility	inappropriate invasion of other fields?	This criterion allows for an additional check, which should usually be redundant.
Originality	superiority	superior with respect to identifiable aspects?	requires to identify respective aspects and to show why they are superior to those of previous contributions
	surprise	potential for surprising peers	Judgement depends on experience & introspection.
	inspiration	potential for inspiring peers – unusual approach or perspective?	Judgement depends on experience & introspection.
Abstraction	concepts	clear focus on concepts that allow for being applied to many cases, also to future cases; should not be vague, instead allow for clear decision whether applicable in a particular case or not	requires checking whether core concepts could be generalized into higher level concepts
	domain	intended application domain can be described in a comprehensible way	the more precise the better

Table 1: General Criteria

Focus on Theory and Theory Application

Deciding whether to aim at a theory or not is mainly a question of feasibility. While it is difficult in general to develop a theory that satisfies the criteria outlined in 8.3, it is a special challenge in ISR. This is for two reasons. The first reason is indicated by the supposition that acting in the context of information systems is usually not based on patterns that are peculiar to this subject. Instead, they will be related to more general patterns of human action, which are subject of disciplines such as psychology and sociology. The second reason is related to the supposed contingency of the subject (see 5.3). It implies that there are no invariant general patterns – unless one succeeds in reducing contingency. Sometimes, a research subject may appear not to be contingent with respect to the research questions. Nevertheless it could be. Indicators of contingency are fast technological progress, intensive competition, changing qualifications and structural change in general. In case, this kind of change can be assumed, one would have to ask whether it will affect the intended theory application. Perceived contingency can be reduced, if the various possible shapes of a phenomenon can be contributed to different causes. If a theory is regarded as feasible, which implies that it satisfies the demands for originality and abstraction, it is required to check how it could be justified. The default justification criterion would be the correspondence theory of truth. In order to provide for the application of this concept, it is required to formulate adequate hypotheses, which are adapted from the theory. In addition to that, an empirical test, e.g. through a field study may be applied to support the theory. This is, however, not obligatory: If a proposal for a theory can be supported by comprehensible arguments (by making use of the coherence and/or the consensus theory of truth) and satisfies the demands for originality and abstraction, it is a major accomplishment on its own, which could be evaluated by further research.

A theory application is more likely to happen in ISR. It is the approach of choice within the behaviourist paradigm. Whether it is appropriate depends on a number of factors. First of all, there should be an interesting research question that has not been answered to a satisfactory degree. Furthermore, an appropriate theory is required, which allows to provide an answer to the research question. Often, so called theories that are referred to in ISR hardly qualify as theories. The collection of theories edited by Schneeberger and Wade [ScWa05] includes many questionable candidates. To give a few examples: The 'resource-based view of the firm' is not really surprising. It is almost tautological. The 'media richness theory' can be reduced to more general theories of communication. The 'soft systems theory' is rather a method than a theory. The 'structuration theory' is rather a conceptual framework or a meta-theory (see below). Applying an inappropriate or low-quality theory to a research question will not produce valuable results. If a refined theory is available, it has to be analysed whether it can explain the phenomenon targeted by the relevant research questions. If the subject is clearly a special case covered by the theory, its application does not come as a surprise. Hence, the originality postulate would not be satisfied. This could be compensated only, if the consecutive test promises to produce a surprising result. Again, contingency is an important issue: If there are some cases that could be explained by applying the selected theory, while at the same time other cases cannot be explained, the application of the theory is not adequate. Even if all observed cases can be explained by the theory application, the subject may be contingent: There may be possible constellations in future cases, which are not covered by the theory application any more. This could be related e.g. to changes in professional training or attitude, which can be expected over time. The more likely these changes seem to appear, the higher is the level of supposed contingency. Only, if it is possible to dissolve this contingency, it can become an option to explain a certain part of the phenomenon by applying the theory. Sometimes, it is not clear, whether the phenomenon to be explained is covered by a selected theory. In this case, applying the theory could result in expanding the theory to a domain that was not covered originally. It should, however, be analyzed carefully, whether it is possible to develop convincing reason for the supposition that the phenomenon could be explained by the selected theory. Take, for instance, the evolutionary theory, which is listed in [ScWa05] as "widely used" in ISR. Selecting it to explain the evolution of ERP systems presupposes substantial similarities between information systems and biological systems – which is at least debatable. Therefore, testing hypotheses, which are based on such a 'remote' theory, does not necessarily validate the theory.

If a theory application appears to be a substantial contribution (surprising, inspiring new knowledge), it could be regarded as a value on its own – similar to the development of a theory. Only, if there are comprehensible doubts, an additional empirical test would be an option. This, however, requires the preconditions for empirical tests to be fulfilled – including a satisfactory operationalisation of key concepts as well as the request for validity and reliability (see 7.1). In addition to epistemological considerations, pragmatic aspects need to be taken into account, too. They include the availability of adequate test instances as well as the access to relevant data concerning these instances. Limited research resources and limited time will usually compromise the scope and depth of empirical studies. These obstacles should be taken into account before deciding for performing empirical tests – otherwise one takes the risk to promote trivial results (see 7.1).

	Criterion	Check	Comment
Feasibility	independence	Is it possible to reduce the theory to other existing theories?	It may occur that a definite reduction is not possible, but a 'close' reduction. In this case, it needs to be decided whether the knowledge contribution is a theory or rather a theory application.
	contingency	Are cases possible, which are not – or will not – be covered by the theory? Indicators: (structural) change and incentives for change	This demands to reflect upon possible future developments.
Originality	surprise	Is the application of the theory not obvious (transfer as a challenge)? Is expected outcome of test surprising? Is contingent subject not comprehensively covered by theory? Is there a chance to dissolve contingency?	Judgement depends on experience & introspection.
Test	operationalisation	Is satisfactory operationalisation of key concepts possible (no substantial loss of relevant semantics)? Are sufficient validity and reliability of corresponding data to be expected?	This criterion is aimed at the question whether a test makes sense even if data are available.
	availability	Are data available? Are resources required to collect and analyse data, available?	This will often be a bottleneck that deserves critical consideration.

Table 2: Additional Criteria to Guide the Decision on Theory or Theory Application

Focus on Interpretation

An interpretation following a hermeneutic approach is an option whenever there is no theory available that would allow for a satisfactory explanation. Also, if core concepts to characterize the targeted phenomenon bulk at operationalisation, an interpretation could be an option. However, as with any other knowledge contribution, an interpretation should promise surprising and/or inspiring insights. This could be accomplished by stressing new perspectives on the studied phenomenon, e.g by applying unusual images or analogies. In addition to that, it should be possible to describe the level of abstraction by demarcating the intended application domain. Sometimes, case studies are regarded as subjects of interpretative research. This is, however, misleading. While a case study may help to foster a better understanding, it is not necessarily required for performing interpretative studies. From an epistemological point of view, it makes no difference whether an interpretation resulted from introspection or from related experience. A case study may only serve to provide preliminary evidence for the interpretation. If the studied phenomenon is characterized by contingency, it should be possible to dissolve it. Otherwise, an interpretation would be caught in the contingency trap, too. It seems that dissolving contingency within an interpretation is not as challenging as within a behaviourist approach: It is sufficient to point at possible – and comprehensible – reasons for variance.

The justification postulate constitutes the main challenge for an interpretative approach. Only, if there is the chance for justifying the core arguments by testing them in a comprehensible way, an interpretation is an option. Typically, interpretations make use of the coherence or the consensus theory of truth. In both cases, the quality of justification depends on the ability to convince a knowledgeable audience (and not just the 'believers', i.e. the followers of a certain school) by deducting the core assumptions to accepted knowledge. With respect to the subject of ISR, interpretations should avoid adopting the traditional techno-phobic attitude that accompanies hermeneutic approaches. That implies among other things to use an elaborate terminology for describing the IT artefact.

	Criterion	Check	Comment
Chance	limit of behaviourist approach	Is the research question addressed not suitable for a behaviourist approach?	This is not mandatory. Also, it does no imply that a behaviourist approach, if it was possible, would be the better option. However, if it is no option, this would create a chance for a hermeneutic approach.
Originality	surprise	new insights, surprising perspective, focus on hidden aspects	Judgement depends on experience & introspection.
	inspiration	inspiring insights, use of metaphors and analogies	Judgement depends on experience & introspection.
Justification	justification criteria	Can theories of truth (coherence, consensus, correspondence) be applied to convince others across the borderlines between different ISR communities?	The application of the coherence theory may tempt to a selective consideration of literature, while the consensus theory may tempt to focus on knowledge that is accepted within a particular community only. Therefore, a self-critical inspection of the justification procedure is recommended.

Table 3: Additional Criteria to Guide the Decision on Interpretation

Focus on Conceptual Framework

A conceptual framework is an option, whenever there is lack of structure for analysing certain phenomena or problem classes – both in research and practice. The concepts proposed by such a framework should serve as instruments to guide research or problem solving. Originality of a framework could be demonstrated by pointing at aspects, which cannot be expressed with existing terminology (superiority claim). Also, a framework can qualify as original, if it provides a conceptual foundation for solving classes of problems, no solution or only inferior solutions exist for. This implies to take into account the intended purpose of the framework. If it is not possible to show the superiority of the framework, one should at least be convinced that it is inspiring for the prospective users. Meta theories such as general systems theory or structuration theory can be regarded as a special case of a conceptual framework. They are original, if they suggest unusual perspectives that foster structuring a research domain in a promising way. Probably the only way to check this criterion is educated introspection. With respect to the level of abstraction, it should be possible to show that the concepts are not only suited to describe the facets of existing domains or certain designs of future possible worlds. Instead, they should allow for covering a wider range of possible instances. This is especially important with respect to the IT artefact. While it is characteristic for an application-oriented discipline to use terminology that is influenced by existing technology, a conceptual framework should aim at a higher level of abstraction, which would allow for covering other concrete forms of technology as well.

Justification of a conceptual framework is a major challenge. It requires showing the framework's adequacy. Judging adequacy implies identifying the purpose, the framework should serve. A general purpose, such as 'supporting the design of integrated information systems' or 'supporting the analysis of cross-organisational information systems', should then be decomposed into more concrete requirements. To justify the framework, it should be possible to show how the concepts contribute to serving these requirements.

Focus on Design Artefact

At first sight, constructing a design artefact is an option, if it promises the solution of a class of problems no solution or only inferior solutions exist for. To fulfil the originality postulate, it is necessary to decompose the purpose into more concrete requirements. Then it should be possible to point at requirements, which are addressed by the intended artefact, but not fulfilled so far by existing solutions. An existing solution may be modified in a way that allows for satisfying additional requirements. However, if this modification appears to be rather obvious, it could hardly be regarded as original: Originality implies surprise. With design artefacts a further, more subtle aspect of originality has to be accounted for: aesthetics. Aesthetics of a design is related to elegance and minimality. If a design appears to be more aesthetical than others, this would contribute to its originality. Of course, this aspect is hard to judge.

To qualify as a scientific knowledge contribution, design artefacts – and the action systems, they are embedded in – should emphasize an appropriate level of abstraction. The intentional abstraction from existing socio-technical systems makes the more sense, the less invariant these systems appear; in other words: the more contingent they are. Take, for instance, certain conceptions of workflow management systems (WMFS), which are a response to the insufficiencies of current information systems. With overcoming these insufficiencies, the corresponding features are obsolete. For ISR, abstraction is a pivotal concept for a further reason: Abstraction is a core design principle of software engineering. Making explicit use of corresponding concepts such as encapsulation (abstraction from internal data structures), generalisation (abstraction from the peculiarities of special cases) or polymorphism (abstraction from the software module that performs a function) emphasizes the promotion of abstraction.

Justifying a design artefact refers to purpose and adequacy. There should be a chance to justify the purpose by pointing at existing or foreseeable demands. This could be possible by deducting the specific purpose from more general purposes, which are usually regarded as relevant within the discipline (e.g. improving the efficiency of cross-organisational business processes). If concrete requirements cannot be deducted from the purpose, they could be justified by referring to theories or theory applications. For instance: If the purpose of a tool to be developed is to foster communication between various stakeholders of a certain class of projects, the requirement to provide a graphical notation cannot be logically deducted. Instead, it might be possible to refer to theories from psychology – or respective theory applications in ISR. Note that with respect to the appropriate level of abstraction, the purpose should not reflect contingent facts. The major focus of justification is on adequacy. This requires comparing features of the artefact against the requirements. In some cases, it may be obvious that a requirement is fulfilled. In other cases, this may be a challenge showing that. Take, for instance, the usability of a new modelling language. While it could be evaluated through an empirical study, the value of such an approach is questionable: The outcome will depend on the observed users' previous experience, their level of training etc. In other words: It is a contingent subject, which is not suited for empirical tests (see above) – unless it is possible to dissolve contingency. Finally, an empirical test is an option only, if there is a theoretical background, i.e. a theory application. If this is the case, the preconditions for testing theory applications (see above) would still need to be fulfilled.

In any case, it should be possible to provide comprehensible reasons to support the claim for adequacy. A prototype can be an option, if there is considerable doubt that it is feasible. From an epistemological point of view, a prototype is less suited to build justification, but rather an instrument to inspire further research. If a design artefact and the corresponding action system are far reaching abstractions of the intentional, it should be possible to justify the possibility of the required change, e.g. by showing that there is a clear potential for change. This could be done by pointing at drivers of change, such as technological progress or comprehensible incentives for fostering change.

	Criterion	Check	Comment
Originality	requirements	Is it possible to specify comprehensible requirements? Does the intended design artefact promise to fulfil certain requirements superior to existing solutions?	This demands for a thorough investigation of existing solutions and a differentiated registration of requirements.
	inspiration	unusual approach? inspiring transfer of design principles from other fields?	Judgement depends on experience & introspection.
	aesthetics	Is the intended solution more elegant than comparable solutions?	Hard to judge, but still relevant.
Abstraction	dependence from technology	Are the concepts used independent from existing technology? If this is not the case, it needs to be checked whether existing technology is supposed to be invariant in time.	Existing technology has a subtle impact on researchers' perception and judgement. Total independence is neither possible nor desirable. However, it should be avoided to use terms that are specific to certain types of technology, which may well be replaced by others.
Justification	purpose	Is it possible to give reasons for choosing a certain purpose? This could be accomplished by providing evidence that a corresponding demand exists or will exist.	While justifying purpose makes sense for an application-oriented discipline, it requires a critical approach: Favouring at a particular demand may imply discriminating other interests.
	specification	Are both requirements and design artefact specified precisely enough for a comprehensible test of adequacy?	Formal specifications would be best suited at first sight. However, the price (loss of semantics) paid for it, should not be too high.
	empirical test	Only an option, if requirements for empirical test are fulfilled, which include the reference to a theory or a theory application.	A so called 'proof of concept' in a single case is usually not a convincing justification. This would be acceptable only, if this single case can be regarded as representative in all relevant aspects.

Table 4: Additional Criteria to Guide the Decision on Design Artefacts

A formal proof is the most convincing justification of adequacy. However, this requires a formal specification of the requirements and the respective features of the design artefact. If this is possible, it is certainly a good option; otherwise, a formal proof contributes to the illusion of correctness only.

Focus on Critique

Compared to construction, critique is the other side of the same coin. Hence, its level of abstraction depends on the level of abstraction stressed by the targeted constructions. It may also focus on an entire class of constructions, thereby featuring a higher level of abstraction than each one of these constructions. Critique is the more original, i.e. surprising, the more established the targeted constructions are. In other words: the less these constructions have been challenged in the past. If critique is aimed at previous critique, its originality depends on the level of acceptance, the previous critique has received. With respect to justification, critique may apply the same criteria used by the targeted constructions. It may also apply other criteria. Furthermore, it may target a lack of logical consistency. In any case, the requirements that are related to justification are the same as those that apply to constructions – however, with a different intention. If a construction is aimed at a contingent subject, the corresponding critique needs to take that into account. It could point at contingent facts, thereby showing that the construction does not account for contingency. On the other hand, if critique aims at showing that a construction is wrong or inadequate, it should not be based on contingent facts either. If, for instance, a software development method is supposed to increase software quality and productivity, it may be possible to refute this claim by performing a field study. Critique of this kind would not be convincing, if certain relevant factors, such as the level of training or the attitude were contingent.

	Criterion	Check	Comment
Originality	construction	the more established the targeted construction seems to be, the more original the critique	Judgement depends on experience & introspection.
	previous critique	Are there previous attempts to challenge the construction? Is the intended critique clearly superior/more effective?	This demands for a thorough investigation of previous critiques.
Justification	inconsistency	Does the targeted construction or critique include logical inconsistencies or weaknesses? Are these inconsistencies essential for the knowledge contribution?	Identifying logical inconsistencies is usually a convincing justification of critique.

Table 5: Additional Criteria to Guide the Decision on Critique

Implications of the Transparency Postulate

The configuration of a research method does not only include the decision for the primary knowledge contribution. Often, justifying the primary knowledge contribution requires to refer to other knowledge contributions and corresponding justification criteria. This is stressed by the transparency postulate (see 8.2). It recommends making all non-evident suppositions that are needed to justify a construction explicit. This includes, e.g. hypotheses that influence the design of a field study, hypotheses that guide the specification of requirements to be met by a design artefact etc. These hypotheses can be introduced as working hypotheses without further test of their truth. Alternatively, their validation may be part of the research as well. In this case, a respective configuration of the research method would be required.

9.4 A Corresponding Process Model

The conceptual model is supplemented by a process model. The process model in Figure 7 is comprised of five phases. It should not been mistaken as a strict sequence. Instead, the phases are interrelated.

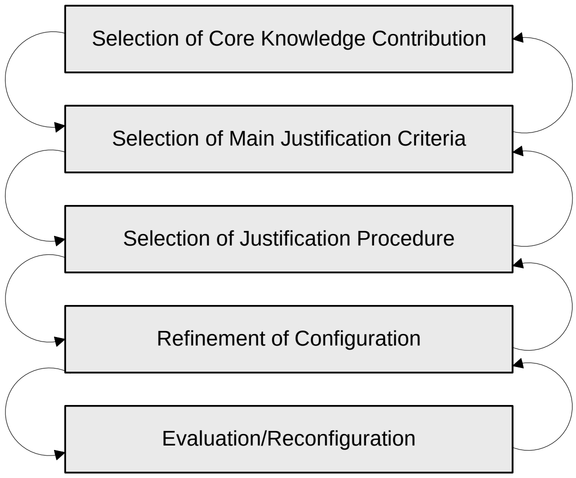

Figure 7: Process Model to Guide the Configuration of Research Methods

The first three phases of the process model are supposed to be completed before the actual research projects starts. They are aimed at the macro-configuration. The remaining phases are not mandatory. The first three steps are tightly interrelated. Selecting a core knowledge contribution depends on the availability of satisfactory justification criteria and corresponding justification procedures. On the one hand, this requires studying the selected research subject and interesting research questions, on the other hand, it recommends applying the criteria discussed in the previous section. Once a satisfactory macro-configuration has been selected, the research project could start. During the course of research, further research questions as well as demands for justification will evolve, often inspired by the demand for transparency. This may cause a refinement of the configuration. Once, the (preliminary) research results are being reviewed, it may by necessary to evaluate the research method, which may result in a reconstruction of the configuration.

9.5 Documentation

In general, there is need for documenting research in a way that supports accessing and evaluating relevant results. In section 7.4 it is outlined why documentation is of special importance for ISR.

9.5.1 Objectives

Documentation should serve the following partially overlapping objectives:

Improve accessibility: The ever growing amount of publications as well as the considerable diversity of the field make it difficult to find a comprehensive list of all publications that are related to a particular research question. Documentation should foster the access to relevant work. This includes aspects of both, content and research method.

Reduce hidden redundancy: Probably, a remarkable amount of work presented in ISR publications is redundant. However, redundancy is often not easy to discover, since it is hidden by the lack of common terminology. Also, similar research questions may be targeted in different sub communities of ISR, which makes it even more difficult to identify redundant work. Furthermore, recurring topics or fads contribute to increasing redundancy. Documentation should support reducing hidden redundancy.

Improve comparability: Due to the lack of common concepts and the large amount of subtle redundancy, research results are often hard to compare – both with respect to their content and with respect to the specific method. Documentation should contribute to a better comparability of research results and methods.

Foster reconstruction of scientific progress: Results of ISR have been presented as a vast conglomeration of research fragments – often redundant and hard to compare. Therefore it is very difficult to identify paths of scientific progress. Documentation should represent various facets of potential progress in a way that fosters the reconstruction of disciplinary progress.

9.5.2 The Contribution of the 'Non-Statement View'

The schools of philosophy of science considered in section 6.2 are mainly concentrated on criteria and procedures to justify knowledge contributions. They do not, however, care about conceptions of how to document results of scientific research. As a consequence, they provide only limited support for the comparison of knowledge proposals and the recognition (or reconstruction) of scientific progress. This is different with structuralism, also referred to as 'non-statement-view' ([Snee71], [BaMo+87], [BaMo+99]). The 'non-statement view' aims at specifying a structure, also called an "architectonic"[1], which should be suited to represent the "'essential' features of empirical knowledge …" ([BaMS87], xvii). The foundation of a theory is characterized by a conceptual framework and lawful propositions with an empirical content. The simplest case of a theory is called "theory-element". It consists of a so called "theory core" and a "domain of intended applications" ([BaMS87], p. 38). The core of a theory is a formal structure. It comprises a set of so called *potential models* (interpretations) of the underlying conceptual framework and a subset of this set, the set of so called *models* of a theory. A model of a theory satisfies the theory's lawful propositions. In order to further define the semantics of a theory, it is possible to specify constraints and to establish associations between theories, so called "intertheoretical links". A number of interrelated theory-elements can form a "theory-net", which corresponds to common notions of a theory ([BaMS87], p. 167). Given such a structure of theory-nets, it is possible to define scientific progress in a formal way: "A theory-evolution is a sequence of theory-nets in historical time subject to some constrictions. Intuitively, a theory-evolution is a changing theory-net. More intuitively still, a theory-evolution may be visualized as a "living" net growing and/or shrinking in different directions over historical time." ([BaMS87], p. 205) In addition to that, the proponents of the 'non-statement view' suggest a structure, which could serve as a knowledge base for all empirical sciences. It evolves from interrelating theory nets across disciplinary boundaries ([BaMS87], p. 386).

Structuralism offers an ambitious choice for representing research results. It suggests an elaborate documentation structure. Its proponents refer explicitly to knowledge representation in Artificial Intelligence and speak of a "'representation scheme' for scientific knowledge" ([BaMS87], xvii). However, describing or reconstructing a theory using the concepts of the 'non-statement view' implies a tremendous effort that would probably discourage many researchers. At the same time, it is not suited for ISR in general, since it focuses on empirical sciences only. Also, it seems that theories in ISR (if there are any) are not described precisely enough to be subject of a structuralistic reconstruction. Nevertheless, structuralism can serve as an orientation for documenting knowledge contributions in ISR. It would, however, require some simplification and adaptation.

---

[1] This term goes back to a suggestion made by Kant, who characterized "Architektonik" on the one hand as the "art of systems", on the other hand as the "study of the scientific within our knowledge" (translated from ([Kant76], B 860, A 832).

9.5.3 Outline of a Documentation Schema

A documentation structure should serve to describe both the content of a knowledge contribution and the research method that was applied. The conceptual model in Figure 2 could be used as a foundation for representing the research method. However, it needs to be extended in order to allow for representing the evolution of research, which requires associating research contributions. Furthermore, it needs to be reconstructed as an object model (or a data model respectively) in order to serve as a foundation for representing actual research contributions. Evolution can be documented through a qualified relationship between a research contribution and a previous research contribution, expressing e.g. refutation, challenge, confirmation, or variation. Sometimes, evolution may be relative to the research subject. A research subject, a knowledge contribution is aimed at, can be differentiated into action system, IT artefact and intended application domain. It can comprise the subjects of previous knowledge contributions. Ceteris paribus, this would indicate scientific progress, because it increases the range of possible applications. To assign a knowledge contribution to its creator, a protected unit of work could be used – referring either to a traditional publication or some other sort of protected content. The preliminary, simplified object model presented in Figure 8 serves to illustrate how research contributions could be represented within an information system. To support the representation of differentiated interconnections between knowledge fragments, the model includes classes that allow for specifying types of relationships, e.g. *FocusAssoc* or *SubjectAssoc*. Note that an artificial language can be part of a research subject or just the instrument used to express research results.

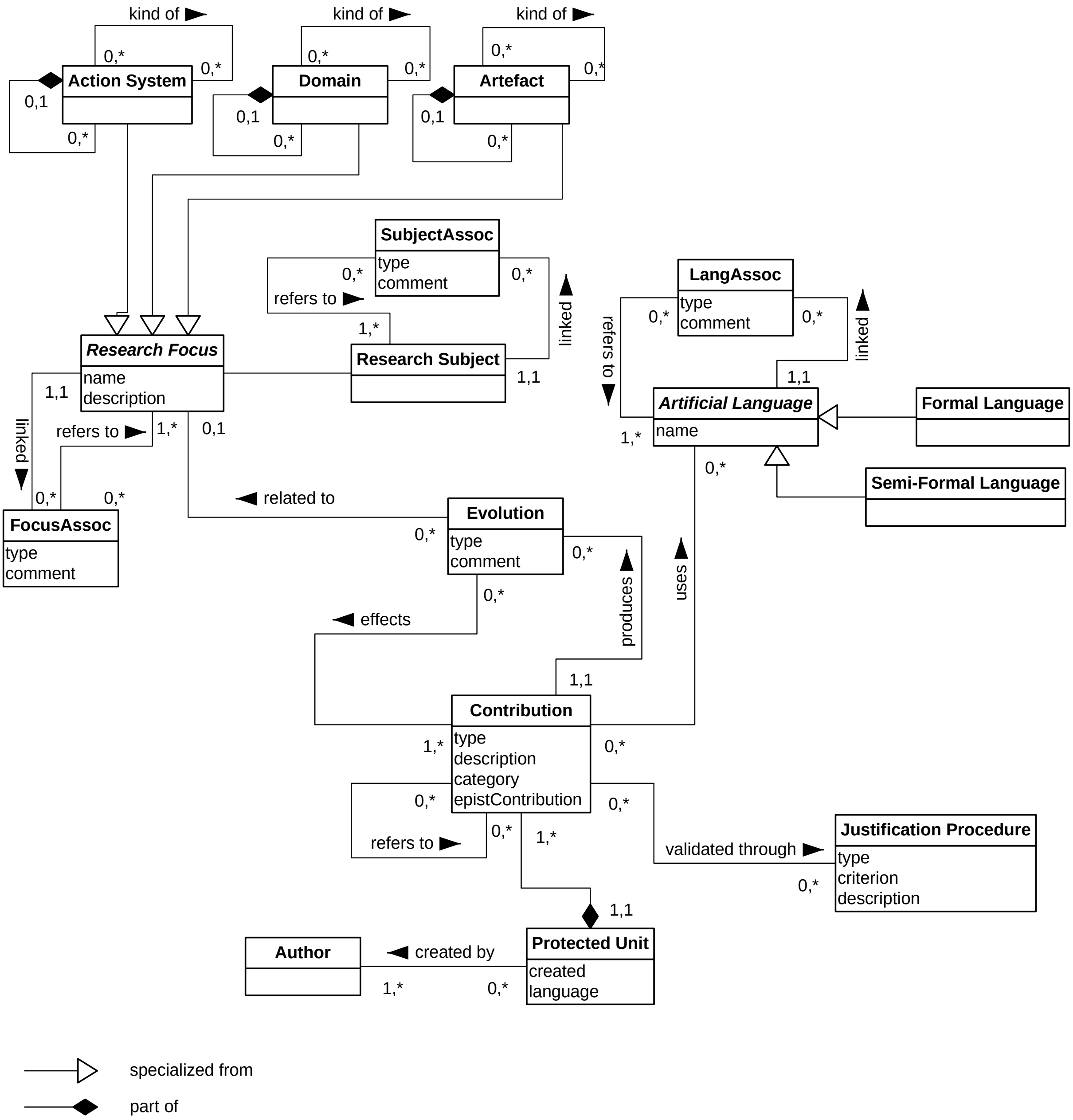

Figure 8: Object Model Reconstructed from Extended Conceptual Model in Figure 2

In addition to a common conceptual foundation, the objectives outlined above recommend specifying unified initialisations of classification concepts. These include association types – e.g. 'similar to', 'refers to' etc. – as well as taxonomies for action systems, application domains, IT artefacts, formal and semi-formal languages. Such a system could gradually evolve to a knowledge net that reflects the actual body of knowledge as well as its evolution. Figure 9 shows excerpts of simplified taxonomies to guide a unified description of research subjects.

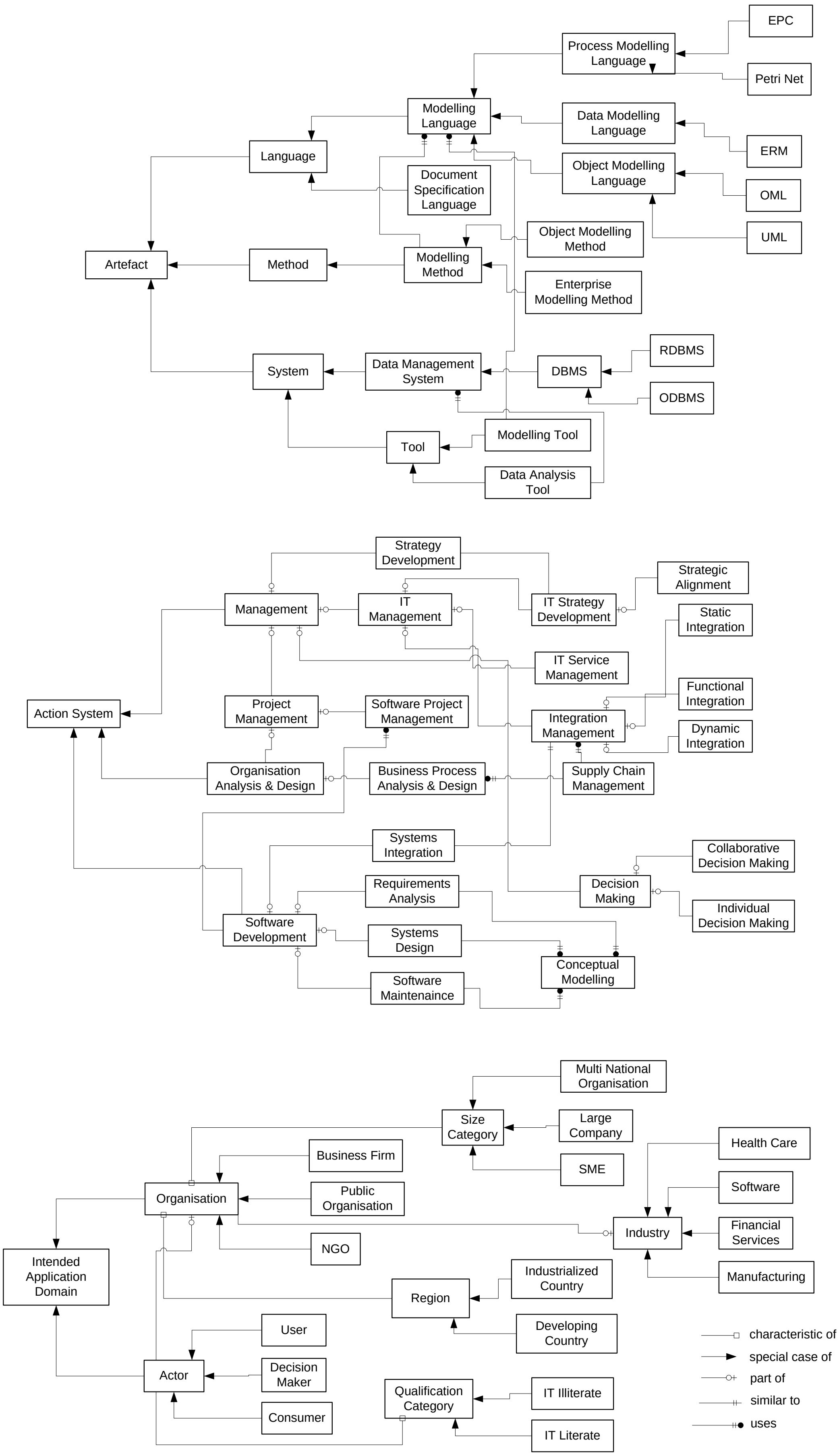

Figure 9: Excerpts of Exemplary Taxonomies

Note that artefacts can also be knowledge contributions, if their construction is a research result. The marked concepts illustrate an exemplary documentation of a research subject.

The object model in Figure 8 shows how research contributions and scientific progress could be documented. However, it leaves the problem of judging the relevant relationships - between knowledge contributions, between application domains, and between research subjects. This is the case, too, for the definition of taxonomies. This implies the question, who should be responsible for making such decisions. To foster a unified representation of the body of knowledge and to allow for reconstructing the progress of the discipline, it would be appropriate to make use of a reviewing process that is aimed at inserting a new knowledge contribution into the existing net. However, with respect to the critique postulate and the freedom postulate, such an approach is problematic: It could be used to enforce one 'official' way to evaluate progress, thereby immunizing it against critique, preventing others from effectively expressing their deviating judgement. With respect to core values of scientific culture, it seems more appropriate to implement a structure and a corresponding process, which allow for expressing and representing a variety of judgements – even though this would compromise comparability and the identification of scientific progress. However, there is need for some sort of quality control – with respect to both, the core characteristics of scientific knowledge and the classification of a particular knowledge contribution. One possible solution could be to combine 'official' reviewing processes, which are in charge of ensuring minimum standards with an open process that would allow everybody to add comments. Obviously, a computerized information system would be required to accomplish this. Such a system could allow for defining specific views on the represented knowledge. There could be views for various groups of researchers, for students at various levels, for decision makers etc. In any case, the establishment of a system to document the body of knowledge and to foster its accessibility is especially attractive for ISR: Not only that it promises to overcome the current dissatisfactory situation, it would also stress a core competence of the discipline, the design of information systems.

10 Conclusions

The application of a research method is commonly regarded as an essential feature of scientific work: "The method counts as key characteristic of scientific procedures and hence, pars pro toto, as core feature of science itself." (translated from [Lore84], p. 876). A method is supposed to guide research, to emphasize core requirements of scientific work, and to support the evaluation and comparability of results. However, only if a method is suited to support all intellectually inspiring and promising perspectives on developing knowledge, it can serve this purpose. Our study has shown that none of the existing methods used in ISR can satisfy this request. At the same time, it does not seem as an attractive option to establish different communities within ISR, each one using one particular method. This would contribute to isolation, impeding exchange and competition. The framework presented in this report is intended to present an alternative. It is aimed at three main objectives:

Common Conception of Science: Integrating various streams of ISR recommends a common foundation. For this foundation to qualify as scientific, it should comply with a generic conception of science that fits all disciplines and allows for being adapted to the peculiarities of ISR. As a consequence, such a conception would contribute to the scientific legitimacy of ISR – without merely copying established disciplines. To accomplish this goal, essential characteristics of scientific research have been reconstructed from various contributions in philosophy of science.

Configuration of Research Methods: The conceptual model presented in Figure 2 illustrates the spectrum of possible configurations. Additional criteria to guide configuration decisions provide further support.

Improved Documentation of Research Results: The ever growing amount of publications and their immense diversity make it more and more difficult to access and compare research results. As a consequence, it is a tremendous challenge to reconstruct scientific progress. The preliminary structure to guide documentation presented in this report makes use of concepts applied for the configuration of research methods. It illustrates how documentation could contribute to the evolution of knowledge nets, which support comparability and the reconstruction of evolution.

The framework presented in the report is a proposal, a starting point. In allusion to Wittgenstein's comment on the use of language[1], I would like to add: "We want to establish an order in our knowledge of the use of *methods* … one out of many possible orders; not the order." It aims at combining both, the need for a common conception of our work and the desire for individual approaches to scientific discoveries – thereby outlining a middle course between the tyranny of one dominant method and methodological anarchy[2]. At the same time, it recommends looking at research subjects and validation criteria from different viewpoints.

There is no doubt that a framework, which supports the configuration of individual research designs allows for more intellectual freedom than the prescription of a particular model. Nevertheless, the level of detail suggested by the framework and the documentation structure may be criticized by some as a restriction, since it indicates a remarkable additional effort that could be regarded as a distraction from the actual research. However, this is not my intention. Instead, the framework is meant to serve as a contribution to conscious and reflexive research by pointing at concepts that

[1] "We want to establish an order in our knowledge of the use of language: an order with a particular end in view; one out of many possible orders; not *the* order." ([Witt71], 132)

[2] Feyerabend's respective recommendation [Feye81] is mainly directed against the ideological prescription of a particular method, not against a common foundation for scientific discourses.

are essential for our work. Also, the freedom postulate suggests taking the framework not as a burden, while the critique postulate demands for challenging and improving it. In any case, the framework is not to be mistaken as a mandatory prerequisite for research: Science needs inspiring and challenging contributions. There is no doubt that some of those will not fit any framework.

References

[AaKu03] Aalst, W., and Kumar, A.: XML-Based Schema Definition for Support of Interorganizational Workflow. In: Information Systems Research Vol. 14, No. 1, 2003, pp. 23-46

[AdHo69] Adorno, T.W.; Horkheimer, M.: Dialektik der Aufklärung. Philosophische Fragmente. Fischer: Frankfurt/M. 1969

[Albe72] Albert, H.: Sozialwissenschaft und politische Praxis. In: Albert. H. (Ed.): Konstruktion und Kritik. Hoffmann und Campe: Hamburg 1972, pp. 94-123

[Albe76] Albert, H.: Konstruktion und Kritik. Aufsätze zur Philosophie des Kritischen Rationalismus. 2nd Ed., Hamburg 1976

[Albe78] Albert, H.: Traktat über rationale Praxis. Mohr Siebeck: Tübingen 1978, pp. 12

[Albe91] Albert, H.: Traktat über kritische Vernunft. 5th rev. Ed., UTB: Stuttgart 1991

[ASB99] Avgerou, C.; Siemer, J.; Bjørn-Andersen, N.: The academic field of information systems in Europe. In: European Journal of Information Systems, Vol. 8, No. 2, 1999, pp. 136-153

[BaLa89] Banville, C., Landry, M.: Can the field of MIS be disciplined? In: Communications of the ACM, Vol. 32 No.1, pp.48-60

[BaKo+04] Barua, A.; Konana, P.; Whinston, A.B.; Yin, F.: An Empirical Investigation of Net-Enabled Business Value. In: MIS Quarterly Vol. 28, No. 4, 2004, pp. 585-620

[BaMo+87] Balzer, W.; Moulines, C.U.; Sneed, J.D.: An Architectonic for Science. Kluwer: Dordrecht 1987

[BaMo+99] Balzer, W.; Moulines, C.U.; Sneed, J.D. (Eds.): Structuralist Knowledge Representation: Paradigmatic Examples. Rodopi: Amsterdam 1999

[BaWo98] Baskerville, R.; Wood-Harper, A. T.: Diversity in Information Systems Action Research Methods. In: European Journal of Information Systems, 2/1998, pp. 90-107

[BeLu67] Berger, P.L; Luckmann, T.: The Social Construction of Reality. A Treatise on the Sociology of Knowledge. Penguin: London 1967

[BeOT05] Bennis, W.G.; O'Toole, J.: How Business Schools Lost Their Way, Harvard Business Review, May 2005, pp. 96-104

[BeS+06] Benamati, J.; Serva, M.A;. Galletta, D.; Harris, A.; Niederman, F.: The Slippery Slope Of MIS Academia: A Discussion Of The Quest For Relevance In Our Discipline, Communications of AIS, Vol. 18, 2006, Article 32

[BeWe96] Benbasat, I.; Weber, R.: Research Commentary: Rethinking Diversity in Information Systems Research. In: Information Systems Research. Vol. 7, No. 4, 1996, pp. 389-399

[BeZm99] Benbasat, I.; Zmud, R. W.: Empirical Research in Information Systems: The Practise of Relevance. In: MIS Quarterly, Vol. 23, No. 1, 1999, pp. 3-16

[BeZm93] Benbasat, I.; Zmud, R.W.: The Identity Crisis Within the IS Discipline: Defining and Communicating the Disciplines Core Properties. In: MIS Quarterly, Vol.27, No. 2, 2003, pp. 183-194

[Carn98] Carnap, R.: Der logische Aufbau der Welt. Meiner: Hamburg 1998

[Carr96] Carrier, M.: Theoriesprache. In: Mittelstraß, J. (Ed.): Enzyklopädie Philosophie und Wissenschaftstheorie. Vol. 4, Metzler: Stuttgart, Weimar 1996, pp. 283-289

[Chatt01] Chatterjee, S.: Information Systems Research and Relevance. In: Communications of the AIS. Vol. 6, Art. 8, 2001, pp. 1-7

[Chec81] Checkland, P.: Systems Thinking, Systems Practice. Wiley: Chichester 1981

[Coug94] Couger, J.D.: Measurement of the Climate for Creativity in I.S. Organizations. In: Nunamaker, J.F.; Sprague, R.H. (Eds.): Proceedings of the 27th Annual Hawaii International Conference on System Sciences. Vol. IV, Los Alamitos, Ca. 1994, pp. 351-357

[Cress01] Cresswell, A.M.: Thoughts on Relevance of IS Research. In: Communications of the AIS. Vol. 6, Art. 9, 2001, pp. 1-7

[Dear72] Dearden, J.: MIS is a Mirage. In: Harvard Business Review. Vol. 50, No. 1, 1972, pp. 90-99

[Diet06] Dietz, J.L.G.: Enterprise Ontology – Theory and Methodology. Springer 2006

[Diet99] Dietz, J.L.G.: Understanding and modelling business processes with DEMO. In: Proceedings of the Annual International Conference on Conceptual Modelling (ER'99). Paris 1999, pp. 188-202

[FaMa99] Falconer, D.J.; Mackay, D.R.: Ontological Problems of Pluralist Research Methodologies. In: Proceedings of the 5th Americas Conference on Information Systems. Milwaukee/WI, 1999, pp. 624-626

[Feye78] Feyerabend, P.: Das Märchen Wissenschaft. Plädoyer für einen Supermarkt der Ideen. In: Kursbuch 53, 1978, pp. 47-70

[Feye81] Feyerabend, P.: Wider den Methodenzwang: Skizze einer anarchistischen Erkenntnistheorie. Suhrkamp: Frankfurt/M. 1981

[Foer03] Foerster, H. von: Understanding Understanding. New York: Springer 2003

[FrKl+98] Frank, U.; Klein, S.; Krcmar, H.; Teubner, A.: Aktionsforschung in der Wirtschaftsinformatik: Einsatzpotentiale und Einsatzprobleme. In: Schütte, R.; Siedentopf, J.; Zelewski, S. (Hg.): Wirtschaftsinformatik und Wissenschaftstheorie. Grundpositionen und Theoriekerne. Arbeitsberichte des Instituts für Produktion und Industrielles Informationsmanagement. Nr. 4. Universität Essen 1998, pp. 71-90

[Gabl94] Gable, G.: Integrating case study and survey research methods: an example in information systems. In: European Journal of Information Systems, Vol. 3, No. 2, 1994, pp. 112–126

[Gada72] Gadamer, H.G.: Wahrheit und Methode. Grundzüge einer philosophischen Hermeneutik, 3rd Ed., Mohr Siebeck: Tübingen 1972

[Gidd84] Giddens, A.: The Constitution of Society. University of California Press 1984

[GlSt67] Glaser, B.G. u. A.L. Strauss: The Discovery of Grounded Theory. Chicago, Illinois 196

[Glas95] Glasersfeld, E. von: Radical Constructivism. A Way of Knowing and Learning. London: Falmer Press 1995

[Gr06] Gregor, S.: The Nature of Theory in Information Systems. In: MIS Quarterly Vol. 30 No. 3, 2006, pp. 611-642

[Habe81] Habermas, J.: Theorie des kommunikativen Handelns: Vol 1: Handlungsrationalität und gesellschaftliche Rationalisierung. Suhrkamp: Frankfurt/M. 1981

[Hein05] Heinrich, L.J.: Forschungsmethodik einer Integrationsdisziplin: Ein Beitrag zur Geschichte der Wirtschaftsinformatik. In: NTM International Journal of History & Ethics of Natural Sciences, Technology & Medicine. Vol. 13, No. 2, 2005, pp. 104-117

[HeMa+04] Hevner, A. R.; March, S. T.; Park, J.; Ram, S.: Design Science in Information Systems Research. In: MIS Quarterly, Vol. 28, No. 1, 2004, pp. 75-105

[Hilb70] Hilbert, D.: Gesammelte Abhandlungen. Bd. 3, Springer: Berlin, Heidelberg et al. 1970

[HiKl03] Hirschheim, Rudy; Klein, Heinz K.: Crisis in the IS Field? A Critical Reflection on the State of the Discipline, Journal of the Association for Information Systems, Volume 4, No. 1, 2003, pp. 237 – 293

[Kant76] Kant, I.: Kritik der reinen Vernunft. Vol. 2, 2nd Ed., Suhrkamp: Frankfurt/M. 1976

[KiLy04] King, J. L., Lyytinen, K.: Reach and Grasp, MIS Quarterly, Vol. 28, No. 4, December 2004, pp. 539-551

[KlMy99] Klein, H.K.; Myers, M.D.: A Set of Principles for Conducting and Evaluating Interpretive Field Studies in Information Systems. In: MIS Quarterly, Vol. 23, No. 1, 1999, pp. 67–94

[KGH+02] Kock, N.; Gray, P.; Hoving, R.; Klein, H.; Myers, M.; Rockart, J.: IS Research Relevance Revisited: Subtle Accomplishment, Unfulfilled Promise, or Serial Hypocrisy? In: Communications of the Association for Information Systems. Vol. 8, 2002, pp. 330-346

[Lang05] Lange, C.: Development and Status of the Information Systems / Wirtschaftsinformatik Discipline - An Interpretive Evaluation of Interviews with Renowned Researchers: Part II - Results Information Systems Discipline. ICB - Research Report, University Duisburg-Essen, No. 3, 2005

[Lang06] Lange, C.: Entwicklung und Stand der Disziplinen Wirtschaftsinformatik und Information Systems - Interpretative Auswertung von Interviews: Teil III Ergebnisse zur Wirtschaftsinformatik. ICB - Research Report, Universität Duisburg-Essen, No. 4, 2006

[Lee99] Lee, A.S.: Rigor and Relevance in MIS Research: Beyond the Approach of Positivism Alone. In: MIS Quarterly. Vol. 23, No. 1 (1999), pp. 29

[Lee91] Lee, A.S.: Architecture as a Reference Discipline for MIS. In: Nissen, H.-E.; Klein, H.K.; Hirschheim, R. (Eds.): Information Systems Research: Contemporary Approaches and Emergent Traditions. North-Holland: Amsterdam et al. 1991, pp. 573-592

[LoHi07] Loebbecke, C.; Hirschheim, R.: Information Systems in der nordamerikanischen Lehre im Zeitalter von zunehmendem IS-Offshoring. In: Wirtschaftsinformatik, Vol. 49, Special Issue "Der Softwarestandort im Zeichen von Industrialisierung und Globalisierung", 2007 pp. 90-97

[Lore96a] Lorenz, K.: Wahrheitstheorien. In: Mittelstraß, J. (Ed.): Enzyklopädie Philosophie und Wissenschaftstheorie. Vol. 4, Metzler: Stuttgart, Weimar 1996, pp. 595-600

[Lore96b] Lorenz, K.: Sprache. In: Mittelstraß, J. (Ed.): Enzyklopädie Philosophie und Wissenschaftstheorie. Vol. 4, Metzler: Stuttgart, Weimar 1996, pp. 49-53

[Lore84] Lorenz, K.: Methode. In: Mittelstraß, J. (Hg.): Enzyklopädie Philosophie und Wissenschaftstheorie. Vol. 2, BI: Mannheim, Wien, Zürich 1984, pp. 876-879

[Lore74] Lorenzen, P.: Konstruktive Wissenschaftstheorie. Suhrkamp: Frankfurt/M. 1974

[LoSc73] Lorenzen, P.; Schwemmer, O.: Konstruktive Logik, Ethik und Wissenschaftstheorie. Bibliographisches Institut: Mannheim et al. 1973

[LoKa84] Lorenzen, P.; Kamlah, W.: Logical Propaedeutic. Pre-School of Reasonable Discourse. University Press of America: Lanham 1984

[Luhm84] Luhmann, N.: Soziale Systeme. Grundriß einer allgemeinen Theorie. Suhrkamp: Frankfurt/M. 1984

[LyKi04] Lyytinen, Kalle; King, John L.: Nothing At The Center?: Academic Legitimacy in the Information Systems Field, Journal of the Association for Information Systems, Volume 5, No. 6 (2004), pp. 220 - 246

[MaSm95] March, S. T.; Smith, G. F.: Design and natural science research on information technology, Decision Support Systems, Vol. 15, 1995, pp. 251-266

[MaVa80] Maturana, H.; Varela, F.: Autopoiesis and Cognition: The Realization of the Living. Boston Studies in the Philosophy of Sciences. Vol. 42. Boston, Dordrecht: D. Reidel 1980

[Matu98] Maturana, H.: Metadesign. Report, Instituto de Terapia Cognitiva 1998. Published online: http://www.inteco.cl/articulos/metadesign.htm (accessed on 12-18-06)

[Ming01] Mingers, J.: Combining IS Research Methods: Towards a Pluralist Methodology. In: Information Systems Research. Vol. 12, No. 3, 2001, pp. 240-259

[Mitt82] Mittelstraß, J.: Wissenschaft als Lebensform: Reden über philosophische Orientierungen in Wissenschaft und Universität. Frankfurt/M. 1982

[Morg86] Morgan, G.: Images of Organization. Thousand Oaks, London, New Delhi: Sage 1986

[Nieh05] Niehaves, B.: Epistemological Perspectives on Multi-Method Information Systems Research. In: Proceedings of the 13th European Conference on Information Systems (ECIS 2005) - Information Systems in a Rapidly Changing Economy. Regensburg 2005

[Orla01] Orlikowski, W. J.; Iacono, C. S.: Research Commentary: Desperately Seeking the "IT" in IT Research – A Call to Theorizing the IT Artifact, Information Systems Research, Vol. 12, No. 2, 2001, pp. 121-134

[Ortn05] Ortner, E.: Sprachbasierte Informatik. Wie man mit Wörtern die Cyber-Welt bewegt. Leipzig: Eagle 2005

[Peir31] Peirce, C.S.: The Collected Papers, Vol. V: Pragmatism and Pramaticism. 1931

[Popp63] Popper, K.R.: Conjectures and Refutations. Routledge and Keagan Paul: London 1963

[Popp68] Popper, K. R.: The logic of scientific discovery. London: Hutchinson 1968

[Popp71] Popper, K.R.: The Open Society and Its Enemies. Vol. 1, 5th rev. Ed., Princeton University Press: Princeton, NJ 1971

[Popp82] Popper, K. R.: Logik der Forschung. 7th Ed., Mohr: Tübingen 1982

[Popp84] Popper, K.R.: Auf der Suche nach einer besseren Welt. Vorträge und Aufsätze aus dreißig Jahren. Piper: München 1984

[RaLa+02] Rai, A.; Lang, S.S.; Welker, R.B.: Assessing the Validity of IS Success Models: An Empirical Test and Theoretical Analysis. In: Information Systems Research, Vol. 13, No. 1, March 2002, pp. 50–69

[Robe96] Robey, D.: Research Commentary: Diversity in Information Systems Research: Threat, Promise, and Responsibility, Information Systems Research, Vol. 7, No. 4, December 1996, pp. 400-408

[SaHe03] Sain, S.; Herpers, S.: Profit Maximisation in Multi Service Networks – An Optimisation Model. In: Proceedings of the 11th European Conference on Information Systems, ECIS 2003, Naples 2003 (electronic edition)

[Simo96] Simon, H.: The Sciences of the Artificial. 3rd. Ed., MIT Press: Boston 1996

[Sipi97] Sipior, J.C.; Ward, B.T.: Sexual Harassment via Employee E-mail: An International Issue or just a United States Quirk? In: Galliers, R. et al. (Eds.): Proceedings of the 5th European Conference on Information Systems., Cork 1997, Vol. I, pp. 313-320

[ScWa05] Schneeberger, S.; Wade, M. (Eds.): Theories used in IS Research. http://www.istheory.yorku.ca/ 2005, accessed on 12/17/06

[Schm70] Schmalenbach, E.: Die Privatwirtschaftslehre als Kunstlehre. In: In: Zeitschrift für betriebswirtschaftliche Forschung. 1970, pp. 490-498 (first published in: Zeitschrift für handelswissenschaftliche Forschung, 1911/12, pp. 304-316

[Schw84] Schwemmer, O.: Idealismus. In: Mittelstraß, J. (Hg.): Enzyklopädie Philosophie und Wissenschaftstheorie. 2. Bd., BI: Mannheim, Wien, Zürich 1984, pp. 167-170

[Schw95] Schwemmer, O.: Solipsismus. In: Mittelstraß, J. (Hg.): Enzyklopädie Philosophie und Wissenschaftstheorie. 3. Bd., Stuttgart: Metzler & Pöschel 1995, pp. 839-840

[Sneee71] Sneed, J.D.: The Logical Structure of Mathematical Physics. Kluwer: Dordrecht 1971

[Stam97] Stamper, R.K.: Organizational Semiotics. In Mingers J.; Stowell F. (Eds.): Information Systems: An Emerging Discipline. Mc Graw Hill: London 1997

[Steg78] Stegmüller, W.: Hauptströmungen der Gegenwartsphilosophie. Vol. I, 6th Ed., Kröner: Stuttgart 1978

[Steg79] Stegmüller, W.: Hauptströmungen der Gegenwartsphilosophie. Vol. II, 6th Ed., Kröner: Stuttgart 1979

[StGe+04] Straub, D.; Gefen, D.; Boudreau, M.-C.: The ISWorld Quantitative, Positivist Research Methods Website. Ed. by D. Galletta, http://www.dstraub.cis.gsu.edu:88/quant/ 2004, accessed on 11/19/06

[Thie84] Thiel, C.: Konstruktivismus. In: Mittelstraß, J. (Hg.): Enzyklopädie Philosophie und Wissenschaftstheorie. 2. Bd., BI: Mannheim, Wien, Zürich 1984, pp. 449-453

[Trau97] Trauth, E. M.: Achieving the Research Goal with Qualitative Methods: Lessons Learned along the Way. In: Lee, A.S.; Liebenau, J.; DeGross, J.I. (Hg.): Information Systems and Qualitative Research. Chapman and Hall: London, 1997, S. 225-245

[VeAg02] Venkatesh, V.; Agarwal, R.: Assessing a Firm. In: Information Systems Research, Vol. 13, No. 2, 2002, pp. 168 - 186

[Wals95] Walsham, G.: The Emergence of Interpretivism in IS Research. In: Information Sys-tems Research, Vol. 6, No. 4, 1995, S. 376-394

[Webe97] Weber, R.: Ontological Foundations of Information Systems. Melbourne: Coopers&Lybrand 1997

[Webe04] Weber, R.: Editor's Comment: The Rhetoric of Positivism Versus Interpretivism: A Personal View. In: MISQ, Vol. 28 No. 1, 2004, pp. iii-xiii

[Weic79] Weick, K.E.: The Social Psychology of Organizing. 2nd Ed., McGraw-Hill: New York 1979

[Welt83] Weltz, F.; Lullies, V.: Innovation im Büro. Das Beispiel Textverarbeitung Campus: Frankfurt/M., New York 1983

[WeOr80] Wedekind, H.; Ortner, E.: Systematisches Konstruieren von Datenbankanwendungen – Zur Methodologie der Angewandten Informatik. Hanser: München, Wien 1980

[Witt81] Wittgenstein, L.: Tractatus logico-philosophicus. Routledge & Kegan Paul: London 1981

[Witt58] Wittgenstein, L.: Philosophical Investigations. Basil Blackwell: Oxford 1958

[Wolt95] Wolters, G.: Reduktionismus. Mittelstraß, J. (Hg.): Enzyklopädie Philosophie und Wissenschaftstheorie. 3. Bd., Stuttgart: Metzler & Pöschel 1995, pp. 521-522

[Zele07] Zelewski, S.: Kann Wissenschaftstheorie behilflich für die Publikationspraxis sein? Eine kritische Auseinandersetzung mit den "Guidelines" von Hevner et al. In: Lehner, F., Zelewski, S. (Eds.): Wissenschaftstheoretische Fundierung und wissenschaftliche Orientierung der Wirtschaftsinformatik. GITO: Berlin 2007, pp. 74-123

Previously published ICB - Research Reports

2006

No 7 (December 2006)
Frank, Ulrich: "Towards a Pluralistic Conception of Research Methods in Information Systems Research ", ICB Research Reports No. 7, Institute for Computer Science and Business Information Systems, University Duisburg-Essen, 2006.

No 6 (April 2006)
Frank, Ulrich: "Evaluation von Forschung und Lehre an Universitäten – Ein Diskussionsbeitrag", ICB Research Reports No. 6, Institute for Computer Science and Business Information Systems, University Duisburg-Essen, 2006.

No 5 (April 2006)
Jung, Jürgen: "Supply Chains in the Context of Resource Modelling", ICB Research Reports No. 5, Institute for Computer Science and Business Information Systems, University Duisburg-Essen, 2006.

No 4 (February 2006)
Lange, Carola: "Development and status of the Information Systems / Wirtschaftsinformatik discipline: An interpretive evaluation of interviews with renowned researchers, Part III – Results Wirtschaftsinformatik Discipline", ICB Research Reports No. 4, Institute for Computer Science and Business Information Systems, University Duisburg-Essen, 2006.

2005

No 3 (December 2005)
Lange, Carola: "Development and status of the Information Systems / Wirtschaftsinformatik discipline: An interpretive evaluation of interviews with renowned researchers, Part II – Results Information Systems Discipline", ICB Research Reports No. 3, Institute for Computer Science and Business Information Systems, University Duisburg-Essen, 2005.

No 2 (December 2005)
Lange, Carola: "Development and status of the Information Systems / Wirtschaftsinformatik discipline: An interpretive evaluation of interviews with renowned researchers, Part I – Research Objectives and Method", ICB Research Reports No. 2, Institute for Computer Science and Business Information Systems, University Duisburg- Essen, 2005.

No 1 (August 2005)
Lange, Carola: „Ein Bezugsrahmen zur Beschreibung von Forschungsgegenständen und -methoden in Wirtschaftsinformatik und Information Systems", ICB Research Reports No. 1,

Institute for Computer Science and Business Information Systems, Universität Duisburg-Essen, 2005.

The Institute for Computer Science and Business Information Systems (ICB), located at the Essen Campus, is dedicated to research and teaching in Applied Computer Science, Information Systems as well as Information Management. The ICB research groups cover a wide range of expertise:

Research Group	Core Research Topics
Prof. Dr. H. H. Adelsberger Information Systems for Production and Operations Management	E-Learning, Knowledge Management, Skill-Management, Simulation, Artificial Intelligence
Prof. Dr. P. Chamoni MIS and Management Science / Operations Research	Information Systems and Operations Research, Business Intelligence, Data Warehousing
Prof. Dr. F.-D. Dorloff Procurement, Logistics and Information Management	E-Business, E-Procurement, E-Government
Prof. Dr. K. Echtle Dependability of Computing Systems	Dependability of Computing Systems
Prof. Dr. S. Eicker Information Systems and Software Engineering	Process Models, Software-Architectures
Prof. Dr. U. Frank Information Systems and Enterprise Modelling	Enterprise Modelling, Enterprise Application Integration, IT Management, Knowledge Management
Prof. Dr. M. Goedicke Specification of Software Systems	Distributed Systems, Software Components, CSCW
Prof. Dr. T. Kollmann E-Business and E-Entrepreneurship	E-Business and Information Management,E-Entrepreneurship/ E-Venture, Virtual Marketplaces and Mobile Commerce, Online-Marketing
Prof. Dr. B. Müller-Clostermann Systems Modelling	Performance Evaluation, Modelling and Simulation, SAP Capacity Planning for R/3 and mySAP.com, Tools for Queueing Network Analysis and Capacity Planning, Communication Protocols and Distributed Systems, Mobile Systems
Prof. Dr. K. Pohl Software Systems Engineering	Requirements Engineering, Software Quality Assurance, Software-Architectures, Evaluation of COTS/Open Source-Components
Prof. Dr.-Ing. E. Rathgeb Computer Networking Technology	Computer Networking Technology
Prof. Dr. R. Unland Data Management Systems and Knowledge Representation	Data Management, Artificial Intelligence, Software Engineering, Internet Based Teaching
Prof. Dr. S. Zelewski Institute of Production and Industrial Information Mangement	Industrial Business Processes, Innovation Management, Information Management, Economic Analyses

For more information visit us on the Web: **http://www.icb.uni-due.de**

ISSN 1860-2770