Relevance and Problem Choice in Design Science

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Abstract. The supposed opposition of rigor versus relevance is based on the mistaken idea that rigor consists of linear technology transfer combined with positivistic science, and ignores the context-dependence of relevance as well as the incorporation of conditions of practice necessary for applicability of knowledge. Historical insights from the history of science and technology show that technology is not transferred linearly from research to practice, and that technical science has more in common with social science than a superficial comparison would reveal. In both fields, (1) practical problems are often solved *without* input from research, and (2) researchers often investigate *past* innovations rather than prepare future ones. And in both fields, (3) relevance is context-dependent, because it depends on changeable goals of stakeholders. Applicability is a more important requirement than relevance to a goal, where applicability is the match between theory and the condition of practice of a concrete case.

This paper summarizes insights from the history of science and technology to substantiate these points and provides an extended framework for design science to incorporate these insights. Since relevance depends on problem choice, the paper also summarizes what is known about classes of relevant practical problems and research questions in technical design science and discusses the relevance of this for IS design science. We finally discuss implications for research methods, research strategy, and knowledge transfer in IS design science.

1 Introduction

In 1983, Schön posed the dilemma of rigor versus relevance as one between technical rationality, where problem-solvers select from alternative solutions the one that optimally contributes to an agreed-upon end, and real-world action, where practitioners use their experience and intuition to muddle through unique, uncertain and unstable situations [1, pages 39–43]. According to Schön, the rational problem-solving view of technical rationality assumes a positivist philosophy, in which basic scientific results are applied to practical problems. This may be appropriate in the technical sciences, he said, but it is not applicable to complex social situations. Rather than emulate technical sciences in an attempt to be rigorous, let's emulate practitioners and be relevant, Schön said. Hence the dilemma of rigor versus relevance.

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Since the 1990s, Schön's dilemma has been subject of discussion in information systems (IS). Various ways out of it have been proposed. Benbasat & Zmud [2] proposed to increase relevance by selecting interesting problems, accumulating context-rich knowledge about those problems and transferring the results to practitioners, all the while preserving the rigor of basic research methods. Davenport & Markus [3] propose a more applied research approach, emulating consultants to select relevant problems, borrowing research methods from evaluation and policy research to investigate them, and transferring the results to students who will later enter practice. Design scientists propose not just developing new knowledge but also new artifacts that solve practical problems [4,5,6,7]. This is an approach common in industrial research, namely developing an artifact to solve a problem, and then investigating the problem-solving properties of the artifact.

These three approaches assume that relevant artifacts and knowledge are developed in basic, applied or design research, respectively, and then transferred to practice. Historical research in science-technology interaction reveals a considerably more complex picture and there is no reason why this complexity should be absent from the interaction between IS research and practice. An analysis of science-technology interaction may reveal relevant implications for IS design science, that may increase our options for problem selection and research strategy.

A brief review of insights from the history of science-technology interactions reveals that in addition to the interaction between scientific research and artifact development typical of industrial research, technology often develops without input from science, and technical research often progresses by curiosity-driven research that solves no pressing practical problem (section 2). This motivates an extension of the framework for mutual nesting of practical problem solving and scientific research proposed earlier [8], which itself refines the framework of Hevner et al. [7]. The extension consists of adding a flow of goals and budgets from the economy to design science, and the production of practical knowledge by practical problem solving (section 3). Relevance of artifacts (the outcome of practical problem solving) and theories (the outcome of scientific research) is context dependent, for it depends on goals from the economy. Applicability of theory or artifacts, by contrast, depends on the incorporation of conditions of practice in the theory or in artifact behavior.

All three approaches listed above agree that relevance is determined, among others, by problem choice. Solutions to irrelevant problems will not be used and problem choice is therefore an important art for IS researchers [9]. Section 4 summarizes kinds of practical problems typically solved in design science, and gives examples from technical as well as IS design science. These problems have shown to be relevant for the economy and therefore there was budget available to solve them. The section also lists typical scientific research questions, with examples from technical and IS design science. This shows that incorporation of conditions of practice is an important prerequisite for applicability of design theories. In section 5 we discuss the implications for research methods, research strategy and transfer of results, and we return to the question to which extent we have now dealt with Schön's dilemma, and what part of it remains untouched by our analysis.

2 Science-Technology Interactions in History

The dominant view of the relation between science and technology is that it is a linear progression from basic research followed by applied research and development, ending with production and diffusion [10,11]. This is the linear model assumed by Schön to exist in the technical sciences. There are some spectacular examples of this in the history of science and technology. Shortly after Benjamin Franklin discovered that lightning is electricity, the first lightning rods appeared [12, page 154]; the theory of ultrasound developed in the 1870s was used in the 20th century in the development of sonar technology and medical ultrasound technology [13]; and basic research into polymer in the 1930s by Carothers at DuPont led to the invention of nylon, one of the biggest moneymakers of the company [14].

However, extensive historical research has shown that these are exceptions and that it is hard to impossible to discover a linear handover of knowledge from basic science to technology [11,13,15,16,17]. The short summary is this: New technology does not spring from science but from the improvement of existing technology; and this improvement is motivated by the desire to meet perceived stakeholder needs.

But if technology is sometimes, but not always, applied science, then what other kinds of relationships are there? Following Gardner [18], we can distinguish cases where (1) there is no relationship from cases where (2) science follows technology, (3) technology follows science, and (4) science and technology develop in interaction. The historically earliest cases are those in which there is no relationship. Science as we know it did not exist in most of human history, and when it finally did it often played no role in the development of much of the technology currently still on the market. A few examples suffice to make the point: In the invention of windmills, the stirrup, barbed wire, the zipper, the revolving door and many other kinds of artifact, science played no role.

The second class of examples is where science follows technology. There are two subcases. First, technology may be transferred to science in the form of instruments . Early examples are such as the telescope, thermometer, barometer and air pump in the 17th century [19,20,21]. The second subcase is that science may study existing technology to discover how it works. A famous example is the investigation of steam machines by Sadi Carnot in the early 19th century to discover why they actually worked, leading to the new science of thermodynamics [22,23]. This is a very common way of working. Galileo studied how machines like levers and pulleys used by ship builders actually worked, starting the new science of machines and of strength of materials [24, pages 36–46]. And over hundred years after lead-acid batteries were introduced, researchers still try to understand how they work [18, page 15]. More examples are given by McKelvey [15] and Gardner [18].

In the third kind of case, technology follows science. There are two subcases here too. In one subcase there is linear progression from science to technology, as in the examples given at the start of this section. The agreement among historians of technology is now that these are spectacular exceptions. The second subcase is more common, in which technologists encounter some problem for the solution of which they turn to already published scientific results [25]. These scientific results have been developed earlier, not knowing for which technical problems they could be useful.

The fourth and last case is where science and technology develop in mutual interaction. This is typical of 20th century industrial research [14,26,27]. It is also the mode of working proposed in design science. I now give a framework that can accommodate all kinds of interactions reviewed here.

3 An Extended Framework for Design Science

3.1 Mutual Nesting of Practical Problem Solving and Research

In our design science framework we distinguish two kinds of problem solving activity, solving practical problems and answering research questions [28]. A *practical problem* is a problem to improve the world with respect to some stakeholder goals. To solve it some artifact, such as a software system, technique, method, process, treatment, etc. is needed. A *research question* is a knowledge question to be answered by scientific research. To answer it, some validated proposition about the world is needed. This distinction leads to a refinement of the design science framework of Hevner et al. [7] shown in figure 1. In this framework, practical problem solving delivers artifacts with the aim of solving practical problems in an organizational environment, and design science research investigates properties of these artifacts.

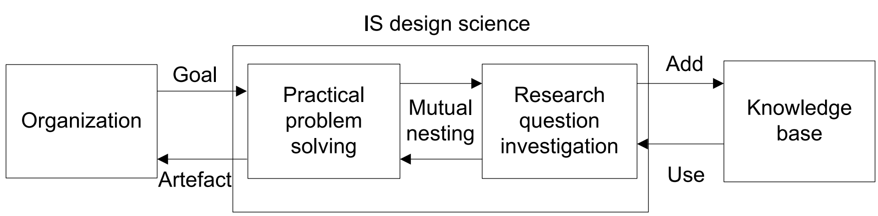

Fig. 1. Refinement of the framework of Hevner et al. [7], adopted from Wieringa [28]

3.2 Extended Framework of Interactions

To accommodate the historical insights from the previous section, we need to further elaborate this framework by indicating that research question investigation serves goals too, and that as any other activity it needs a budget. This gives us the elaborated framework of figure 2.

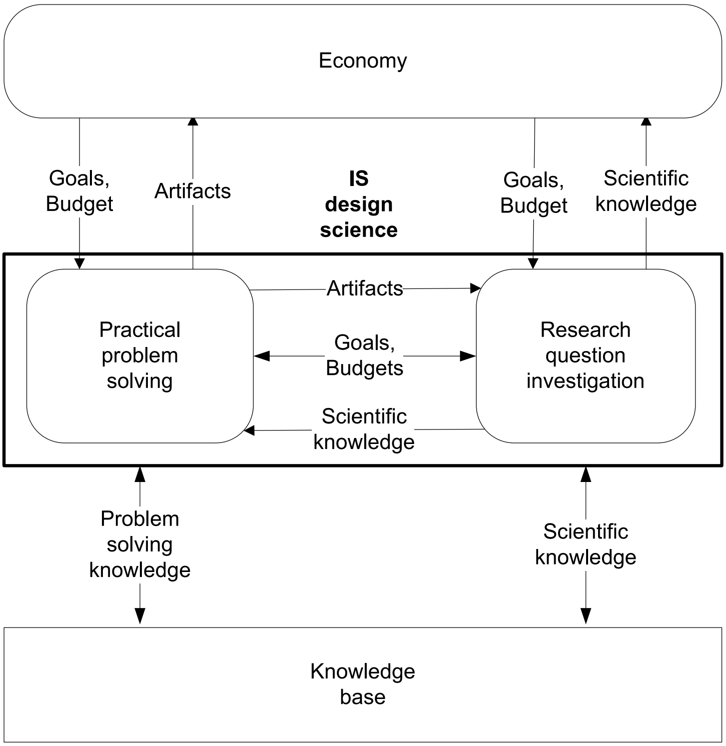

Fig. 2. Further elaboration of the framework for design science. Nodes represent activities or results of those activities (rounded corners) or enduring results of these activities (sharp corners), arrows represent flows of information, money or artifacts. Flow of control is not represented.

The nodes with rounded corners represent activities, not people or organizations. One person or organization can perform activities in any of these boxes at the same time. The sharp-cornered rectangle represents an enduring result of these activities, namely knowledge. The arrows represents flows of money, information or artifacts without indicating who triggers a flow: the sender or receiver. The arrows from the knowledge base box terminate on the design science box, meaning that these interfaces are accessible from both kinds of design science activities, practical problem solving and scientific research.

The environment of design science has been framed as the economy, which here is intended in a broad sense as the allocation of finite resources to goals, where not every goal can be allocated all the resources needed. In other words, in this paper we will view the economy as consisting of activities to achieve goals with a finite budget for means. My thesis is that these goals are the sources of relevance for design science.

I now discuss the interfaces of research question investigation and practical problem solving. Practical problem solving receives a budget to produce artifacts that are intended to achieve goals. This may add problem-solving knowledge to the knowledge base, an interface not shown earlier. Problem solving knowledge is what Vincenti [29, pages 217-222] calls practical considerations and design instrumentalities. *Practical considerations* consist of accumulated experience laid down in design procedures, rules of thumb, generalizations from observation not explained by scientific theory, etc. *Design instrumentalities* are how-to-do knowledge such as knowledge of procedures, ways of thinking, and judgmental skills that may partly be tacit.

Practical problem solving may also provide its artifacts to scientific research, for example as instrument to do research or as object of research itself. If transferred as instrument to do research, then apparently it received goals and budget from this research activity; if transferred as object to investigate, then it may transfer some of its goals and budget to the research activity and in return for that it receives scientific knowledge. Practical problem solving may also draw in the store of scientific knowledge published earlier, as indicated by the arrow from the knowledge base to the design science box. However, it cannot *add* scientific knowledge directly; this would involve a scientific research activity triggered by practical problem solving.

Research, like practical problem solving, receives goals and budget from the economy, either directly or indirectly from a practical problem solving activity. It returns the favor by producing knowledge, which is added to the knowledge based through scientific publication channels and communicated to the economy through the professional and popular press and, in the case of universities, in the minds of undergraduate and graduates who enter the economy. These channels should be used by IS design researchers too: The professional and popular press to reach managers and other practitioners (as urged by Benbasat & Zmud [2]) and students for a long-term upgrade of the workforce of practitioners (as urged by Davenport & Markus [3]).

The triangle relating the economy, practical problem solving and research was first proposed by Aitken [30], who used it to analyze the development of radio from laboratory instrumentation. It has independently been used by Lyytinen and King [31] to indicate that technology (corresponding to practical problem solving in figure 2) creates the economic surplus to make the budget available to investigate artifacts scientifically. The addition of this paper is to integrate the frameworks and elaborate the interfaces in the light of historical evidence.

4 Problem Selection in Design Science

Several historians of technology have inventoried classes of problems occurring many times in the development and investigation of artifacts [32,33,34,29]. In the next two sections I summarize this and discuss the relevance of these problem classes for IS design science. The practical problem-solving goals and design research questions listed below further refine the Practical problem solving and Research question investigation boxes in figure 1.

4.1 Sources of Relevance in Practical Problem Solving

Practical problems are characterized by improvement goals, but there are many special cases, as illustrated in figure 3.

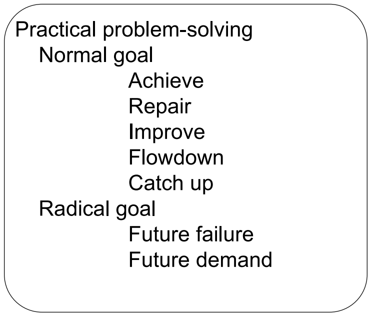

Fig. 3. Practical goals identified from the literature

– *Achieving some economic goal.* This is the normal model of practical problem solving, where the goal is set by some private (business or non-profit) or government stakeholder. Relevance is determined by the value of the economic goal.
– *Repairing failures.* When an artifact fails, a practical problem solver such as a technologists will try to diagnose the failure and repair it. Examples from IS are the attempt to configure an ERP system such that it stops failing to achieve the goal of cost reduction, or to improve effort estimation techniques so that effort estimation stops delivering underestimations.
– *Improving performance.* Even if an artifact achieves its goals satisfactorily, technologists and other practical problem solvers will aim to improve its performance. This will be an activity without economic budget if the problem-solver's honor is the only goal to be served, but if some economic stakeholder's goal is served by it too, budget may be available. For example, functionality and performance of a collaborative software tool may be improved by observing the behavior of its users and exploiting the possibilities of new technology; an implementation effort estimation technique may be improved by collecting data to fine tune the technique.
– *Flowing down system goals.* In the development of complex systems such as aircraft, overall system goals imply goals for subsystems such as the propulsion system or landing gear [32,29]. Deriving subsystem goals from the goals of the overall system is called flow-down in systems engineering. For example, implementation of an e-commerce sales channel may involve subsystems for order tracking, payment and security, and overall system goals will imply goals for those subsystems.
– *Catching up with large systems improvement.* Hughes [35] introduced the concept of large technological system as a system of diverse artifacts not

centrally managed but with a common goal, such as the system of private transport by car, which consists of car manufacturers, car financing, insurance, roads, petrol supply and legislation that jointly make it possible for individuals to drive cars. He also introduced the concept of *reverse salient* in large technological system as a part that holds back advancement of the system as a whole [33]. For example, a reverse salient for car transport by electrical cars is the scarcity of battery reloading stations. An example in IS may occur in value chain automation, where for example a shared goal of an extended enterprise may be thwarted by lack of an adequate information risk assessment techniques for extended enterprises.

- *Circumventing predicted performance limits.* In an analysis of jet engine development around the 1930s Constant [32] observed that a few visionary engineers predicted that contemporary aircraft propulsion technology would fail at higher speeds and altitudes, and also predicted that future economic goals would nevertheless require those speeds and altitudes. Constant called this kind of problem a *presumptive anomaly* but here I call it a predicted performance limit. This is interesting, for in contrast to all previous cases there is no experienced problem. Examples from information technology are the development of new computing paradigms (e.g. quantum computing) or storage technology to meet future performance limits. Examples from IS are harder to give, probably because in the technical cases just mentioned, future performance limits can be predicted with certainty from the laws of physics. As soon as a system contains social components, i.e. people, predictions would include a prediction of human performance and this is notoriously hard and controversial: As Popper famously made clear, the course of human history can perhaps be explained but not be predicted. The current goal of meeting the needs of the future enterprise by introducing service orientation and cloud computing depends on a prediction of the performance of future enterprises that is debatable: The future may develop this way only if people decide to follow this vision, and many unforeseen factors may then intervene for this scenario to fail.
- *Meeting predicted demand.* This differs from the circumvention of predicted performance limits in that in this case a practical problem solver predicts that there will be a demand for an artifact the he or she will develop. The artifact may generate new goals in the economy but it does not solve future performance limits of current artifacts because there is no current artifact. Predicting demands in this way is an entrepreneurial competence. Examples are the introduction of laptops and of mobile phone technology and of ambient technology on the market.

This list is not claimed to be exhaustive but we can nevertheless draw some interesting lessons from it. Circumventing predicted performance limits and meeting predicted demand are *radical goals* [32], and may lead to radical innovations. Typically, these involve entrepreneurs with a vision and the stamina to achieve it. Famous early examples are Edison and Marconi, and famous recent examples are Steve Jobs and Bill Gates. Radical problem solving is high-risk, high-reward.

A more normal problem solving goal is to achieve some economic goal with an incremental improvement of current technology, or to repair failures, to improve performance, to flow down system goals, or to catch up with large systems improvement. Any of these efforts may lead to radically new technology, but normally they lead to incremental improvements of existing technology.

Interesting in all cases is that goals come from outside practical problem solving. The practical problem solver must understand and deal with goals of other stakeholders, who are interested in a solution artifact only in as far it helps them achieve their goals. This points at the need, also in normal problem solving, to manage the relationships between stakeholders and practical problem solvers in figure 2, a role called *engineering manager* by Wise [17, page 245]. These must be people who understand market needs and can translate these in solution artifacts.

These observations puts Gray's call for IS researchers to lead the market with new technology into perspective [36, pages 337–338]. In our framework, managing the relations at the three activity interfaces are different activities and historical evidence indicates that they require different competencies. At the very least it would be far fetched to require of every IS researcher to produce radical innovations; incremental improvements are the normal mode in technical sciences, and should be so in IS design science as well. I return to this in the discussion at the end of the paper.

4.2 Examples of Design Research Questions

Turning to the research activity in design science, we can build on a list of kinds of research questions identified by Vincenti [29]. In figure 4 I classify them according to their place in the engineering cycle [8]. It turns out that these questions are easily recognizable in IS research too.

- *Why does it fail?* Failures are gold mines of information to improve technology [37]. IS research too investigates failures of projects to reach their targets, failures of implementations to achieve their goals, and failures of methods to deliver their promises.
- *Why does it work?* This is a common question in technical research, as many artifacts exist that do work but of which the underlying mechanisms are not well-understood. Examples mentioned earlier are the study of heat machines in the 19th century and of aircraft technology in the 20th. This and the previous question why an artifact fails always are combined with a third one, which is what actually happens when the artifact is used. This is an interesting analog to evaluation and policy research, which investigates the actual outcomes of an intervention in social systems and why these outcomes are produced [38]. In figure 4, these questions are lumped together in the category *problem investigation questions,* in which the goal is to find our how well stakeholder goals are achieved with artifacts they currently use.
- *What will be the problem-solving power of this proposed artifact?* Practical problem solving delivers a design of an artifact claimed to solve a practical

Design science research questions
- Problem investigation questions
  - Why does it fail?
  - Why does it work?
- Artifact validation questions
  - What will be the problem-solving power of this artifact?
- Conceptual questions
  - How to measure this?
  - How to compute this variable?

Fig. 4. Research questions identified from the literature

problem. The research question to be answered is then whether it will indeed solve the problem. One way is to try it and see what happens. A more rational way followed in design science is to predict what will happen and check whether this would help stakeholders. Earlier I decomposed this into two questions, namely a prediction (what effects will this artifact produce in this context?) and a valuation (what is the value of these effects for these stakeholders?) [28]. Another important question to answer is external validation, or in the words of Vincenti [29], assessing the certainty of these predictions for future artifacts once implemented. In figure 4, all these questions are called *validation questions,* in which the goal is to predict the properties of an artifact in a practical problem situation before it has been implemented in that situation.

– *How to measure this?* This question is as well-known in technical science as it is in social science. How to measure effort, speed, usability, maintainability, security, risk, or any of the other attributes relevant in IS research? The special constraint in design science is that measurement must be cost-effective not only for the researchers, but to be usable in practice it must also be cost-effective for practitioners. For example, it is of not much practical use to acquire knowledge about risk indicators that cannot be measured in practice.
– *How to compute this variable?* Answering research questions in the service of practical problem solving places the design science research under the constraints that solutions can actually be computed, not just mathematically proven to exist. This may involve trading mathematically rigorous methods that are not computable, or that are too expensive to use, for approximate methods that are computable and also cost-effective to use. The historian of technology Edwin Layton sees this as the characteristic feature that distinguishes an engineering science from physics [39, page 575]. An example in IS design science could be the development of practical techniques to estimate implementation cost of ERP systems, where these techniques may be less exact but more cost-effective than some other more accurate technique; combined with an empirical research to validate these techniques. Measurement and computation questions have been classified in figure 4 as

conceptual questions, in which the goal is to define constructs, indicators, and computations that are valid with respect to a class of phenomena.

This list of research questions is not exhaustive but it does indicate that sources of relevance of design science research include (1) the ability to contribute knowledge about practical problems, in particular about causes of failure and success, (2) the ability to predict the outcome of implementing an artifact in a context and (3) the satisfaction by this knowledge of the constraints of practical observability and computability.

This indicates a constraint on design knowledge that is not applicable to other kinds of scientific knowledge: applicability. Nineteenth century engineers contemplating to use the results of science found the results too abstract to be useful and have pointed out the fact that practical problem solvers cannot ignore conditions of practice [40, pages 692–693] [41, page 331]. In an ECOOP 2009 dinner speech, Bill Cook phrased this eloquently when he warned academics not to try transfer a solution to an abstraction of a real-world problem: Practitioners have to deal with the whole problem [42]. *Conditions of practice* are all natural and social factors present in a practical problem, including all natural causes and stakeholder-defined performance criteria that cannot be abstracted away from. An example of the difference between abstractions in basic science and the conditions of practice is given by Küppers in an analysis of the differences between thermodynamics and combustion technology [43]: In the natural science of thermodynamics, the goal of understanding a flame in a furnace is achieved when the shape of the flame, the flow pattern and the course of the reaction or the radiation pattern of the flames is understood. The combustion technologist needs to answer the same questions, but additionally needs to know whether the flame is stable (burns in the same place), when it does not oscillate, if the furnace will be damaged by turning the flame on or off, and whether a certain domain of regularity prescribed by safety requirements can be reached and maintained. The additional variables that interest the engineering researcher have their source in the fact that in real practical problems, variables cannot be abstracted away, and are relevant for stakeholder goals.

Conditions of practice are present in IS design science research too: An ERP implementation is subject to a large number of variables that cannot be wished away [44], process improvement is impacted by a large number of risk factors [45], etc. This has an important implication for research strategy, which is that design science researchers cannot stop when they have have understood a phenomenon in the laboratory, but must eventually *scale up* to investigate what happens under the conditions of practice of the intended practical problem situation. In the next section we discuss the implications for design science in more detail and discuss which part of Schön's dilemma has been touched by our analysis.

5 Discussion: Relevance and Applicability

Our analysis motivates a definition of *relevance* as suitability of an artifact or of knowledge to help achieving a goal, and *applicability* as sufficient incorporation of conditions of practice in a theory or in artifact behavior. Knowledge is applicable to a case if it can be related to the conditions of practice of this case. There is a one-way dependency, because non-applicability implies irrelevance. When a medical doctor investigates a patient, most of the results of medical science are applicable, because medicine is a practical science aiming a practical knowledge; but only some of it is relevant for the problem at hand. However, abstract knowledge that does not incorporate the conditions of practice of the problem at hand, is irrelevant for any practical goal. Now let us consider the implications of this distinction for design science.

Importantly, there is no particular implication for *research methods.* The fact that conditions of practice must eventually be included does not exclude a priori any scientific method from being used.

However, there is an implication for *research strategy:* Even if design science research starts investigating an artifact in the laboratory, it eventually needs to scale up to conditions of practice. For example the first prototype jet engines developed where small scale models that were investigated in the laboratory under controlled conditions, but these were subsequently scaled up to realistic sizes and eventually test models were used to propagate an airplane flown by a test pilot [32]. Scaling up is thus a requirement for design science. This means that some methods of real-world research such as case studies and action research become important towards the later stages of artifact development [46,47]. The only way to produce conditions of practice is to move to practice. I therefore agree with Benbasat & Zmud [2] that IS design research needs to produce context-rich knowledge. And I agree with Davenport & Markus [3] that evaluation and policy research [38] provide useful methods for doing so.

A third implication concerns *design theories.* If a significant number of conditions of practice must be incorporated, then design theories are likely not to be universal (nomothetic) but likely have a middle range generalization [48], an observation also made by Kuechler and Vaishnavi [45] but so far ignored by proposals for design theories [49]. The need for middle range diagnostic theories and treatment theories has also been observed in psychological practice [50].

A fourth implication is about *technology transfer.* If a theory does not relate to conditions of practice, then it will not be deemed relevant by practitioners. This can explain why, in a study of technology transfer at NASA, it turned out that managers are reluctant to use software technology that had been investigated empirically in the laboratory, and were more easily convinced by results from case study research [51]. My explanation is that managers can more easily relate case studies than laboratory research to their own conditions of practice. Technology transfer is risk taking, and managers need specific information to be able to estimate this risk.

Application of technology in a practical problem always implies applying knowledge about this technology, and calling this technology "transfer" is misleading, because a lot more is involved than simple transfer. As pointed out by Gardner [52, pages 9–12], application of design knowledge involves combining different conceptual frameworks, dealing with missing data and ill-defined variables, figuring out the interaction of conditions mentioned in different theories, and in general building a mini-theory of the case at hand. This is also pointed out by Van Strien [50]. Application of knowledge to practical problems is an underestimated problem.

Fifth, our framework (figure 2) indicates different *roles* to play in design science. Design science *researchers* operate on the interface of practical problem solving and artifact investigation. Research *managers,* mentioned earlier, operate on the interface of design science and the economy, matching economic realities with design science possibilities. *Entrepreneurs* also operate on this interface but take higher risks by speculating on future demand. It would be a tall order to ask of every design science project to be entrepreneurial in this sense, as some commentators seem to suggest in a panel discussion at ICIS 2002 [36, pages 337–338]. Studying existing artifacts to understand how and why they work and fail is a normal mode for design science that delivers applicable and potentially relevant results.

Finally, let us return to the dilemma of rigor versus relevance as posed by Schön. Our analysis has shown that design science research does not have to lead to a particular practical problem solving project, but should deliver applicable knowledge that may not be relevant to any current goal but is potentially relevant for some future and possibly unknown goal. Applicability must be achieved by incorporating conditions of practice and satisfying the constraints of practical measurability and computability mentioned earlier. Within these constraints, methods used in technical design science, such as pilot studies and test flights correspond closely to well-known methods in social science such as case studies and action research.

This deals with some of the issues of uncertainty and instability in practical situations mentioned by Schön. However, part of the source of instability of the subject of social science is the *historicity* of the subject: Human subjects may join the researcher in interpreting social phenomena, and eventually will learn about social theories, may internalize them and then change their behavior [53, pages 29 ff.]. This affects the applicability of IS design science theories, and hence their relevance in a given context. But this phenomenon, complex as it is, should not blind us for the fact that a large part of the problem of applicability of IS design theories is shared with technical design science, and can be answered in the same way.

Our research group has been using the framework presented in this paper for several years to structure PhD theses [54] and papers [55]. Our experience is that it helps to find the right questions to ask if we get stuck, and to improve our understanding of the problems we are aiming to solve. This experience should be followed up by a more objective evaluation and we plan to do so once enough experience has been collected. This will be an action research reflection in the sense that we have used our own artifact (the framework) to improve our own practice and will then reflect on the utility of this artifact to actually improve the practice.

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