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Constructive Synergy in Design Science Research: A Comparative Analysis of Design Science Research and the Constructive Research Approach

ABSTRACT:

Information systems research is focused on creating knowledge which can be applied in organizations. Design science research, which specifically aims at applying existing knowledge to solve interesting and relevant business problems, has been steadily gaining support in information systems research. However, design science research is not the only design-oriented research framework available. Accordingly, this raises the question of whether there is something to learn between the different approaches. This paper contributes to answering this question by comparing design science research with the constructive research approach. The conclusion is that the two approaches are similar and compatible, save for details in practical requirements and partly underlying philosophical assumptions. The main finding that arises from the comparison is, however, that there is a potential problem in claiming knowledge contribution from evaluation of the utility of an artifact. That is, utility-based evaluation often builds the argument on adoption of the artifact, assuming that adoption and utility in general validates also claims

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to knowledge contribution. We show that this mode of evaluation has philosophical and practical problems that need addressing in further research.

Keywords: *design science research, design research, design-oriented research; the constructive research approach; epistemology; pragmatism; utility-based evaluation; evaluation of artifacts*

INTRODUCTION

Scholars in management science and information systems remind us every once in a while that rigorous research is a worthwhile effort, but that it should be able to deliver results which are applicable and relevant to practice as well (Starkey & Madan, 2001; Hodgkinson et al., 2001; Benbasat & Zmud, 1999; Holmström et al., 2009; van Aken, 2004; 2005). To reach a compromise and ground the abstract theoretical research to everyday activities and problems, it is generally conceded that information systems research (ISR) should generate new theoretical insights about the world and use them to solve relevant (business) problems (e.g. Benbasat and Zmud, 1999; Sein et al. 2011).

Within the last ten years the ISR field has seen a proliferation of Design Science Research (DSR), that following Simon's (1996) original concept (first published in 1969), aims to solve significant practical problems through purposeful synthesizing or construction of IT and other artifacts based on existing scientific knowledge. A key publication that raised the profile of DSR was Hevner et al. (2004), which built on earlier contributions from Walls et al. (1992), March and Smith (1995) and Markus et al. (2002). DSR has been viewed as one, or even the most important, means to enhance the fulfillment of the dual mission of ISR, namely rigor and relevance (Carlsson, 2007; livari, 2007). Since then DSR has received wide attention within ISR, while also gaining ground in the management field, especially through the efforts of van Aken (2004; 2005; 2007).

However, the demand to combine contributing to the body of knowledge while solving practical problems was recognized already before emergence of a coherent DSR framework in the field of social science in the mid 20th century through the development of the action research approach (e.g. Susman and Evered, 1978) and later through the proliferation of other "interventionist" research approaches (Jönsson and Lukka, 2007), such as the Constructive Research Approach (CRA) in 1990s (e.g. Kasanen et al., 1993). It has been argued that there is a significant overlap between DSR and action research (Järvinen, 2007), up to a point where an action design methodology is proposed (Sein et al., 2011) albeit there are also some key differences with underlying assumptions (livari and Venable, 2009). Continuing on the vein of bridging understanding between the different approaches within the multidimensional ISR field, we undertake an analysis of the CRA, although our attention goes past of arguing whether or not CRA and DSR are similar or compatible, as we concentrate on what there is to learn CRA.

The goal shared by DSR and CRA is using applicable theories, technical norms (e.g. Niiniluoto, 1993), or theories-in-use (Gregor, 2006), with high industrial relevance to design practical solutions. Thus, it seems that there are potentially two similar methodologies or research approaches applicable in similar problem situations. The first challenge is that different approaches and assumptions may inhibit understanding and comparison of results between domains; as Niehaves (2007a) points out, working on the same subject does not, alas, mean mutual understanding between the collaborators. Additionally, while the DSR framework has matured considerably over the last ten years, we are interested in examining what do the other design-oriented research traditions have to contribute to DSR. To this end we will analyze the CRA side by side with DSR to gain insight about compatibility between the two traditions and contributions towards design-oriented research in the future. CRA may have something to teach DSR, at least from the critical realist perspective outlined by Carlsson (2007) who has called for a wider approach than the artifact-centered focus. The rationale for choosing CRA in particular is motivated by the significance of CRA in Scandinavian context, and the parallels between the processes. Our guiding questions are: “What can CRA contribute to our understanding and practice of design-oriented research?” and secondarily, to answer the first question we address the question “Are the traditions compatible; what are the similarities and differences between DSR and CRA?”

To answer the research questions, we analyze the literature on CRA as well as DSR critically to uncover the common features and position the methodologies with respect to each other, and to discuss whether the approaches have lessons to teach each other. Methodologically we follow the approach assumed by other in the ISR field (Järvinen, 2007; Iivari and Venable, 2009), that is an analysis, reading or interpretation, of the published research guidelines, followed by a structured comparison of the research missions/application areas; the methodological frameworks, including guidelines and processes; as well as underlying philosophical issues including epistemology, mode of reasoning and justification of knowledge claims. In general we expect to contribute to the theory and practice of design-oriented research by improving the transparency and comparability of published results and by raising awareness of issues that need further study.

In particular, our analysis underlines a problem previously surfaced by Iivari (2007) who pointed out that design artifacts are often loosely coupled with the underlying theory they are supposedly built on. We will argue further, based on our analysis, that this loose coupling creates a rarely recognized challenge for DSR, as design-oriented research often measures success based on acceptance of artifacts, and the loose coupling may limit the theoretical contribution of DSR significantly.

The rest of this paper is organized as follows: The second section will present an overview of CRA and DSR frameworks, essentially defining our interpretation of the two units of analysis. The section third presents a comparison between them. The fourth section expands the discussion

on the background assumptions of design-oriented research as far as it applies to CRA and DSR and discusses the implications of our findings for the practice of design-oriented research. The fifth section concludes the paper and, and points out directions for further research.

OVERVIEW OF THE DESIGN-ORIENTED RESEARCH FRAMEWORKS

Summary of the Constructive Research Approach

The constructive research approach (CRA) has been a dominant design-oriented framework in Finnish and to some extent Scandinavian management literature (Jönsson and Lukka, 2007), while also receiving some attention within the international information systems community (Gregor, 2006; Gregor and Jones, 2007). CRA was introduced, according to the authors, in 1986 (Kasanen et al., 1993). The international diffusion of CRA started with the English language follow-up in the *Journal of Management Accounting Research* (Kasanen et al., 1993). In brief, CRA aims to increase the relevance of management science research through putting theory to use by constructing or designing “constructions” (Kasanen et al., 1993; Lukka, 2003; 2006). The original field of application for CRA was management accounting (Kasanen et al., 1993), where CRA was developed to facilitate design and implementation of management accounting systems such as activity based costing in organizations. Since its inception, CRA has established itself in various fields under the umbrella of industrial management and (management) information systems; for example, in logistics (Lukka, 2003), real estate management (Lindholm, 2008) as well as decision support systems and technology management (Elfvengren, 2008).

The methodological discussion around the constructive research approach (CRA) has been mainly spearheaded by the authors of the original paper “Konstruktivinen tutkimusote liiketaloustieteessä” (Kasanen et al. 1991) or “The Constructive Approach in Business Economics/Administration”, published in the Finnish journal *Liiketaloudellinen aikakauskirja* (The Finnish Journal of Industrial Economics or alternatively Business Administration) and the follow up “The Constructive Approach in Management Accounting Research” (Kasanen et al. 1993). We base the bulk of our analysis, unless otherwise noted, on two of the most recent publications that we are aware of: Lukka (2006) in a Finnish textbook on philosophy in applied social sciences, and Lukka (2003), which is marked as a reprint of an article in the Finnish methodology repository *Metodix* (Lukka 2001). Translations from Finnish references, most importantly Lukka (2006) and Kasanen et al. (1991), have been done by the authors.

In the most recent account, Lukka describes CRA as “a methodology that creates innovative constructions to solve real world problems and thus contributes to the field of study where it is applied” (Lukka 2006 p. 112). It should be carefully noted that the Finnish word “konstruktio”,

literally “construction”, can be both a verb and a noun, and within CRA means either to deliberately design an artifact or a deliberately designed artifact including all human-made artifacts, such as models, charts, plans and strategies, organizational structures, commercial products and information systems (Ibid.), as opposed to emergent socially constructed phenomena and artifacts. To be clear, we will henceforth refer to “constructions” with the word artifact (as used in DSR) to avoid confusion.

Besides the basic definitions, the literature has also set basic guidelines on how the research mission is to be fulfilled, or conditions that a research project has to satisfy in order to be classified within the CRA (Lukka 2000; 2003; 2006):

1. It focuses on real-life problems, which need solving.
2. It produces an innovative artifact, intended to solve the original real-life problem.
3. It includes an attempt to implement the artifact in order to test its applicability.
4. It includes intimate teamwork between the researcher and practitioners where the aim is to learn through experience.
5. It is carefully linked to existing theoretical knowledge.
6. It pays special attention to creating a theoretical contribution.

CRA also has a clear-cut process which is elaborated by Lukka (2006; 2003). Figure 1 condenses the main activities in the process. The CRA process starts by finding or choosing a problem, which is scientifically relevant and interests or troubles practitioners. The second phase is to organize a project around the problem and to ensure the commitment of the stakeholders. The advice in the literature is to include key personnel from the target organization in the project team and to make a formal agreement concerning the project, organization, funding and goals. The third phase then consists of analysis of the problem and review of relevant literature to gain holistic and thorough understanding of the problem space and the target organization as well as relevant literature. This phase may contain descriptive studies of the problem and the target organization, to describe and understand the problem, as well as study of previous literature related to the issues.

The fourth phase, the construction of the artifact, is described as an innovative and rather unstructured activity where little generic advice can be given (Lukka, 2003, p. 87). The researcher comes up with the construction serendipitously through examination of the problem and relevant literature. Kekäle (2001, p. 557) interprets that the researcher proposes a solution to the researched problem based on pre-understanding built in the previous phases of the process, practical experience or theory. Although Lukka (2003, p. 87) states that explicit methodological advice is hard to give for the phase of construction, Kasanen et al. (Kasanen et al., 1991, p. 320; Kasanen et al. 1993, p. 258) propose some guidelines for its construction: 1) proceeding step-by-step following the chosen methodological and theoretical framework, 2) auditability and documentation of each consecutive step in the process of construction, and 3) the goal and criteria to be filled

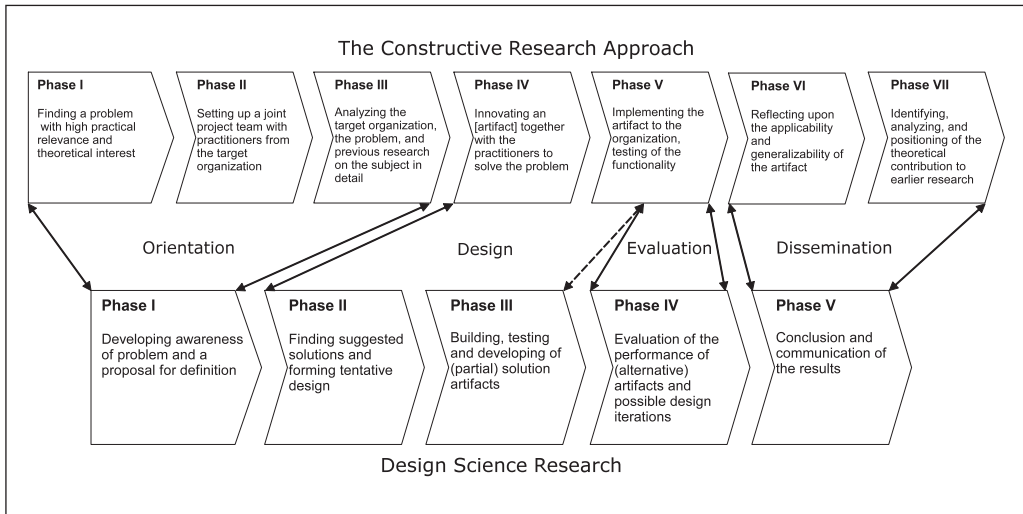


Figure 1: The process outlines and main activities in the CRA and DSR (adapted and rephrased from Lukka, 2003; 2006 [CRA]; Vaishnavi and Kuechler, 2004 [DSR])

through the construction. For further clarification Kasanen et al. (1991 p. 320) write that results in applied disciplines should be “relevant, simple and easy to use” as additional quality criteria for CRA artifacts.

After the construction, or synthesis, of the artifact, the process continues to implementation and in fact evaluation of the artifact in the fifth phase. Lukka (2006, p. 119) posits that the main and in fact only necessary test to validate or evaluate the artifact is a “holistic market test”, which serves to evaluate the artifact as a whole. The holistic test should reveal whether the artifact can be made to work in real business organization or not (Jönsson and Lukka, 2007, p. 385).

Kasanen et al. (1993) discuss the spectrum of validation through a (“holistic”) market test from weak, through semi-strong, to strong market test and explain that the idea is based on innovation diffusion theory. The innovation diffusion analogy is not explored to great lengths, but we interpret that the authors argue that validity is in correlation with the diffusion of the artifact, and the more managers that adopt the artifact, the stronger evidence we have regarding its validity. In short, the types of test which can be used to validate artifacts in CRA are described as follows (Kasanen et al 1993, p. 253):

- *Weak market test:* Has any manager responsible for the financial results of his/her business unit been willing to apply the [artifact] in question for his/her actual decision making?
- *Semi-strong market test:* Has the [artifact] become widely adopted by enterprises?
- *Strong market test:* Have the business units applying the [artifact] systematically produced better financial results than those which are not using it?

The CRA literature discusses generalizability in reference to the sixth phase of the process, where the researchers are advised to “ponder [about] the scope of applicability” (Lukka, 2003, p. 88) of the artifact. The authors (Kasanen et al., 1993; Lukka, 2003; 2006) claim that a useful artifact in itself is a manifestation of a scientific law and almost necessarily will work in similar contexts. The argument is that when there is an artifact which is successfully implemented as a working instantiation, it is likely that this solution applies to other enterprises of the same type (Kasanen et al., 1993 p. 260), and that it is natural to reflect upon the universal properties concerning the means-ends relationships it reveals (Kasanen et al 1991; Lukka 2006). Lukka and Kasanen (1995, p.85) have explicitly addressed “constructive generalizability” in problem-based case studies as based on a pragmatist epistemology, according to which a proper analysis of the problem and linking the solution to previous literature forms the basis for claiming that “a solution that has worked in a particular case can work in similar situations in other companies as well”.

In addition to applicability or generalizability, the seventh and last phase of the process is the identification of a theoretical contribution. Jönsson and Lukka (2007, p. 384) explain that generally the researcher compares the *ex ante* proposition that motivates the artifact with an *ex post* analysis of the intervention or instantiation, and identifies the causalities that lead to the observable outcome (in terms of utility or acceptance) of the instantiation. This comparison gives rise to the theoretical contribution. Lukka (2003, p. 89) identifies two main types of theoretical contribution. In the first case “if the designed new [artifact] is found to work in the primary case, it will provide a natural contribution to prior literature”; that is to say, the means-ends relationship exhibited in the artifact should be considered a contribution in their own right. The proposition seems to be that by virtue of the market test, the artifact or the proposition behind the artifact should be promoted to a technical norm (Niiniluoto, 1993; von Wright, 1963) or theory-in-use (Gregor, 2006). In the second case, the artifact along with the analysis of the instantiation should give significant insight to be added to the existing theory (Lukka 2006, p. 119). The sources of this contribution stem from the “positive relationships behind the [artifact].” This is to say that observation of these positive relationships or inferences enables the researcher to develop a new theory, or to refine an existing theory behind or embodied in the artifact. Although Lukka here offers the means-ends relationship embodied in the artifact for further more ‘traditional’ *ex post* analysis, Lukka (2006, p. 118) also states explicitly that after the first market test, further implementation and analysis should not be considered to be a task for the initial researcher, but for larger academic community and practitioners.

Summary of Design Science Research

Design science, as a problem-solving paradigm for ISR, seeks to create innovations that define the ideas, practices, technical capabilities, and products through which the analysis, design,

implementation, management, and use of information systems can be effectively and efficiently accomplished (Hevner et al., 2004). Gil and Hevner (2011) go further and propose that the mission of DSR is to produce artifacts that are useful and sustainable in an organization. As such, in information systems a DSR contribution requires identifying a relevant organizational (information technology [IT]) problem, developing an (IT) artifact that addresses this problem, rigorously evaluating the artifact, articulating the contribution to the (IT) knowledge-base and to practice, and explaining the implications for (IT) management and practice (March and Storey, 2008).

To condense the position presented in the core DSR literature, Hevner et al. (2004) address the difference between routine design and DSR by defining design as application of knowledge to solve a previously examined problem, while DSR contributes to existing knowledge by seeking solutions to (previously unsolved) non-trivial problems in novel and innovative ways. To be more specific, design science as an activity can be characterized as formulating design theories (Walls et al., 1992; Venable, 2006; Gregor and Jones, 2007), i.e. valid prescriptions on how to develop classes of artifacts, including constructs, models, methods, or instantiations (March and Smith, 1995), to fill a certain problem space (Markus et al., 2002). It is also prudent to note that a given design can only cover a limited problem space and the prescriptions are only valid for certain kinds of meta-requirements in that problem space.

In terms of practical guidance, Hevner et al. (2004) describe a basic framework by explaining that IS research in general and DSR in particular, should be linked to both the surrounding (business) environment and the knowledge base built by previous research. They suggest that DSR builds and evaluates artifacts, and theories (Vaishnavi and Kuechler, 2004), using applicable knowledge from the knowledge base and business needs from the environment as input for design. Hevner (2007) later proposed that DSR rests on three related cycles of activities that aim to solve the research problem. Firstly, there is the “relevance cycle” which interfaces with the environment to gather the (functional) requirements and constraints for the artifact. Secondly, the “rigor cycle” accesses the knowledge base for theories, justificatory knowledge (Gregor and Jones, 2007) and practical knowledge for the kernel of the artifact. And thirdly, the central “design cycle” builds and evaluates plausible artifacts to fulfill the requirements. Ideally, through these three cycles, DSR will produce artifacts which solve business problems. In the process, new knowledge and insights are created through design which can be then added to the knowledge base as a feedback to the rigor cycle, and the resulting artifacts can be implemented in the environment through the relevance cycle (Hevner, 2007). Besides the general framework, Hevner et al. (2004) present guidelines similar to Lukka (above), arguing that DSR should:

1. produce a viable artifact (construct, model, method or instantiation);
2. develop (technology-based) solutions for important and relevant business problems;
3. demonstrate utility, quality and efficacy of the design rigorously;

4. provide a contribution (a) in the form of an artifact and/or instantiation and (b) to the foundations (knowledge base) of the design;
5. apply a rigorous methodology to construction and evaluation of the artifact;
6. search for available means to attain the ends under the constraints of the problem environment; and
7. present the results to both technology and management-oriented audiences.

Vaishnavi and Kuechler (2004) were the first to introduce a concrete process description (Figure 1, on the bottom) to operationalize the DSR framework. Later, Peffers et al. (2008) have modeled a DSR process and methodological framework, which fits into Vaishnavi and Kuechler's (2004) description, even though the process outline is different. The first phase of the DSR process comprises finding a relevant problem and defining it. The second phase then concentrates on suggesting solutions to the problem defined in the proposal, where the knowledge base is accessed to find feasible solutions. The third phase is effectively the design phase. Here the researchers use the suggested solutions to develop or construct the artifact. After design and/or demonstration, the artifact moves into evaluation. The purpose of evaluation is to test how well the artifact contributes to the solution of the problem, through any reasonable empirical methodology as well as logical proof that the artifact solves the problem. Gil and Hevner (2011) present a more sophisticated understanding of utility of an artifact, which decomposes utility of an artifact to its usefulness as a solution to the problem and its fitness, i.e. ability to keep its usefulness over time, or in other words sustainability. For Hevner et al. (2004) such evaluation can follow established practices in IS research, including:

1. Observational (study of instantiations)
2. Analytical (structural and performance analysis)
3. Experimental (controlled or simulation experiments)
4. Testing (functional or structural)
5. Descriptive (plausibility of the systems in use cases)

Vaishnavi and Kuechler (2004) and Peffers et al. (2008) argue that the process is not linear since evaluation may produce new insights for design and may lead to changes which call for new evaluations. Moreover, the design and evaluation may reveal an altogether different problem to be solved, which results in a completely new design cycle. For example, Markus et al. (2002), whose paper was outlined as a prime example in DSR by Hevner et al. (2004), developed a rapid cyclical development procedure, an agile method in fact by today's terms, which resulted in incremental iterative development and instant evaluation of the revisions. After the artifact is stable and satisfying, the process moves to the conclusion phase where the results are communicated.

COMPARISON BETWEEN CRA AND DSR

Comparison of the research missions and methodologies

The previous section already reveals significant similarities between the CRA and DSR processes. In the following discussion, we will summarize the main similarities and differences. Starting from the definitions, we can note that the difference is that DSR literature seems to put more weight in applying previous knowledge through a specific kernel theory in the design, where CRA proposes a more soft or creative approach. This does not mean that CRA artifacts cannot be based on (a kernel) theory as discussed, but that it is not strictly required.

Based on the cited sources, it would seem that the objective of DSR is similar to that of CRA, but DSR approaches the task of problem solving with a different trajectory. The basic position in DSR seems to be more oriented toward design driven by, on one hand, use of existing theory to solve problems and, on the other, validation or development of new theories based on the experiences. Especially the earlier authors clearly prescribe the explicit use of a kernel theory as the core of the design (e.g. Walls et al. 1992; Markus et al. 2002), while CRA literature does not explicitly demand that the design would be based on a specific theory.

Nevertheless, CRA does not reject the use of a kernel theory as for example Kekäle (2001) discusses the sources of solutions in CRA and proposes that the artifact may be based, among other things, on a theory. An theory driven example of CRA is presented by Kasanen et al. (1993, p. 247) where one of the authors developed a model for capital budgeting using the value maximizing paradigm and a specific budgeting framework. The artifact one of the authors developed to solve a budgeting problem was based on a rather clear kernel theory; it was tested with simulations and then implemented as an instantiation to verify the utility.

To underline the difference, we can make a simplification of the general definition of DSR (e.g. Cross 1993; Hevner et al., 2004; Vaishnavi and Kuechler, 2004) and dress the difference in methodological terms: the basic logic of discovery in earlier DSR literature seems to be deductive. Stereotypically one takes a previously unsolved problem and tries to find justificatory knowledge (Gregor and Jones, 2007) or a kernel theory (Walls et al. 1992) which can help solve the problem. In effect, the researcher takes a general causal inference (a theory) and deduces a solution to a particular problem from the general abstract inference. The kernel theory (justificatory knowledge), thus, offers general principles that can be applied to the specific problem and, in doing so, ends up contributing to such a theory either by providing further exemplary proofs of concept, or by extending the problem domain and thus the generalizability of the design principles. However, to make matters more complex, Vaishnavi and Kuecher (2004) propose that the logic of discovery

in DSR is actually abductive¹ when while developing the first tentative design or solutions and only becomes deductive when building and evaluating the final artifact(s). Fischer and Gregor (2011) go on to explain the difference between *context of discovery* and *context of justification*; in the former reasoning can be abductive, but in the context of justification of scientific claims reasoning moves to deduction and induction. Thus, depending on how the design problem is approached, and the phase of the research process, DSR can move between abduction, deduction and induction (Fischer and Gregor, 2011).

Moving on to CRA, as described in the literature, the solution is based on deep understanding of the problem and existing theory and is found through a heuristic search process (Lukka 2003; 2006). Even though CRA literature does not use the term “abductive” explicitly, comparing the description of the problem solving phase in the CRA process and the description of abductive reasoning we can draw a parallel between the two. Nonetheless, Lukka (2003; 2006) proposes that the researcher should reflect upon the solution and seek general inferences revealed by the artifact’s implementation in the later stages of the process, which seems to fit the description of inductive logic, where the scientist observes a particular aspect of the world and inducts general inferences or explanations from the observations.

In sum, the theory development process in CRA and DSR is contingent on when and where the knowledge contribution is sought. The early aim of extending or contributing to a kernel theory through the design of a new artifact in a new problem domain is different than the aim of finding generalizable principles through the reflection on an already designed artifact. Indeed, both paths seem to be possible in CRA as well as DSR, but the critical task for the researcher is a transparent and explicit choice in this respect.

There is yet another analogy between CRA and DSR. The *definition and use of the word “construction” is quite similar to the “artifact” as it is defined in the DSR literature. When we compare March and Smith (1995) with Lukka (2006), the first paper articulated that products of DSR, the artifacts, include models, methods and constructs besides actual instantiations of IT. The second paper by Lukka includes things such as models, charts, plans and strategies, organizational structures, commercial products as well as IT artifacts/instantiations.*

1 The abductive logic of scientific discovery can be described as search in or synthesis of a set of seemingly unrelated facts, the problem and existing theory, with a pre-condition that they are connected and that the theory has an answer to the problems by the researcher (Kovacs and Spens, 2005). Based on this reflection, the researcher comes up with an empirical hypothesis, or an artifact which embodies the empirical hypothesis (Yu, 1994; Burch 2008). Johansson (2003) describes abduction by quoting Peirce as a reasoning following the logical rule:

if we observe unaccounted/unexplained/surprising fact C, and
if C follows from A,

hence, when we observe C, we can propose that condition A is in effect

In plain language, if we know that unsteady walking follows, among other possible things from intoxication, and we observe a person walking unsteadily, we can suspect the person has consumed a liberal amount of libations, but the observed unsteadiness might be just as well a symptom of an unrelated medical condition.

Going from the abstract to the practical, the process in CRA and DSR for the most important parts is quite similar. If we compare the processes presented in Figure 1, we can draw multiple parallels. In a side-by-side comparison, the process description of CRA puts more weight on the collaboration aspects, but the basic tasks are quite similar. Both processes go from developing problem awareness and definition to proposing solutions, developing artifacts and evaluating them. The difference is that DSR places evaluation in a separate phase whereas in CRA the market test is included in the implementation phase.

This is conceivably due to the DSR guideline prescribing that an artifact needs to be tested thoroughly before being “released” into practice (Hevner, 2007, Gil and Hevner, 2011), but also to the more broad guidelines for DSR evaluation (Hevner et al., 2004). Another difference is that the conditions a research project has to fulfill in order to be considered CRA are stricter. When we compare the basic guidelines between Lukka (2006) and Hevner et al. (2004), there are restrictions in the basic guidelines for CRA with respect to DSR. More specifically, CRA guidelines 3 and 4, which require an instantiation to validate the artifact and require that the artifact is developed in close collaboration with the target organization. DSR literature does not prescribe a definitive process or mode of collaboration. DSR literature is also less unanimous about requiring an instantiation, even though Hevner et al. (2004) prescribe that DSR should result in a viable artifact, which is rigorously evaluated, and Gregor and Jones (2007) propose that a complete design theory should include an expository instantiation.

Yet another comparison can be made in terms of the context of application. DSR has been mostly prominent in the IS field, but has a bridge head in management and engineering (e.g. Pef-

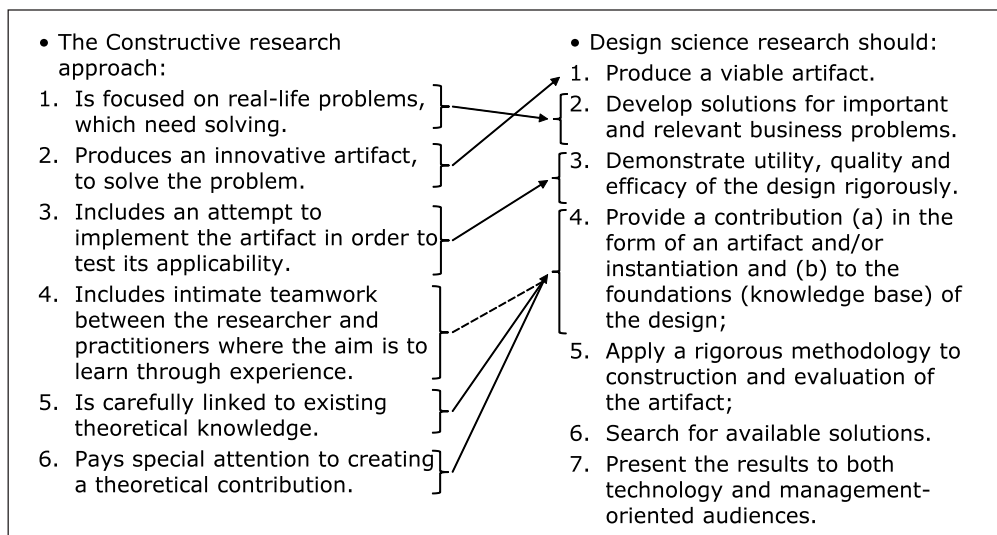


Figure 2: Comparison of methodological guidelines (Kasanen and Lukka 1991; 1993; Hevner et al. 2004; Hevner, 2007)

fers et al. 2008; van Aken, 2004). The most prominent examples of DSR seem to be IS-related projects such as systems development, e.g. Walls et al. (1992) and Markus et al. (2002). If we look at some of the recent examples of DSR, the topics include a system for text analysis in communication (Abbasi and Chen, 2008), a value-based pricing system for co-branded products (Chang, 2008), a virtual enterprise architecture for logistics service (Moeller et al., 2008), a new kind of travel advisory service (Novak and Schwabe, 2009), collaboration processes (Kolfshoten et al., 2009) and tracking in logistics/operations management (Holmström, et al, 2010). There are also examples that can be positioned toward management science e.g. ontology for business models (Osterwalder, 2004) and a knowledge representation for knowledge dissemination and reuse (Wu, 2009). The examples given in CRA literature include a capital budgeting model (Kasanen et al. 1993), a set of measures for quality of vocational education, performance measures for a customer focused industrial company, performance measures for a networked SME-firm, and a model for working capital control in an industrial company (Lukka and Tuomela, 1998). Additionally there has been at least a process for product concept recognition using group support systems, (Elfvengren 2008), a corporate real estate management framework (Lindholm, 2008). If we look at the projects from both disciplines, many projects (1) state a clear problem which is linked to real-world management, (2) solve the problem by developing a novel artifact and (3) may or may not use technology, such as information systems, as a part of the solution.

Overall, it seems that despite different backgrounds, there is more in common between the methodologies than what sets them apart. The main differences in the frameworks seem to be the slight difference in the logic and thus organization of the activities, and the evaluation, which is broader in DSR than CRA. To conclude the comparison, it seems to us that CRA projects could be regarded as DSR, with some reservations, as a project which fills the guidelines presented for CRA cannot be rejected as DSR. However, the other way round, we cannot conclude that at least all DSR satisfies the conditions for CRA in the form presented above. In this sense, if one will, CRA can be positioned as a subset of DSR but not the other way around. In writing this, we obviously have no pejorative intentions, but only the intent to facilitate scientific discussions and transparency of design-oriented research. However, the generalization of CRA as a subset of DSR leaves certain differences between the background assumptions unaccounted for, which is the subject of the next section.

Comparison of the background assumptions and epistemological notes

The earlier comparison between CRA and DSR has given the picture that, when all is said and done, the main difference between these design-oriented research approaches is not the process of research, or even the associated evaluation methods, but the background assumptions that act as the implicit premise of the claims to validity embedded in these research approaches. Ponder-

ing how CRA or DSR researchers perceive truth may seem like an underlying issue not directly related to the method or even less to everyday research, but these assumptions are implicitly used to justify validity claims and if the basic assumptions fail, the research claims have no truth value. Explicit philosophical and methodological choices not only determine the way that researchers approach their subject, but also allow the readers of the published results to evaluate them properly. This is why we want to take a moment to discuss the foundations of DSR and CRA.

Before delving deeper into the assumptions, it is worth noting that the examined bodies of literature, with their philosophical foundations, come from different traditions at least to some extent. CRA is framed within accounting and management (Kasanen et al., 1993) or more broadly in social sciences (Jönsson and Lukka, 2007), DSR comes mainly from engineering and information systems (Kuecheler and Vaishnavi, 2008; Cross, 1993; Simon, 1996; Bayazit, 2004) although they both overlap on management and accounting information systems (e.g. Kasanen et al. 1993; Elfvingren, 2008). The difference between the backgrounds might account for the different approach to use and development of theory as observed in the previous section. It is expected that this multidisciplinary background also creates different possible groundings for the respective methods. Nonetheless, those different backgrounds are precisely the source of onto-epistemological ambiguity and the same fields can be understood differently once those assumptions come to the surface.

Furthermore, if we look at ISR, the fact that it is a multidimensional field, which has given rise to different research approaches (Mingers 2001), based on different epistemological assumptions and typically divided into: positivist, interpretive and critical philosophical traditions (Klein and Myers, 1999). It was through new understandings of two of the main premises of information systems that ISR methods became amenable to more rigorous validation, generalization and publication. The first premise is understanding the field as either a socio-technical field or a social science in its own right, (e.g. Hirschheim, 1992). The second is understanding the nature of the object of study, as a contextualized and user-shaped IT artifact serving a social need, e.g. (Orlikowski, 2000; Walsham, 2005). These two pillars gave rise to different schools and journals specialized in a particular strand of IS research.

Despite the lively discussion on the philosophy of IS research, DSR literature is not very explicit on philosophy as pointed out by Niehaves (2007b). For example, Hevner et al. (2004) take the middle-of-the-road approach in writing "The goal of behavioral-science research is truth. The goal of design-science research is utility. As argued above, our position is that truth and utility are inseparable. Truth informs design and utility informs theory." This position is in fact very close to maxims of instrumentalist or pragmatist epistemology (cf. below), which has lead Kuecheler and Vaishnavi (2008) to conclude that pragmatism is the main epistemological current in DSR. Others go further and state that artifacts have no truth, only utility (March and Smith, 1995).

This, in fact turns the validation question around by arguing that validation is not done against a measure of truth, but rather against a measure of pragmatic value, through pragmatic success (Moody, 2003; Gil and Hevner, 2011).

Focusing on CRA, Lukka (2003, p. 85) states that CRA “is based on the belief, brought from pragmatist philosophy of science, that by a profound analysis of what works ... in practice, one can make a significant contribution to theory”. Kasanen et al. (1991, p. 322) cite Peirce’s pragmatism and conclude that “validity of construction in the field of business administration has to be approached by practical functionality”. More or less the same argument follows in Kasanen et al. (1993). In matters of truth or truthfulness, CRA refers to “the pragmatist notion of truth” (Lukka and Kasanen, 1995, p. 83) and specifically to William James (1955), although this discussion is not elaborated at length in the examined literature.

James himself (1995, p. 77) writes about Pragmatism’s Conception of Truth that “[t]he truth of an idea is not a stagnant property inherent in it. Truth happens to an idea. It becomes true, is made true by events. Its verity is in fact an event, a process: the process namely of its verifying itself, its verification. Its validity is the process of its validation.” He then follows by arguing that verification and validation “signify certain practical consequences of the verified and validated idea”. According to James (1995, p. 78) validation follows “the ordinary agreement formula”. In essence, we interpret James (1995, p. 79) that a logical claim is truthful if (1) acting upon it has the consequence which can be reasonably extrapolated from the logical claim, and (2) that the consequences prove to be useful. The argument is akin to the popular (pragmatist) maxim “what works is true”, i.e. perceived utility of the artifact as a solution to the problem specified as measured by acceptance validates the research.

CRA, on the other hand, explicitly adopts the pragmatist epistemology, but neither DSR nor CRA addresses the grounding to ontology, with the exception of a proposal to use the Popperian 3-world ontology as a basis for DSR (Iivari, 2007). To condense, what we can say based on our reading of the literature is that early work on DSR held more pragmatist or instrumentalist assumptions, similar to CRA literature, but recent discussion has made DSR open to alternative epistemologies (Iivari 2007; Niehaves 2007b).

Summary of the comparison

To set the stage for discussing the findings, Table 1 summarizes the main characteristics that surfaced from the previous comparison between CRA and DSR. While the literatures use different terms and express their intent in different term, it would still appear in the big picture that the approaches are comparable and compatible. Several findings show a common thread between the two design-oriented research approaches as a way to facilitate comparisons between published research, to provide entry points for sharing lessons learned and to make explicit the un-

Table 1. A side-by-side comparison of CRA and DSR

(Common) Characteristic	CRA	DSR
Research mission or goal	Problem solving in real business environment (Kasanen et al., 1993; Lukka, 2003; 2006).	Problem solving in real business environment (Hevner et al, 2004).
Artifact	<p>Konstruktio (Lukka, 2006): “all human made artefacts, such as models, charts, plans, action plans and strategies, organizational structures, commercial products, and information systems”.</p> <p>Examples: Corporate real estate management framework (Lindholm, 2008), product idea generation process/method (Elfvengren, 2008), performance measures for different industries/organizations (Lukka and Tuomela, 1998), an AHP model for choosing manufacturing strategies (Takala, 2000).</p>	<p>Artifact (March and Smith, 1995): constructs, models, methods or instantiations.</p> <p>Examples: pricing system (Chang, 2008), decision support system (Muntermann, 2009), data warehouse, software reuse measure, voice and video over IP software, IS planning method (Peffers et al, 2008).</p>
Research guidelines	<p>(Lukka 2000; 2003; 2006): Focus on real problems Produces innovative artifact that solves the problem Includes implementation “attempt” to test practical applicability Includes collaboration between researchers and practitioners Linked to existing theoretical knowledge Creates theoretical contribution.</p>	<p>(Hevner et al, 2004): Produces viable technology-based artifact to solve a problem Rigorously demonstrating utility, quality and efficacy of design Contribution in the form of the artifact and to the knowledge base Rigorous construction and evaluation methodology Means-ends problem space searching Present results to technology and management oriented audience.</p>
Research process	<p>(Lukka, 2003; 2006): Find relevant and theoretically interesting problem. Setup a joint project team Analyze problem and context Collaboratively construct artifact Implement and test artifact Reflect upon applicability and generalizability Identify, analyze and position theoretical contribution.</p> <p>Labro and Tuomela (2003), Lindholm (2008) follows the process adapted after Labro and Tuomela (op.cit.).</p>	<p>(Vaishnavi and Kuechler, 2004): Problem awareness and proposal. Find solutions and tentative design. Build and test artifact Evaluate and iterate Communicate results</p>
Theory in desing	<p>The prescribed process starts with problem identification and developing a solution, thus theory may emerge inductively in the process. Also “Soft” support for theory driven deductive design, i.e. using a kernel theory. Examples: Kasanen (1993) model for capital budgeting (not explicit which kernel theory was employed), Lindholm (2008) uses a previously developed (grounded) theory to build a system, and Kekale (2001) supports kernel theory in general.</p>	<p>Support for a kernel theory when emphasis is placed on developing a design theory (Markus, 2002; Walls, 1992, 2004; Gregor and Jones 2007; Hall et al, 2003).</p>
Logic of discovery	<p>Abductive, according to Lukka (2003; 2006). Possibly also inductive in confirming theoretical contributions</p>	<p>Deductive by looking at Cross, (1993); may also move from abductive in the first stages through deductive in deriving design propositions to inductive in evaluation (Vaishnavi and Kuechler, 2004; Fischer and Gregor, 2011).</p>
Domain of application	<p>Management and some IS (GSS), examples included in section 3.1.</p>	<p>IS mostly but some management, examples included in section 3.1.</p>
Underlying philosophy	<p>Pragmatism explicitly (e.g. Lindholm (2008).</p>	<p>Not explicit, could be pragmatism, interpretivism or positivism (Niehaves, 2007b).</p>
Evaluation	<p>Utility-based market test, used in evaluation of the artifact and research process (Lindholm, 2008); evaluation of an artifact (Takala, 2000); evaluation of the utility of the artifact (Hilmola, 2007).</p>	<p>Utility-based (March et al., 2008; Hevner et al, 2004), concerning usefulness and sustainability of the artifact (Gill and Hevner, 2011).</p>

derpinnings where there might be possibilities for convergence or for pointing out potential differences. The characteristic in the table follow the material presented in the previous sections in terms of common concepts, guidelines, research process, and emphasizes the role of theory, underlying philosophy and evaluation / validation. The table also sets the stage for the final discussion and conclusions pointing at further research and opportunities for improvement of transparency and evaluation criteria.

DISCUSSION

The side-by-side comparison indicates that DSR and CRA are compatible with each other in their main features. Regarding the mission of this paper, this finding allows us to draw lessons from the analysis of the one to the other. The question is then, what have we learned from our analysis that contributes to the discussion on DSR? Firstly, we found that the DSR literature is not unanimous on issues concerning the background epistemology. This can be seen as a weakness, but also as strength, because DSR allows some flexibility in terms of underlying philosophy (Niehaves, 2007b), so it can invite a wider audience. Potentially this openness improves transparency and allows wider dissemination of design-oriented research from different backgrounds, but this only holds under the requirement of being explicit about the assumptions behind the particular research approach.

Besides finding the approaches compatible, we can see the CRA as a ‘straw man’ in a sense: by analyzing it rather critically at times we have learned something new about design-oriented research in general. To recapitulate: CRA exhibits pragmatist research in quite a pure form in the sense that it directly seeks to solve a problem through design and draws conclusions about success and validity based on how well the design artifact was perceived to be useful in solving the problem. The same logic applies to DSR as well in many settings, especially when artifacts are evaluated solely based on their utility.

A critical examination of the philosophical assumptions of CRA raises an interesting question that is pertinent to DSR and design-oriented in general: can we explore validity through utility alone, especially using adoption of an artifact as a surrogate measure, and how does this reflect on our conceptions of truth? Generally speaking, analysis of the assumptions behind evaluation through a market test or some other utility measure reveals a problem, at least from a positivist standpoint. If one evaluates validity of a design artifact based on utility, or even further, truth based on utility, the setting is rich in ontological assumptions. Taking the market test as an example of a utility measure, the pragmatist assumption behind the market test is that the acceptance of the artifact among the users validates the truthfulness of the design or (i.e. kernel) utility theory that it embodies by correspondence (Venable, 2006; Walls et al. 1992). In plain terms, this means

that the implicit assumption is that the underlying (design) theory attached to the artifact is true and valid, or true enough, if the artifact works. Although utility and acceptance are relevant measures for the completion of the practical objectives of design, this doing away with 'truth' might render pragmatism inadequate precisely because truth (defined in pragmatism in terms of utility) is no longer the issue in this setting (Iivari, 2007).

Taking validation through acceptance of an artifact as a special case in utility-based evaluation, we have to consider the relationship between the artifact, its acceptance and validity of underlying justificatory knowledge. First we consider adoption as a measure for utility. Validation through adoption or other measures of utility is problematic, because acceptance is not an inherent aspect of the artifact. In other words, when thinking about acceptance as a proxy for utility and as a validation measure, we need to ask whether the adoption of the artifact really measure its qualities. If we do a simple thought experiment, we can think of artifacts that have been accepted many years after their construction or artifacts for which initial acceptance dwindles down. This change in acceptance, when not caused by a change in the artifact, suggests that acceptance is not intrinsic to the artifact. Looking at the discussion on acceptance of collaboration/group support systems, it is well documented that they are objectively useful in solving problems of collaboration and they have been exceptional good investments for many organizations, yet still they tend not to be sustainable because of incentives that do not support such facilities (e.g. Briggs, et al. 2003). There is also significant evidence which mounts against acceptance as a measure of validity from completely opposite cases where managers intentionally use suboptimal solutions or downright false information to justify their chosen views, decisions and/or to keep their position (e.g. Jackson, 1985; Olesen and Myers, 1999; Cecez-Kecmanovic, 2001).

Second, we consider whether acceptance of the artifact implies validation of the underlying theoretical constructs (especially in the case when there is a novel theoretical contribution). In fact, Iivari (2007) argues that the artifact is typically weakly linked to the underlying theories and thus the logical link from the artifact to the theory is equally weak; whereupon it follows that if we take the artifact as a "black box" and use only utility measures and do not know why exactly the artifact works and why it was accepted, we arrive in the situation that we have only weak additions to the knowledge base. After all, contributions to the knowledge base should include causal inferences, even if at a tentative level (Gregor, 2006). The point of our critique is that the adoption of an artifact by 'a real manager' does not necessarily imply validity of the artifact as a solution to the problem or even measure its utility, as discussed. If we examine the thing *an sich*, adoption might not in fact tell anything about the artifact and its suitability other than that the designer has convinced the manager to try it.

If we take the matter further, to consideration of usefulness as a measure of truthfulness in a more general setting, it opens up a further potential critique. If we think of science and theories

as justified true beliefs, one has to ask: can we truly justify a belief, causal inference or technical norm, purely on the grounds that it appears to be working in a given context, if we know nothing of its nature and the underlying mechanisms? Without an *a priori* understanding or theory about the causal inferences or means-end relations embodied in the artifact and validation of these inferences, the artifact or instantiation might end up giving the right results for all the wrong reasons, or wrong results for the right reasons for that matter. In the ISR field it is generally thought that an instantiation of an existing class of systems as such is not a contribution, but a new class of systems or a system based on a different theoretical basis can very well be (Markus et al., 2002; Hevner et al., 2004). An instantiation that solves a problem still counts as advancement in practice, but it cannot add to our knowledge if we do not know how it works and for what reason. In general, an artifact, or an instantiation, in itself does not necessarily represent a contribution, unless it demonstrates a novel approach to solving a problem, is based on a novel theory or validates or falsifies an existing one. As Hevner (2007, p. 91) put it. "... practical utility alone does not define good DSR."

This leads us to the question regarding the relationship between theory and the artifact. Lukka (2003) proposes that the theoretical contribution of design-oriented research is found when examining the instantiation *ex post*, while the DSR literature (Walls et al., 1992; Cross, 1993; Markus et al., 2002; Gregor and Jones; 2007) suggests that the artifact should be built on an existing justificatory knowledge. Within DSR Gil and Hevner (2011) argue that "The investigation of use artifacts, on the other hand, is largely the domain of behavioral research. Inasmuch as they have already been constructed, the principles incorporated in their design are likely to be of less interest than the principles determining how their use impacts the entities (e.g. organizations) in which they are embedded. Nevertheless, it is certainly possible—indeed probable—that important principles that may guide future design can be acquired by observing [artifacts] in use. This highlights the complementarity and need for communication between DSR and other research paradigms." This seems to say that DSR is interested in construction or synthesis of an artifact, rather than the impact or outcome of an instantiation. This question about the relationship of the artifact to its underpinnings has also some repercussions on evaluation/validation. Iivari (2007) raised the concern whether the design validation in fact validates claims to knowledge contribution if the theory is not sufficiently embodied in or operationalized by the artifact. The remaining question is then: does the artifact embody the theory, or is it a proxy for operationalizing and thus examining the theory, if indeed there is any specific theory at all? And if the artifact does not act as a proxy for testing theoretical conjectures, what new insights can DSR bring to the knowledge base?

In sum, our analysis underlines a seldom recognized feature in the (pragmatist) logic of measuring truthfulness or validity through utility and particularly acceptance. First, user acceptance as

a proxy for utility and truth respectively is in fact quite a weak and relatively easily biased indicator. Second, we recognize that the artifact itself is often conceptually quite weakly associated from the theoretical conjectures it is built on. These two findings lead us to the following methodological challenge: if the specific reasons for adoption and acceptance are not known, we cannot know whether the adoption is a consequence (in pragmatist terms) of our design and the underlying theoretical proposition that goes with it. Not to put too fine a point on it, we may have succeeded in getting a satisfied client, but at the same time we may have failed to gain new knowledge.

The implication of this latter finding for research is quite straightforward. Considering utility-based validation, a successful validation should take the logical form of a classical syllogism, where the main line of argument is that, based on the state-of-the-art knowledge, to solve problem X , we should take the course of action A , or a series of actions A_1 to A_n , where A is the major premise, based on a theoretical proposition of inference between condition A and outcome X . The minor premise to fill the syllogism form would be then that the instantiated action(-s) A_i instantiate or follow the theoretical proposition closely enough to enable us to say that indeed the course of action followed the prescription. With these conditions, if the original problem was solved and the users are satisfied, we can propose logically that the causal conjecture was valid. This allows us not to abandon utility-based validation, but strengthens the argument, and following this form allows more convincing claims for theoretical contributions as well. This line of reasoning can be built into a design theory, which helps to formalize the reasoning about the theoretical underpinning of an artifact, the propositions about its effect in the organizations, its expected mutability or evolution as an instantiation and the logic of evaluation (Gregor and Jones, 2007; Piirainen and Briggs, 2011). To us it is conceivable that using a DT as a pivot for design an evaluation can at least elucidate the relationship between the artifact, instantiation and theory, if not completely mend it. Further, Using a DT does not have to depend on the logic of discovery that was employed to construct it, or any particular epistemology; the framework of the DT can be employed while the content changes.

In fact this discussion leaves the DSR researcher between a rock and a hard place. On one hand, a researcher should design a practicable solution to an important problem, and on the other gain new knowledge in the process of synthesizing and evaluating the artifact. In practice though, one has enough trouble to create an instantiation that is adopted to the organization in the first place let alone be sustainable. Further, reliable data collection for evaluation especially for more social than technical artifacts is often cumbersome and may result in partial or otherwise low quality data, especially in longitudinal research which seeks to examine the long term impact of the artifact. These reasons make the market test a lucrative approach to evaluation of artifacts in an organizational setting.

The CRA literature draws our attention to the issues discussed by Carlsson (2007), who proposes that in order to serve practical interests better, DSR would benefit from considering the implementation and sustained use of the products of DSR. While we have presented some potential shortcomings of the market test, the idea that the product of DSR should be accepted and perceived useful is important. We would propose that rigor in evaluation or chasing general solutions to generic business problems should not be at the expense of producing applicable design knowledge and solutions to practical problems as well. One solution for improving the chance of acceptance of the design might be combining collaborative design with explicit theorizing. While this need has been already demonstrated by Markus et al. (2002) and later formalized by Sein et al. (2010) who have proposed combining DSR and action research, the CRA tradition is itself quite closely associated with AR (Jönsson and Lukka, 2007) in the sense that it sports features aimed at gaining acceptance in the organization. Thus, we might propose that in practical terms CRA guidelines about securing early involvement of the target organization may in fact be beneficial to DSR as well, while it easily leads to an action-type process.

The most apparent benefit from involving the target organization in a DSR process would be potentially increased buy-in of the artifact through increased commitment to the process and better understanding of its properties. Further, as collaborative development can be seen as a cure for many challenges of design, closer examination of these practices would be potentially beneficial to design-oriented research within and beside DSR. Besides the risk of bias (Iivari and Venable, 2009), there are potential benefits to be gained from deep involvement in the design; the stakeholders for example might divulge information more readily for a participant they view as an equal instead of an outside observer. Participation also gives access to richer data such as documents and participant observation, besides the usual interviews and questionnaires. Participation can also result in more rich descriptions of the research process and better understanding of the artifact and instantiation (Jönsson and Lukka, 2007). Accordingly, despite the risk, developing best practices for collaborative development has the potential to improve practical impact of DSR. However the need for commitment will probably depend on the problem, artifact and its intended use; the more users are required, and the more the processes, and individual workflows are affected and expected to change, the more important it is to involve stakeholders to the process. In purely technical, engineering type research, where the artifacts are transparent to the main user base, for example back-office systems, databases, algorithms, software components, broad-based commitment to the process is conceivably not as important.

An interesting development in DSR evaluation literature is presented by Hevner and Gil (2011), whose quote above can be interpreted to suggest that they would like draw the line between the DSR as an approach to synthesize new artifacts and behavioral science that studies their impacts in organizations. However, based on the discussion on utility-based evaluation, this demarcation has its hazards when it comes to contributing back to the knowledge base by DSR.

In fact CRA literature proposes one solution to this predicament, by proposing that the theoretical contribution of CRA arises from the study of the instantiations. This underlines the importance of communication between behavioral science and DSR, if not broadening DSR to include on-going or *ex post* behavioral science research component to DSR.

To close the discussion, we want to take our own medicine and be explicit about our own assumptions; we have conducted our analysis from a DSR perspective. Our philosophical background is in Popperian realism and interpretivism and positivism as understood in IS and management fields. These assumptions can be also outlined as the main limitations of our study, as much of the critique on utility-based evaluation stems from assumptions different from instrumentalist or pragmatist philosophy. That is to say, we have an underlying assumption in the analysis that research contributes to refining, refuting or developing theory while achieving other goals in the process. However, we propose that the utility-based validation issue stands regardless of individuals' philosophical assumptions: as there is no escape from the fact that the market test may be a pseudo-validation for knowledge claim stemming from DSR, if we do not in fact know whether an artifact works as it is intended, and why it works, or whether it only appears so.

CONCLUSIONS

Our research objective in this paper has been to analyze and compare the constructive research approach and the design science research framework. Our interest was to find out whether the two approaches have significant overlap besides the superficial similarity, and what this comparison can teach us about DSR.

The similarities between DSR and CRA are quite remarkable considering the different histories and roots, even to the point that CRA can be positioned as a subset or variant of DSR. On the surface, the greatest difference between DSR and CRA is that the former does not necessarily require an instantiation, but the default in CRA is that the validation is done through an instantiation. Within information systems, DSR is picking up pace and recognition, perhaps primarily due to the fact that it offers a suitable framework for research which is both rigorous and relevant. However, because it is still in the process of being adopted, several open issues include a clear underlying epistemology and evaluation/validation criteria. These are issues that are critical both from the point of view of the researcher(s) wishing to apply DSR and from the point of the view of the community that must evaluate or "accept" a DSR contribution (e.g. for publication). By conducting this analysis, we expect to have increased understanding of design-oriented research approaches in general and to have added transparency and comparability between two existing research frameworks in this vein. CRA and DSR have similar intentionality and in many cases similar problem domains and types of resulting artifacts.

The main message and contribution of this research is our analysis of utility-based evaluation, which raises pertinent questions for claiming knowledge contributions from DSR. The pragmatist leaning of the DSR literature is reflected on the emphasis placed in the evaluation of the performance of the artifact in terms of functional requirements, instead of validation of the theoretical claims associated with the artifact. This leads us to the question regarding the relationship between theory and the artifact. In our view, it seems that acceptance and perceived utility of an artifact are only soft indications of the validity of a design theory, or do not teach one new things about the kernel theories (Walls et al., 1992) or justificatory knowledge (Gregor and Jones, 2007), as the artifact may be very weakly linked to its conceptual underpinnings. Iivari (2007) raised the concern whether the design validation in fact validates the kernel theory if the theory is not sufficiently embodied in or operationalized by the artifact. The remaining question is then: does the artifact embody the theory, or is it a proxy for operationalizing and thus examining the theory, if indeed there is any specific theory at all? And if the artifact does not act as a proxy for testing theoretical conjectures, what new insights can DSR bring to the knowledge base?

Reflecting upon the market test from a realist perspective highlights the somewhat open question of validation and evaluation in DSR literature which we should like to propose for further research. As underlined in our analysis, utility-based validation of knowledge claims has some fundamental philosophical challenges, which can be studied further at least from two perspectives. One is closed examination of the interplay between (design) theory, artifact and instantiations, and its implications to evaluation in DSR. Another is comes from Lukka (2003), and tentatively from Gil and Hevner (2011), who propose that the theoretical contribution of design-oriented research is found when examining the instantiation *ex post*.

We also have also discussed the practice of iterative and collaborative development as prescribed by Lukka (2006; 2003) and later by Sein et al. (2011) in the DSR field. As noted, there are certain challenges in assuring research rigor with collaborative development, but there are also ways of dealing with the challenges as long as the researchers are aware of them. The most apparent benefit is the increased buy-in of the artifact through increased commitment to the process and better understanding of its properties, but as collaborative development can be seen as a cure for many challenges of design, closer examination of these practices would be potentially beneficial to design research. Participation can also result in more rich descriptions of the research process and better understanding of the artifact and instantiation (Jönsson and Lukka, 2007). Accordingly, despite the risk, developing best practices for collaborative development has the potential to improve DSR.

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