Meinung/Dialog

Empirical Research Strategies in Conceptual Modeling – Silver Bullet or Academic Toys?

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In Information Systems research conceptual modeling is an important field. Wand and Weber for example distinguish four elements: conceptual modeling grammars, conceptual modeling methods, conceptual modeling scripts, and conceptual modeling contexts [WaWe02]. With these elements they discuss the research opportunities to gain and evaluate results like artifacts. A common way to ensure a statistical, sampling-based generalizability is empirical testing, particularly used in the Anglo American region [cp. in detail LeBa03]. However, methodical problems can occur: To evaluate the quality of a new modeling method, the test person has to be familiar with the method before he/she can use it appropriately, which may affect the results. Thus the evaluation results are difficult to analyze and interpret. Extensive empirical tests for modeling methods – being very labor intensive and time consuming – are also rather counterproductive to the practical science process. Furthermore, economic or social objectives of conceptual modeling projects may affect the results too.

So what are the guidelines in information systems research [HMPR04] and especially for empirical research strategies in conceptual modeling? Are empirical evaluations of different conceptual modeling elements, as described by Wand and Weber acceptable or not? What type of empirical evaluation should be used in each case? Which challenges and problems have to be faced, and more important which quality factors of a modeling artifact cannot be measured appropriately by empirical research strategies?

The following seven contributions (in alphabetical order of authors' names) will adress the state of the art as well as specify some future research opportunities regarding empirical testing of conceptual modeling artifacts. The distinguished views demand further discussion of information system research yet. If you would like to present your point of view on this matter, please submit your article (max. 2 pages) to the chief publisher: Prof. Dr. Wolfgang König, Universität Frankfurt am Main,
email: koenig@wiwi.uni-frankfurt.de.

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Adding Value

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Empirical science has two major objectives: to describe particular phenomena in the world and to develop theories that can be used to explain and predict phenomena. The constituting characteristic of an empirical statement is its capability of being tested by a confrontation with focused observations or with results of suitable experiments. For instance, a particular observation can confirm or refute a theory. This characteristic distinguishes empirical statements from statements of formal sciences like logic, mathematics etc. Do conceptual modeling artifacts (languages, methods or models) have an empirical content that can be empirically tested?

The empirical content of typical modeling artifacts is doubtful. For example, the Entity-Relationship Model (ERM) provides a set of constructs that is useful to describe the world. But the ERM is not a description of the world by itself. A modeling method prescribes actions of how to build an information system. And a model built during systems analysis typically doesn't describe a given piece of reality, but specifies characteristic features of an information system that should be implemented in the future. From this perspective empirical research strategies are of less importance in the field of conceptual modeling.

However, it is not necessary to understand modeling artifacts as theories having an empirical content. Modeling artifacts might also be interpreted as tools used in systems development or as norms being established in modeling practice. This view raises several important questions:

- Does modeling improve domain understanding?
- What are the effects of applying modeling artifacts?
- Does modeling speed up systems analysis?
- Does modeling decrease the error rate during systems implementation?
- Does UML outperform classical structural approaches during systems analysis and design?

It is obvious that these questions cannot be answered only by formal investigations of pure syntax and semantics. In fact, modeling artifacts are information products that are used for different purposes. Besides technical purposes, one important objective is to facilitate the communication between different system's stakeholders, e.g. end-users, analysts, or programmers. In other words: Modeling artifacts are often produced and interpreted by humans – and not by machines. So, it is necessary to develop theories describing, explaining and predicting conceptual modeling practices. The scope of such theories is *about* the application of modeling artifacts.

Besides the desired effects, a modeling artifact may cause several (undesired) side effects. For example, conceptual modeling may decrease the ability to react on a rapidly changing system's environment. These side effects might be foreseen and considered by the developer of a conceptual model. However, these predictions may be false, and thus have to be tested. In addition, several side effects cannot be foreseen and must be examined by empirical studies based on real modeling processes. These studies may bring out that a new modeling language is not as useful as predicted. So, it is obvious that theories about conceptual modeling have empirical content and must be empirically tested.

Furthermore, theories about modeling artifacts may guide the development of new modeling languages. For example, if it is confirmed that systems analysts have problems understanding ternary relationships, it might be better to prevent their usage in modeling practice. In fact, it might be sensible to design modeling languages that do not support ternary relationships.

Which empirical research methods are useful in conceptual modeling? We can think of the full spectrum of empirical research strategies. For instance, case studies may be used as a first demonstration that new modeling languages might work. This approach suffers from its lack of objectivity and its weak generalizability because the investigated cases are often not representative and the results of these investigations are biased by researcher's interpretations. So case studies allow gaining useful exploratory information on real modeling processes and might be complemented with other approaches. For instance, a laboratory experiment allows investigating the influence of different modeling languages on user comprehension. There are many more empirical research methods (field experiments, surveys, action research etc.) that have their specific strengths and limitations. Hence we argue that different methods should be used to develop and to test theories about the application of modeling artifacts.

Are there reasons why empirical research strategies are often not used in the area of conceptual modeling? Empirical research strategies have to deal with subtle challenges: For instance, the user of a modeling language has to be familiar with the language before he/she can use it appropriately. The users' practical or theoretical background knowledge may affect the evaluation results in an uncontrollable manner. Furthermore, economic or social objectives of conceptual modeling projects cause additional influences on the results. As a consequence of these challenges, the appropriateness of empirical research strategies for the evaluation of conceptual modeling artifacts is unclear and may not be seen as useful.

We have to admit that the aforementioned objections make empirical research rather challenging. But we have to point out that these objections have an empirical content and must be empirically tested. In other words, the knowledge about the problems gained by empirical research strategies already provides interesting insights into the area of conceptual modeling. Such objections demonstrate that it is not easy to show the usefulness of modeling. Thus, empirical research can explicate confounding variables and demonstrate that a particular modeling approach is not useful in all but some situations.

To conclude, modeling artifacts do not need to have an empirical content. However, it is necessary to develop theories *about* modeling artifacts. It is obvious that such theories are about factual reality, namely about people using modeling artifacts for systems development. Such theories can only be tested by empirical research strategies. Hence, we argue that the results of empirical research strategies add value to the body of conceptual modeling knowledge.

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Contribution

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Conceptual modeling is a pivotal research topic in 'Wirtschaftsinformatik'. At the same time, it is at the core of the discipline's curriculum. Also, it seems reasonable to assume that work on conceptual modeling has significantly contributed to the discipline's profile and its relevance in practice. Despite this success story, research on conceptual modeling is facing a severe challenge: the need for *validation*.

The notion of scientific research is based on the idea of progress – in terms of growing knowledge and improving technologies. Progress, however, implies the existence of criteria that allow for discriminating between competing options – be it explanations of reality or artifacts that help to cope with it. A research discipline that does not seriously care about such criteria risks sacrificing its identity. In other words: Suggesting that a research result contributes to scientific progress requires giving convincing *reason* why it is an improvement over existing solutions. This leads directly to the topic of this discussion: Is empirical research the solution to the evaluation challenge in conceptual modeling research? To discuss this issue in more detail, let us first look at typical questions that occur with the validation of research results in conceptual modeling:

(a) Does a modeling language offer concepts that cannot be expressed equivalently in other modeling languages? (b) Is a modeling language better suited as a tool for business analysts than others? (c) Does a modeling language allow for designing models, which can be automatically transformed into executable software? (d) Is a reference model suited to guide the development of information systems that promises superior competitiveness?

Apparently, most of these questions relate to the use of modeling artifacts in the real world. Hence, this seems to be the case for empirical research. Empirical research in general is based on the idea of systematic access to reality in order to evaluate and/or generate hypotheses/interpretations. In the natural sciences, the validation method of choice is based on a concept of *truth*, i.e. by comparing research results against reality. The concept of truth applied in the natural sciences refers usually to critical realism: There is an objective reality that we can measure and/or perceive. However, perception as well as measurements may fail, which recommends skepticism (falsification). This type of empirical research was adopted by behavioristic research in the social sciences. On an international scale, behavioristic research has become the dominating research method in Information Systems. Its main promise is to contribute to objective, comparable descriptions/explanations of reality. However, it may only fulfill this promise, if there are one or more hypotheses that can be applied for their validation against 'objective' features of reality. Unfortunately, this is often not the case with modeling artifacts. Reference models are typically not just abstractions of a variety of existing systems. Instead, they are usually intended to guide the design of systems yet to be built. In other words: They describe possible *future worlds* that (should) offer advantages over existing worlds. A solution to this epistemological problem could be to investigate prospective stakeholders' judgments of reference models. Note, however, that this would result in statements about people's opinion only, not in an evaluation against reality. Also, many potential stakeholders will not be capable to fully understand the impact of a particular reference model. Additionally, their preferences may vary over time. Moreover, developing conceptual models imposes the challenge to evaluate modeling *languages*, since a modeling language (its semantics, abstract syntax and graphical notation) has a pivotal impact on the quality of models. 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. Therefore any research that aims 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 it. At the same time, any language we use for this purpose will bias our perception and judgment – or, as the early Wittgenstein put it: "The limit of my language means the limit of my world." Behavioristic research that ignores these obstacles bares the risk to sacrifice a differentiated appreciation of modeling artifacts for the illusion of scientific objectivity. Nevertheless, behavioristic research is suited to foster progress in conceptual modeling. This is especially the case with investigating the *actual* use of modeling methods. However, this kind of research is at least in part subject of other disciplines, such as cognitive psychology or linguistics.

What are the alternatives to behavioristic research? With respect to empirical research, a hermeneutic access to reality is a further option. It is based on the concept of interpretative or discoursive truth ('understanding' vs. 'explaining') – and it does not depend on a comparison against reality. Instead, it is aimed at discoursive judgments of current or future systems. This can be helpful for studying requirements to be met by modeling artifacts. However, hermeneutic approaches have a serious shortcoming, which can be a thread to scientific objectivity and freedom. They depend on rational discourses, which require the participants to have certain skills and obey a number of rules. But who is going to decide which group of people is best suited to participate?

Both types of empirical research come with a problem that cannot be ignored: Conducting empirical studies in a convincing way will often require an amount of time and resources that most research institutions cannot afford. If empirical studies are conducted nevertheless, it is likely that they will not contribute to the construction or evaluation of interesting *theories*. It seems that especially behavioristic research is often not used as an epistemological instrument but as a vehicle that serves one purpose only: gaining legitimacy – for a single author as well as for the information systems discipline in general. It is needless to emphasize that we should beware of this kind of research in conceptual modeling.

Last, but not least, there is one more approach to evaluate modeling artifacts. It corresponds to common practice in engineering disciplines or computer science. It stresses the comparison of a solution against precisely specified requirements. In an ideal case, it can be proved that the requirements are being fulfilled. For requirements and solutions to satisfy academic standards, they need to stress a high level of abstraction and some degree of comprehensible originality. This approach recommends thoroughly and precisely describing requirements as well as comparing the solutions against existing ones. It fails, if requirements (or design objectives) cannot be specified in a precise and comprehensive manner, e.g. with questions (b), (d).

To summarize, there is need for evaluating research results in conceptual modeling using procedures that satisfy academic standards. Due to the specific nature of conceptual modeling, it is not enough to simply deploy one particular research method, such as behavioristic research. Instead, there is need to develop – and eventually agree upon – specific standards for research on conceptual modeling. I do not think that there is a silver bullet. However, it seems that a pluralistic approach is the only option. Depending on the peculiarities of the research topic to be addressed, it would make eclectic use of empirical or engineering methods. Pluralism is also related to cross-disciplinary research, since conceptual modeling includes topics that are subject of other disciplines, like human perception (of language and information artifacts) or the impact of language on human thought and social interaction.

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Empirical Research in Conceptual Modeling – A Theoretical and Practical Imperative

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The "Analytical Advocacy" Model of Research

The majority of conceptual modeling research currently follows what has been described as an *analytical advocacy* model (see Figure 1). This is an approach that is frequently used in software engineering and computer science research but criticized as being unscientific [e.g. FePG94; Glass94; Hatt98; Tich98; ZeWa98]. In this approach, researchers describe some new technique or method in detail, make claims about its potential benefits, justify it using logical or theoretical arguments, apply it to an example and recommend that it be adopted in practice. Usually what is missing is any empirical evidence, making it difficult to distinguish valid claims from spurious ones. In more mature fields such as medicine, it is mandatory for researchers to conduct empirical research to evaluate the efficacy of proposed new practices prior to advocating their use [SRRH97]. However in conceptual modeling research, it is often sufficient for researchers to argue on logical or theoretical grounds that their approach is effective.

Merely saying that a technique is effective and providing arguments as to why it *might* be effective conveys no real information [ZeWa98]. Making such claims without empirical evidence and on the basis of logical arguments, theoretical arguments, examples or anecdotes is unscientific [FePG94]. Properties of methods cannot be proved formally or deductively, only by empirical evaluation. Examples, even if they are extensive, really only illustrate a method rather than validate it. Also, the fact that the method is both applied and evaluated by the researcher(s) who proposed it to an example they chose or developed themselves, means that it does meet standards of scientific objectivity [ZeWa98]. A scientific discipline cannot live off such weak evaluations in the long term [Tich98].

The primary criterion for validation of scientific knowledge is not theoretical or logical argumentation but whether it is consistent with observed facts [Popp63]. Examples and argumentation don't satisfy the principle of *falsification*, which is the single most important criterion for distinguishing science from pseudo-science. According to Popper [Popp63], any genuine test of a scientific claim should be an attempt to falsify or refute it. Examples and arguments are used only to support claims, not to falsify them.

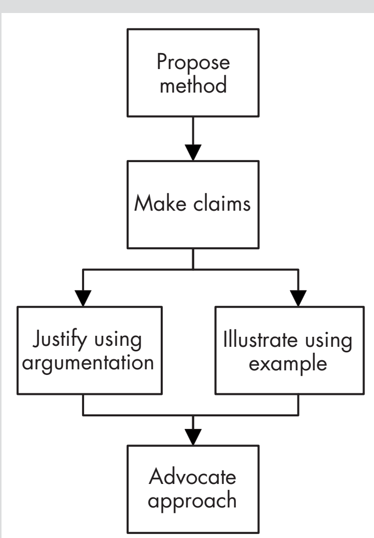

Figure 1 Analytical Advocacy Model: Validation by Argumentation and/or Example

Theoretical Imperatives for Conducting Empirical Research

For conceptual modeling to be regarded as a legitimate research discipline, it must follow established methods for scientific enquiry. *Empiricism* is one of the central principles of the scientific method, which states that scientific claims are subject to and derived from observations about the world [Neum00]. In the *scientific method* (see Figure 2), predictions are made based on theory and data is collected to evaluate whether the results confirm or disconfirm predictions [FePG94]. Any proposed conceptual modeling technique includes claims about its efficacy relative to existing methods. In the absence of supporting empirical evidence, these claims must be considered as conjecture only. However such claims represent predictions about the efficacy of the technique in practice, so can be empirically tested. Empirical research thus provides a way of objectively evaluating the truth of these claims.

Empirical research provides an essential evaluation role in a research field: it provides objective evidence about the effectiveness of different approaches that can be used to identify the most promising ones and to improve them [Hatt98]. This can help channel research into the most productive directions and avoid wasting effort on approaches that are unworkable [Tich98].

Practical Imperatives for Conducting Empirical Research

Empirical research is also important for informing practice. The conceptual modeling field is inundated with different modeling approaches and researchers are producing new ones all the time [SiRo98]. These results in confusion for practitioners: there are so many competing approaches and little objective data available to help them make informed decisions about which to use [Siau04]. Empirical research can be used to evaluate the efficacy of different approaches and so build up a reliable "evidence base" to support decision making in practice [Mood03]. Decisions in practice should be made based on facts (empirical evidence) rather than intuition and opinions [FePG94; Hatt98; SiRo98; Tich98].

How much is Enough?

The lack of systematic literature reviews means that current levels of empirical research in conceptual modeling are difficult to estimate precisely. A recent review of research in one research area within conceptual modeling (conceptual model quality) showed the percentage of empirical papers to be around 20% [Mood05]. There is no *a priori* reason to think that this research area is different to any other, so this is likely to be indicative of conceptual modeling research as a whole. According to Tichy [Tich98], a mild requirement for empirical research in a field is that each new idea should be followed by at least two empirical validation studies. This means that around two-thirds of papers should be empirical, which is more than three times the current level.

Conclusion

In this paper, I am not arguing that research into new and improved conceptual modeling techniques (*formulative* research) should be abandoned, but that there should be a better balance with empirical (*evaluative*) research: currently the proportion is grossly skewed. New ideas will always be needed, but empirical research is required to find out how good these ideas really are and how they can be improved. As Hatton [Hatt98] says: "Whatever direction we take, any attempt to improve in the absence of measurable feedback seems doomed to fail, however much fun it may be."

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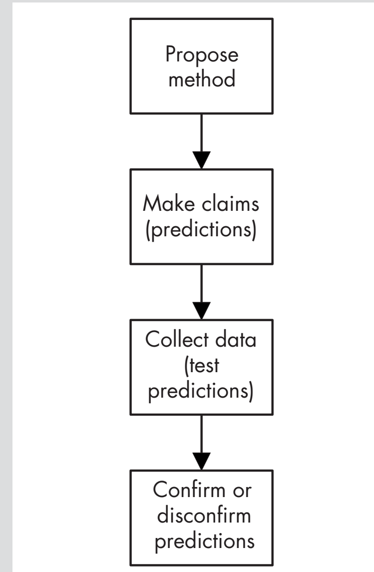

Figure 2 Scientific Method: Empirical Validation

Empirical Research in Conceptual Modeling – Using Experiments to Understand Semantic Expression

Prof. Jeffrey Parsons, PhD

Many conceptual modeling grammars have been proposed. However, most have had little impact on practice. In the wake of this largely unsuccessful research endeavor, attention has turned more recently toward empirical evaluation of grammars widely used in practice, in order to identify their strengths and weakness and/or to propose improvements to the techniques. The growing interest in empirical evaluation indicates that important progress is possible in our understanding of conceptual modeling techniques, and that an improved understanding might ultimately leading to better techniques and guidance for using them.

Empirical evaluations of grammars fall into two broad categories [WaWe02]. First, evaluation can be based on criteria related to constructing scripts using a grammar, as it was done in many early studies in this area. In this case, evaluation focuses on issues related to the analyst who develops the models, such as understandability and usability of the grammar, or the perceived quality of scripts developed. Second, evaluation can be based on criteria related to interpreting scripts constructed using the grammar. In this case, evaluation focuses on the degree to which scripts facilitate communication about, and understanding of, the semantics of the modeled domain. In my view, the second kind of evaluation deals with more basic conceptual modeling questions that must be understood before undertaking evaluations related to script construction. The remainder of this discussion deals with issues in conducting "interpretation" evaluations effectively.

To contribute effectively and systematically to improving understanding of conceptual modeling techniques, we should evaluate by testing theoretical predictions about the quality of scripts constructed with conceptual modeling techniques used by practitioners. Early empirical research evaluating conceptual modeling techniques consisted largely of intergrammar comparisons. Such studies generally lacked strong theoretical foundations, making it difficult to understand the factors contributing to significant differences in performance of the techniques being compared. Moreover, some studies compared techniques intended for different purposes (e.g., modeling state versus behavior), making it difficult to understand the meaning of observed differences on measures of quality such as ease of use.

Recently, researchers have begun using theoretical foundations to conduct intragrammar studies. Such studies typically compare variants of a single grammar to determine whether scripts constructed according to one set of rules (typically motivated by theory) are in some way better than those constructed according to existing rules (or conventional wisdom) of the grammar. One promising class of such studies has used the ontology of Mario Bunge [Bung77] to predict the effectiveness of certain conceptual modeling practices involving state representation in grammars such as the ER model (e.g. [BuWe99; Gemi99; BPSW01]). These studies follow two simple premises. First, a conceptual model represents aspects of the perceived real world; thus, conceptual modeling grammars should contain constructs that correspond to how humans think about the real world. Second, since ontology deals with the nature of the real world, it serves as a natural foundation for studying conceptual modeling constructs.

Among the complicated issues to be resolved in this kind of work is determining whether and how certain ontological constructs are or should be reflected in specific conceptual modeling constructs. For example, [BuWe99] using Bunge's assertion that "properties do not have properties" to predict that relationships should not have properties; instead, separate relationships between classes should be modeled instead. However, an alternate interpretation is that relationship properties represent Bunge's notion of "property precedence" and, hence, are ontologically justified [PaCo04]. Clearly, work is needed to find ways of assigning a sound ontological interpretation to conceptual modeling constructs. Moreover, Bunge's ontology focuses on things and states, and many studies using Bunge to evaluate conceptual modeling grammars focus only on static elements of the world. Other ontologies focus on events, and it would be particularly useful to study their relevance for conceptual modeling techniques that focus on behavior.

There is tension in conceptual modeling research between the control offered in a laboratory setting and the realism that can only be achieved by studying how methods and techniques are used in practice. This tension is misleading, as both kinds of research are needed. A staged approach to conceptual modeling research, focusing first on understanding and improving the capacity of grammars to represent domain semantics, is needed. A key empirical question in understanding conceptual modeling techniques is how domain semantics is represented using a technique's grammar. Our current understanding of basic representational issues in conceptual modeling is poor. Much additional research in this area is essential for continued progress in this field. For example, graphical techniques use symbols that are combined to express certain knowledge about the problem domain. It is critical to understand how different grammatical rules facilitate or impair understanding of domain semantics. This is best studied in a laboratory setting, where the effects of representation mechanisms can be studied in isolation. The complexity and lack of control in real world settings makes it nearly impossible to isolate such basic effects.

A solid understanding of representation issues is necessary to build a foundation for more ambitious research. Inadequate understanding of how grammars express domain semantics can only impede our ability to understand how grammars interact with other factors in practice, such as analyst experience or domain complexity, in constructing high quality models. Field studies can bring needed perspective to laboratory experiments, but should be used cautiously until we more fully understand basic representation issues.

Conceptual modeling is a core information systems topic. A concerted effort from our research community is needed to bring scientific discipline to bear on this area.

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Just Do It

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The scope of conceptual modeling has expanded significantly over the last two decades and the initial focus on systems analysis and design has been widened substantially. Nowadays, conceptual modeling is not only conducted for a variety of applications (*e.g.,* workflow management or enterprise systems), but increasingly it is also used for non-IT requirements engineering (*e.g.,* business process management or compliance with regulations such as Sarbanes-Oxley).

The design-related challenges of conceptual modeling, *i.e.* designing new modeling methods and grammars, building corresponding prototypes, or extending existing methods and grammars, are researched intensively. Yet the proliferation of proposed modeling methods and grammars (Yet Another Modeling Approach – YAMA [OHFB92]) can be seen as one indication that the community still has not addressed the core issues of conceptual modeling. Empirical research might help us. But unlike many other areas of information systems research, conceptual modeling is an area with a rather limited utilization of empirical research methods. Why is that the case?

Overall, it seems that the majority of conceptual modeling-related research can be characterized as curiosity-driven. As a consequence, there are similarities to research in the field of operations research. The challenges in both disciplines are perceived by many researchers as intellectually stimulating. This includes in conceptual modeling, for example, tasks such as designing new modeling methods and grammars. Research in this context has often been non-empirical for three reasons. First, new research challenges related to constructing new artifacts could easily be identified. Second, research on conceptual modeling is often conducted by researchers without experience in (and passion for?) empirical research. Third, empirical research must be theoretically grounded to come up with conceptually interesting research questions; since theoretical reference was often lacking, modeling-related research could often not be linked to empirical investigations.

One major problem of conceptual modeling *in practice* is the complexity of conceptual modeling as expressed by the sheer endless number of modeling methods and grammars, their degree of sophistication and the possible interrelationships between them. An interesting piece of empirical research in this context is Erickson and Siau's work [ErSi04] on determining the (practical) complexity of UML. Using a Delphi study they identified a subset within a use-based UML kernel, which represents the most commonly used constructs. Such an outcome can directly feed into UML-related education or the pre-configuration of UML tools.

Personally, I see a significant demand for quantitative and qualitative empirical research on conceptual modeling offering promising opportunities for researchers in this area. This should not compromise, however, the current research streams, but meaningfully complement them.

As [BaMa95] showed, the research on modeling seems to be decoupled from the areas of interest for practitioners. *Exploratory* empirical research in the early phases of the research lifecycle can provide important insights into the practice of modeling. Grounded in sound quantitative and qualitative research methods it can provide valuable guidance for (more) relevant research in terms of being interesting, applicable, and current [BeZm99]. This situation is also the classical application area of major issue studies in which the main challenges of a domain under analysis (here, conceptual modeling) are to be discovered.

Studying the practice of modeling itself can also lead to new research topics [DGRG04]. Here are some examples of typical research questions selected from our current research projects utilizing case studies, focus groups and surveys: What are the major issues of large scale modeling projects? What are the main issues of utilizing SAP reference models? What are the critical success factors of business process modeling? All these topics have been identified from a pool of major issues with modeling and have been articulated by system vendors and business analysts.

Besides exploratory research, empirical research is of course used for *explanatory and evaluative* purposes. Explanatory work is required if the research is concentrated on gaining a deeper understanding through the identification of statistically significant relationships (*e.g.*, testing a model of success factors and success measures of business process modeling). Evaluative research is important as a phase that follows, *e.g.* the ontological analysis of modeling grammars. For example, we recently interviewed 21 experienced ARIS users in order to test ontological shortcomings of the ARIS meta model that have been identified in prior research. One contribution of this type of research is the comparative evaluation of modeling methods, grammars and tools.

Empirical research on conceptual modeling faces of course *key challenges*. I briefly outline just four selected challenges. The first, and most important, challenge is asking the right questions. Empirical research faces the danger that it is (mis-)used to confirm rather obvious propositions. Respective researchers might utilize quantitative methods aiming for rigor, but fail to address relevant research questions, which is the all important 'So what?' A second hurdle is gaining access to sufficiently qualified participants. The relevance and credibility of empirical research is limited, if, for example, students are utilized as proxies for experienced practitioners – a shortcoming of many academic research projects. The reasons are typically the convenient access to this group as well as the lack of appropriate contacts to practitioners. For the credibility and relevance of future empirical research it will be important that feedback is increasingly consolidated from experienced modelers and model users, who apply conceptual modeling in a real life context. However, sometimes this experienced sample set can be just impossible to obtain. For example, where do I find business analysts experienced in the proposed new standard Business Process Modeling Notation (BPMN)? Third, it is still challenging to clearly define "conceptual modeling". Conversations with different stakeholders quickly reveal that different people have different interpretations when it comes to conceptual modeling as the purposes and contexts can vary significantly. Fourth, the dynamic development of conceptual modeling methods, grammars and tools is a challenge in two ways. On the one side, it demands a certain response time of empirical research projects (so we do not study outdated tools or specifications). On the other side, it also means that results of an empirical study can quickly be outdated.

In summary, the question cannot be, if and how much empirical research on conceptual modeling is required. Along the phases of a research project, empirical research in all its facets cannot only complement all stages, but also provide ways to identify *new and relevant* research topics. It is time for the modeling research community to acknowledge that "the proof of the pudding is in the eating". Empirical evidence should be more than waiting for the uptake of a proposed new modeling method or grammar in practice. And this is where empirical research with its exploratory, explanatory and evaluative power is indispensable.

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On the Appropriateness of Empirical Research Strategies in the Field of Conceptual Modeling

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Research Goals of Information Systems (IS) as an Academic Discipline

Each academic discipline is based on at least three constituents: (1) a subject, (2) research goals which refer to this subject, and (3) a set of theories, methods and procedures which are used to achieve these goals.

The subject of IS is denoted by the name of the discipline: Information systems in business and administration. In the following, we refer to an information system as the information processing subsystem of a business system [FeSi01]. The tasks of an information system are carried out by human and machine actors. Machine actors of information systems are called application systems. Thus, an application system is a component of a comprehensive information system.

The academic discipline IS deals with planning, development, implementation, operating, management and evaluation of information systems. From a more general viewpoint, the goals of IS refer to analysis and design of information systems. Here we have to keep in mind a major difference between IS and related sciences, e.g. social sciences. In contrast to societies, information systems and in particular application systems predominantly are artifacts. They do not evolve but have to be explicitly designed. Consequently, design goals play an important role within IS. In other words: IS is essentially an engineering discipline.

Conceptual Modeling of Information Systems

Conceptual modeling isn't an end in itself. Conceptual models are for an information systems designer what engineering drawings are for a civil engineer or a mechanical engineer. Thus, conceptual models are the most powerful utilities to achieve the goals of IS. The purpose of a conceptual model is to facilitate analysis and design of complex information systems. Therefore, a comprehensive conceptual model must provide a multi-perspective, multi-view representation of the information system or subsystem under design. Relevant perspectives are e.g. the outside and the inside perspective. Views focus on specific characteristics from a given perspective, e.g. structure and behavior from the inside perspective of an information system.

The "basic tool kit" of conceptual modeling includes:

- modeling languages (meta models) which define types of building blocks and relationships between building blocks as well as corresponding rules and constraints,
- process models to guide the execution of the modeling task, and
- software tools for computerized support of the modeling task.

A modeler who is equipped with heuristic knowledge and experience of how to map a certain real world issue into the schematic representation of a given modeling language is able to use such a methodology and to produce solid results, e.g. a conceptual data schema representing the data view on an information system.

Nevertheless, the basic tool kit is not sufficient for comprehensive modeling of information systems. In fact an "advanced tool kit" comprising additional tools is needed, e.g.:

- integrated meta models, based on powerful metaphors, allowing harmonized modeling of different perspectives and views on a complex information system,
- architecture models, which help to manage the complexity of models by dividing them into different layers, subsystems and views,
- reference models and patterns, providing reusable heuristic modeling knowledge,
- ontologies, which help to capture the semantics of building blocks and relationships.

Mastering an advanced methodology like this in order to develop a comprehensive conceptual model of a complex information system marks the level of "craftsmanship of conceptual modeling".

We all know that there is never the one and only right model. Rather there are more or less appropriate, more or less complex and more or less understandable models. This leads to the next level of modeling maturity, the "art of conceptual modeling" (analogous to Don Knuth, The Art of Computer Programming [Knu97]). From the viewpoint of constructivism, a modeler perceives the real world, interprets the relevant part of the real world, separates it from its environment, and reconstructs the relevant parts of the real world and its environment in the form of a conceptual model using a given methodology. All this is subject to the modeler's understanding of the modeling goals and objectives as well as to his or her methodological knowledge and modeling experience.

Research on Conceptual Modeling

In the last years, extensive research has been done on conceptual modeling. As recent academic conferences show, many questions meet ongoing interest and the list of research topics is even growing. Current themes include:

- utilization of ontological concepts for conceptual modeling,
- meta-modeling,
- generic models,
- verification and validation of models,
- agile modeling and extreme modeling,
- model engineering,
- semantics-preserving model transformation,
- model-driven architecture, and
- automatic processing of models.

The examples show that conceptual modeling is a very agile field of research in IS. Wand and Weber propose a research agenda on conceptual modeling in IS based on the question "How can we model the world to better facilitate our developing, implementing, using, and maintaining more valuable information systems?" [WaWe02].

Empirical Research on Conceptual Modeling

The goal of empirical research is to observe, describe, analyze and explain phenomena around the subject of investigation. The "tool kit of empirical research" comprises techniques like case study, action research, experiment, enquiry, interview, observation, field study etc. What can empirical research contribute to conceptual modeling in IS? Some examples for conceivable contributions are:

- Empirical research can look over the shoulder of a modeling craftsman or artist and help to identify best practices of modeling and thus encourage the improvement of model construction.
- Empirical research can observe users interpreting models and thus help on learning about clarity and understandability of models.
- Empirical research can help to compare different modeling methodologies in specific modeling scenarios.

As these examples show, empirical research can support the further development of conceptual modeling methodologies. Thereby it can unveil surprising results. Bowen, O'Farrell and Rohde report on an experiment on the relationship between the level of ontological clarity of data structures and query performance. The results indicate that users of the ontologically clearer implementation of the data structure made significantly more semantic errors, took significantly more time to compose their queries, and were significantly less confident in the accuracy of their queries [BRF04].

Empirical research on conceptual modeling is both challenging and error-prone. One of the pitfalls is to underestimate the complexity of modeling scenarios. To give a negative example: investigating the proliferation of the different UML diagrams would produce only poor insight if methodology and context of the modeling scenarios are not considered sufficiently.

Conclusion

Empirical research, facing the challenges and pitfalls, can provide valuable and complementary contributions to conceptual modeling in IS. However, an increasing focus on empirical research must not disregard research on the tool kits as well as the craftsmanship and the art of conceptual modeling.

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Contribution

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Over the last 15 years, I have been involved in conducting empirical evaluations of conceptual modeling grammars with a number of colleagues. At the outset, our primary concern was lack of theory to guide our empirical work. We lacked theory that we could use to predict the strengths and weaknesses of conceptual modeling grammars. As a result, we had no formal basis to guide our empirical work. We had to use our intuition and experience to select those features of conceptual modeling grammars that we would evaluate. Similarly, we had to use our intuition and experience to create research contexts in which the features that were our focus could be evaluated. Given the number and complexity of phenomena associated with conceptual modeling activities, however, we suspected our research (like so much earlier research) would produce few useful results. In the absence of theory to guide our work, we quickly found we were right. Our research results were uninteresting, if not useless.

When we recognized that theories of ontology might provide us with a theoretical basis to predict the strengths and weaknesses of conceptual modeling grammars, we felt we had made an important breakthrough in our work. We could now evaluate conceptual modeling grammars in terms of how well they instantiated the constructs of an ontological theory. We could use concepts like construct overload, construct redundancy, construct excess, and construct deficit to predict the strengths and weaknesses of conceptual modeling grammars as a means of representing real-world domains. Our subsequent empirical work produced more-compelling results.

Lack of theory still continues to undermine empirical work that is aimed at evaluating conceptual modeling grammars. In this regard, my colleagues and I still see few alternatives to ontological theories to provide the theoretical basis for our work. Moreover, while a number of other colleagues have argued that the ontological theories we have used so far are too few, too restrictive, or of limited quality, we see little, if any, evidence of additional, substantive theoretical work to redress these concerns. It is also clear that new conceptual modeling grammars continue to be designed in the absence of theory. For instance, I continue to wonder what theories guide the designers of UML in their work.

Even if we were fortunate enough to have a plethora of good theories to guide empirical work on conceptual modeling grammars, my experience is that we would continue to confront three major problems. The first is that we still lack clarity about the nature and purpose of conceptual models. For instance, some recent empirical work has evaluated end-users' performance using "ontologically sound" versus "ontologically unsound" conceptual models when the users query a relational database via SQL. It is not clear to me, however, that this task is an appropriate context in which to evaluate conceptual models empirically. A conceptual model is intended to be a faithful representation of someone's or some group's perceptions of the semantics of some real-world domain. One might argue that the only basis on which conceptual models should be evaluated, therefore, is in terms of how well they represent stakeholder perceptions of a real-world domain. They may or may not help users to create the sorts of mental models that assist them to formulate SQL queries successfully when they interrogate a relational database. In short, the tasks used to evaluate conceptual models in empirical research need to be congruent with the purposes of conceptual models. Currently, however, we do not have a clear definition of the boundaries of tasks where conceptual models might be useful. In the absence of a clear understanding of tasks, we also do not have a clear understanding of stakeholders and their roles. Who are the stakeholders who need to engage with conceptual modeling grammars in their work? What roles do these stakeholders play? Unless these matters are clear, research designs that are developed to evaluate conceptual modeling grammars potentially will be flawed fatally.

The second problem relates to the difficulties experienced in trying to evaluate conceptual modeling grammars in realistic contexts. In this regard, much prior empirical research has used experiments to evaluate conceptual modeling grammars. Realism has been traded off in an effort to control factors that might confound performance when users employ conceptual modeling grammars. As a result, the tasks and the conceptual models employed in the research are small and constrained. They do not reflect the types of demands that would be placed on users in typical organizational contexts. The alternative is to undertake case studies or action-research studies in which users employ conceptual modeling grammars in realistic contexts. As researchers, the problem we then confront is that many factors may affect users' performance with a conceptual modeling grammar. For instance, factors like task complexity and users' experience with a grammar may obfuscate results to the point where it is impossible to tell whether theoretically based propositions are supported or not supported. In a nutshell, we face the same problems that programming researchers faced in their concerns about whether results obtained from "programming in the small" would hold when the context switched to "programming in the large."

The third problem relates to the effects of history. Users of conceptual modeling grammars must have some experience of conceptual models and conceptual modeling grammars before they can employ them effectively. In empirical work we have undertaken, my colleagues and I have found it difficult if not impossible to mitigate the effects of prior training in and experience with particular data modeling approaches. For instance, many modelers do not understand the difference between conceptual modeling and data modeling. Many seem forever constrained by views of the world shaped by the first modeling grammar they learned – for example, the entity-relationship modeling grammar or some type of object-oriented modeling grammar. Many seem unable to conceive of the world except via the third normal form relations of the relational model. Many are committed strongly to a particular modeling approach and are reluctant to change. Unless history can be undone, however, the research results obtained during evaluations of conceptual modeling grammars are contaminated. Currently, I do not see a simple solution to this problem. Once some learning of a modeling approach has occurred, it seems to impact subsequent learning inextricably.

In summary, I believe high-quality empirical work on conceptual modeling grammars will not occur in the absence of high-quality theory. After more than 30 years of work on conceptual modeling grammars, however, we still lack an abundance of good theory (which is perhaps a manifestation of the difficulty of the task). Nonetheless, even if good theory were widely available, designing empirical tests of theoretical predictions about conceptual modeling grammars that have both internal and external validity has proved to be a daunting task. We sorely need exemplars to guide our work. Without them, we will continue to have difficulties building a cumulative base of empirical research on conceptual modeling grammars.

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