

A Risk Management Framework for Design Science Research

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Abstract

As a new and complex form of research, which combines very heterogeneous activities requiring different skills, Design Science Research (DSR) in IS has new and difficult areas of risk to manage in order to ensure success. As yet, there is little experience with managing risk in DSR or even identification of types of risks to be managed. This paper analyses DSR research activities and applies the ideas and practices of risk management to DSR to develop a framework for identifying, assessing, prioritizing, and treating potential risks inherent to DSR. The framework developed identifies six potential risk areas and specific key risks within each area and includes a scale designed to simplify quantification and prioritization of risks as well as measures to mitigate risks in DSR. Finally the paper applies the framework to an ongoing DSR case study to provide initial evidence of its value and feasibility.

1. Introduction

Like most activities in life, conducting research has risks. While Design Science Research (DSR) in the IS field is subject to the usual risks of other forms of research, its novelty, new kinds and combinations of heterogeneous research activities, and its potential complexity add extra and important risks not present in other forms of research. As a relatively new area of research, DSR is particularly prone to risk, because there is still little experience in IS DSR from which to learn. As a form of research that invents new technologies, DSR presents risks not present in more traditional empirical or theoretical research. DSR also involves a complex blend of problem analysis and formulation, new solution technology development and construction, theorising, and theory testing through solution technology evaluation, often in complex organisational situations.

The purpose of this paper is to develop and provide a framework for DSR researchers to improve their ability to manage risk in DSR. To do so, we analyse, identify and describe what sorts of risks there are in undertaking DSR in IS, what causes there are for such risks, and what IS DSR researchers might do in order to assess, prioritize, and reduce or mitigate the risks identified.

What risks are relevant depends of course on what is at risk and for whom it is a risk. Those for whom there are potentially risks (and benefits) are called stakeholders. There are several key

stakeholders in IS DSR – IS DSR authors/researchers, other research participants (e.g. people and organisations by or in which newly designed technologies are evaluated), IS reviewers, editors, and publishers, (potential) readers of publications of IS DSR results (including IS researchers, IS practitioners, and managers), and those affected by actions taken by others based on IS DSR publications.

Each of the above stakeholders has different interests in and concerns for different risks in the conduct and outcomes of IS DSR. For IS design science researchers, there are risks that the research may not be successful, including risks that the research is not completed on time, on budget, or at all, risk that completed research is not publishable for a variety of reasons, and risks that erroneous results are reported, which are later found to be incorrect. For editors, there are also risks that published IS DSR results are erroneous or otherwise of low quality (e.g. lack of clarity), which may affect their reputation. For other researchers, there are risks that they misinterpret the meaning of published IS DSR or use results that are incorrect. Finally, for practical users of IS DSR (i.e. IS practitioners and managers) as well as potential beneficiaries and others affected by uses of IS DSR, there are risks that the results of IS DSR are of low quality (e.g. incorrect or unclear), leading to inappropriate actions by IS DSR users). With the proposal and development of new technologies, the risks to potential users of the technologies and those affected by them are a particular concern. Our intention in this paper is to assist IS Design Science researchers in assessing and reducing risk for the benefit of all stakeholders.

It is, of course, impossible to enumerate all possible risks to all possible DSR projects. As in the security (e.g., Baskerville, 2005) and project management (e.g., Iversen et al. 2004) literature we must be satisfied with a comprehensive framework to guide risk management, including its analysis and assessment (Bandyopadhyay et al. 1999). Without such frameworks, major risks may be poorly understood and therefore go unnoticed and untreated (Coles & Moulton, 2003). The major elements of the framework that follows are drawn from established theory in risk management. Our scope of interest is in the application of these frameworks to DSR, and we illustrate this application by developing examples of DSR risks and risk management. Because of the diversity of settings and risks, it is not possible to create a complete set of examples. Certainly, others will occur to the reader. However, the frameworks from the literature are recognized as comprehensive, and are further validated in a DSR case study.

In the next section, we review the literature on risk management and derive key principles and activities. Following that, to aid in risk identification, section 3 analyses areas of risks in conducting and utilising DSR based on the framework of DSR activities by Hevner et al. (2004). Section 4 deals with analysis and prioritisation of the risks identified. Based on that analysis, section 5 proposes ways to reduce and mitigate risks in IS DSR. Section 6 illustrates the framework for identifying, prioritising, and proposing treatment of risks on a case study of an IS DSR project currently in progress. Finally, section 7 summarises and provides recommendations for further research.

2. Principles of Risk Management

A *risk* is a potential problem that would be detrimental to a DSR project's success should it materialize. This may lead to a design with wrong or inadequate operation, rework, implementation difficulty, delay or uncertainty (Boehm 1991) *Risk Management* is identification

and response to potential problems with sufficient lead time to avoid a crisis. Thus Risk Management is proactive.

Research on Risk Management has been focused on specific issues such as quantifying risks (Apostolakis 2004), uncertainty (Alter & Ginzberg 1978), creating a sound portfolio based on risk thinking (McFarlan 1981 & 1982; Benson & Bugnitz 2004), the distinction between vulnerability and risk (Sarewitz et al. 2003), and the history of risk management (Covello & Mumpower 1985). Other streams of research in risk management have been in smaller specified areas such as software process improvement (Iversen et al. 2004).

Systematic Risk Management generally involves four main activities. The first is identifying risk: An organized, thorough approach to seek out the real risks associated with the DSR project. The second is analyzing risk: This includes examination of identified risks to determine the probabilities of undesired events and the consequences associated with those events. The third is risk treatment: Deciding how to handle or minimize the identified risks. Finally, the fourth is monitoring risks: Tracking the status of and changes in risk (e.g. potential, occurred, and omitted).

Some authors have other activities than the above four. PMBOK (2004) adds risk planning and distinguishes between quantitative and qualitative risk analysis. Likewise Boehm (1991) adds planning and splits and separates prioritization from planning.

In our framework, we use the main four activities above and consider how they can be applied in DSR. In section 3, we deal with risk identification in DSR, presenting a classification of potential risks. In section 4 deals with risk assessment and prioritization. Section 5 deals with risk treatment and, briefly, with tracking.

3. Risk Identification in DSR

There are two main ways to identify risks. One is to gather a number of key stakeholders and brainstorm “What could go wrong in this DSR?”. The second is to take a checklist of things that could go wrong or have gone wrong in other projects (i.e. potential risks) and ask one by one “Could this go wrong in this DSR project?”

In this section, we develop such a checklist of potential risks by analyzing what sorts of risks there are in conducting IS DSR. To do so, we make use of the process framework for Design Science Research developed by Hevner et al (2004) (see Figure 1), which identifies the different areas of DSR activity. We reason that the activities are potential areas of risk and identify six areas of potential risk (A-F) in Figure 1. In this section, we both draw upon our collective experience and earlier work (Baskerville et al, 2007) and reason deductively to analyse and derive key risks inherent in each area.

We can also use Figure 1 to consider what is different in risk for DSR. Of the areas of risk (A-F) that we consider, we would assert that areas C and E are unique to DSR (as a research approach) and areas A and D, while they can and have been researched separately, have particular importance and somewhat different emphasis in DSR. Further, the interactions with the knowledge base (B and F) are also somewhat different in emphasis from traditional empirical or

theoretical research and present new challenges. Finally, the complexity and diversity of the combination of all these different activities itself presents new challenges and risks to IS Design Science researchers.

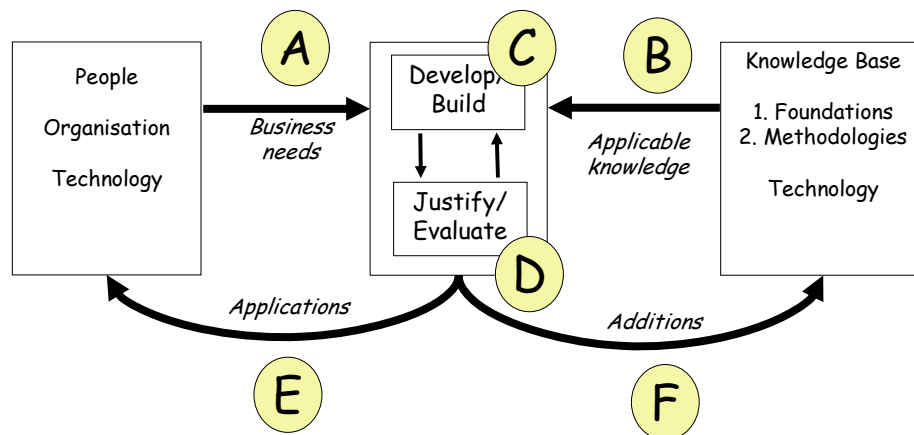


Figure 1. Process framework for Design Science Research (Hevner, et al. 2004)

A. Business Needs (Problem Analysis and Choice)

Design Science Research is oriented toward problem solving (Simon 1996, Walls et al. 1992, March and Smith 1995, Hevner et al. 2004) to meet business needs. Developing one’s understanding and formulating a definition of the problem to be solved are important parts of problem solving (of which DSR is a special case). An analysis of this activity identifies the following potential risks for DSR (numbered for later reference).

A-1 Selection of a problem that lacks significance – A significant problem should be addressed. The less significant the problem is, the less worthwhile the outcome of the DSR will be and hence the less publishable the research will be and the less useful the result will be for practice, even before one considers the cost or ease of implementing the (potential) solution.

A-2 Difficulty getting information about the problem – In some situations for some kinds of problems, it may be difficult or impossible to get adequate information about the problem to be solved. For example, people or organisations may not be willing to participate and provide information. This leads to the risk of poor understanding of the problem and other risks.

A-3 Different and even conflicting stakeholder interests – The “business needs” may not be agreed upon by stakeholders with different interests in the problem. In this case pleasing all

stakeholders and arriving at evaluation criteria or even a clear idea of what would constitute solving the problem may be difficult or even impossible. This leads to the next two risks.

A-4 Poor understanding of the problem to be solved – In order to solve a problem, it is important to understand it as fully as possible. As H. L. Menken (1917) said, “... there is always an easy solution to every human problem — neat, plausible and wrong.” Simplistic understandings of problems lead to overly simplistic solutions, which (usually) don’t work.

This is a very significant risk as it may lead to all kinds of problems, such as addressing incorrect causes or developing solutions that don’t work because they don’t actually fit the way the problem operates.

A-5 Solving the wrong problem – This risk is closely related to having a poor understanding of the problem to be solved. From within a problem space, attention may be focused on the wrong aspects and significant aspects may be overlooked. This may lead to incorrect focus and problem selection within a greater problem space.

Kimball (1957) identified this as the “*error of the third kind*” (besides type I and type II hypothesis testing errors) as “*the error committed by giving the right answer to the wrong problem*” (p.134). Mitroff and Featheringham (1974) refined the idea of type III errors as being “*the error... of having solved the wrong problem... when one should have solved the right problem*” (1974), p.383).

A-6 Poor/vague definition/statement of problem to be solved

Understanding and selecting a problem is one thing, but a problem also needs to be stated and defined, preferably with great clarity. Various aphorisms support the importance of this, including “A problem well stated is a problem half solved.” (Charles F. Kettering). Robert A. Humphrey also notes that “An undefined problem has an infinite number of solutions”, which makes for a very large search space for seeking solutions!

In addition to their restatement of Kimball’s error of the third kind, Mitroff and Featheringham (1974) also discussed the importance of problem representation and formulation, proposing that “*one of the most important determinants of a problem's solution is how that problem has been represented or formulated in the first place*”. They propose that errors of the third kind include “*the error... [of] choosing the wrong problem representation... when one should have... chosen the right problem representation*” (1974, p. 383).

A-7 Inappropriate choice or definition of a problem according to a solution at hand – This risk arises particularly (but not exclusively) in DSR. It is commonly the situation that the spark for a piece of design science research may come from familiarity with existing technologies and the recognition that they could be applied to a particular problem situation. This creates a hypothesis or theory that a proposed and as yet undeveloped technology could be useful to solve a (more or less known) problem. At this point, it is easy to concentrate on developing the solution rather than developing an understanding and a definition, or even really assessing the significance of the problem.

A-8 Inappropriate formulation of the problem – Developing an understanding of a problem and the specification of the problem to be solved can collectively be termed problem formulation. This activity is often a group activity, since problems are often solved in or by groups. Dumdum (1993) suggests six problems with problem formulation that apply here.

1. Insufficient attention to problem formulation – lack of realisation of risk

2. Bounded (limited) rationality – limited time and ability to reason effectively
3. The self-sealing tendency – focussing on a tentative formulation and avoiding conflicting formulations and evidence
4. Unchallenged assertions – accepting dubious assertions, e.g. to avoid conflict
5. Lack of issue management – not keeping track of and addressing open issues
6. The lack of common (shared) understanding – different perspectives, interests, etc.

All of these problems with problem formulation apply to DSR, but the first of these is the most important one. If the problem to be solved by DSR is not attended to, a simplistic or otherwise unrealistic or even incorrect formulation of the problem may be used as the basis for the problem solving activity. This will inevitably lead to less useful and possibly completely useless solutions, or even solutions that make things worse!

B. Applicable Knowledge (Retrieved from the Body of Recorded Human Knowledge)

DSR researchers should draw upon the extant body of knowledge from research and practice in order to facilitate and enhance the development of problem understandings and the formulation of potential solutions. All of the risks described in section A above also apply here, but other potential risks are also inherent. These are identified and described below.

B-1 Ignorance or lack of knowledge of existing research relevant to the problem

understanding – Prior research may be available that sheds light on the problem, related problems, and known causes. Failure to identify such research may lead to poor understanding of the problem or misunderstanding the problem’s significance.

B-2 Ignorance or lack of knowledge of existing design science research into solution technologies for solving the problem

– Prior research may also be in the form of known approaches, techniques, or other technologies for solving the problem. Such extant solutions may not be full solutions or may be ineffective or inefficient in some way. Failure to realise the existence of known potential solutions (even if not fully effective), or of research results about their effectiveness or reasons for any ineffectiveness may limit the choices considered when designing new solutions or even lead to “re-inventing the wheel”.

C. Develop/Build (Develop Theory/Knowledge and Build an Instantiation)

In this activity, a hypothetical (untried) solution is developed based on the ideas for a solution hypothesised by the DSR researcher(s). An instantiation is often constructed, although this may not be an outcome of the research. Whether instantiated or not, a solution is hypothetical until tried in practice.

C-1 Development of a conjectural (uninstantiated) solution which cannot be instantiated

(built or made real) – In this situation, the idea for a solution is actually unworkable (at least by the researchers involved) and cannot be constructed in any practical way. The components may not fit together or work as was envisaged. Note that this risk is primarily to the researcher and those funding research, in possibly wasting the researcher’s time and funding.

C-2 Development of a hypothetical (untried) solution which is ineffective in solving the problem – This results when the operation of the solution and/or how it will address the problem or its causes is not well understood or misunderstood. While a solution can be instantiated, it simply does not solve the (stated) problem or make an improvement. For example, Thomas Edison and his associates tested thousands of filaments before developing a light bulb that worked and lasted long enough to be useful. Edison’s work followed much other work by other researchers. This risk is primarily to the researcher (unless the results are published without suitable evaluation of the solution). However, even such (failed) results might be reported in order to direct other researchers in other directions, particularly where causes of failure are understood.

C-3 Development of a hypothetical (untried) solution which is inefficient in solving the problem – This results when the true costs of implementing the solution in practice are not well understood or as predicted. This is especially true in comparison to other potential or known solutions. Note that the worst case is that the cost of the solution (in some way) is more than the costs of the problem to be solved.

C-4 Development of a hypothetical (untried) solution which is inefficacious in solving the problem – This results when an otherwise effective solution does not function appropriately in the broader context or environment of its operation in a practical situation. In other words, there are one or more pre-requisite characteristics in the implementation environment that are not met for the solution to operate properly in that environment.

C-5 Development of a hypothetical (untried) solution which cannot be taught to or understood by those who are intended to use it – This risk is a subset of inefficacious solutions. This results when the concepts and/or parts of the solution are too complex or unfamiliar for the solution’s users to grasp.

C-6 Development of a hypothetical (untried) solution which is difficult or impossible to get adopted by those who are intended to use it – This risk is also a subset of inefficacious solutions. This may arise for many different reasons relating to the acceptability of the solution to those who would be expected to use it. For example, it may be that the solution violates some norm within the organisation.

C-7 Development of a hypothetical (untried) solution which causes new problems that make the outcomes of the solution more trouble than the original problem – This risk is a third subset of inefficacious risks. When new problems arise in an organisational situation as a result of implementing a solution, the solution may be withdrawn.

It is important to note that in situations where solutions are published with inadequate or without any evaluation, risks as described above are borne by potential users of the solutions and may be significant.

D. Justify/Evaluate (Justify Theory/Knowledge and Evaluate an Instantiation)

The above risks of untried solutions may be reduced through justification (or possibly falsification) of an IS Design Theory (ISDT, Walls et al. 1992) and the evaluation of instantiations of the solution. However, evaluation itself carries risks of making errors, resulting in possible type I (false positive) or type II (false negative) errors (Baskerville et al. 2007, Baroudi and Orlikowski, 1989). Baskerville et al. (2007) analysed potential sources of errors in testing ISDTs and evaluating instantiations. Figure 2 (adapted from Baskerville et al. 2007) shows that risks can arise at some specific stage of evaluation. These arise from sources that span the evaluation process, or from factors in the social and organisational context. Specific risks are numbered (e.g. D-1a, D-A) in figure 2 for later reference.

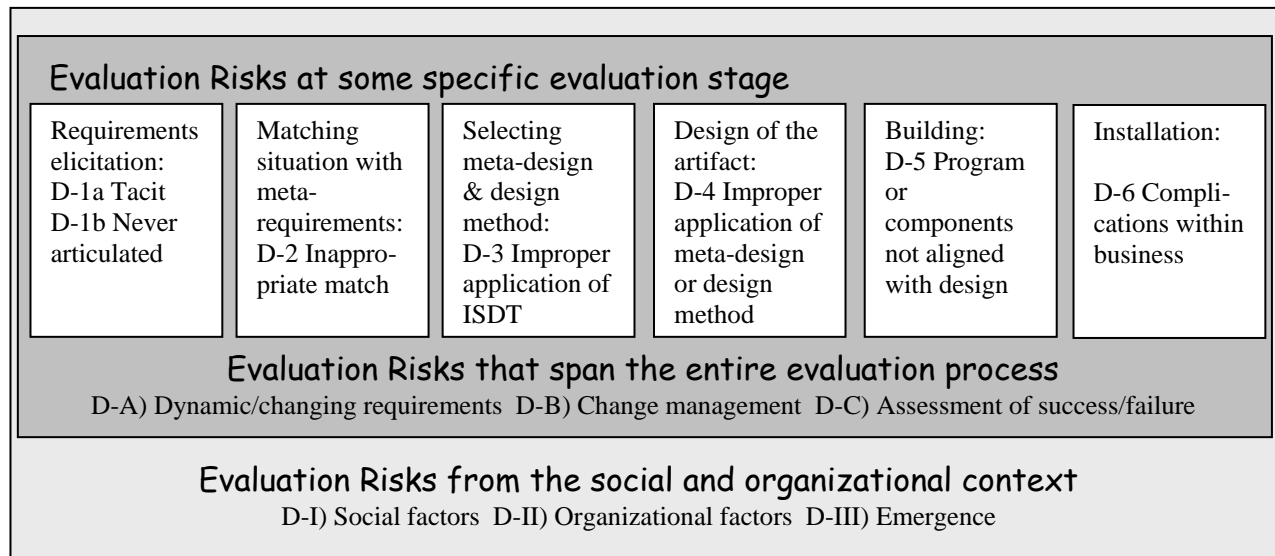


Figure 2. Potential source of DSR errors (adapted from Baskerville et al. 2007)

E. Applications (of Knowledge to Business and Organisational Problem Situations)

Once a new solution has been published and promoted to the public, especially if it doesn't work well or at all, but also even if it actually can work effectively, there are a number of other risks than those described above. The solution and how to apply it may be misunderstood and/or misapplied by those who would use it. The risks here are largely to practitioners and organisational managers, but failures may be evaluated as reflecting poorly on the solution and even the researcher(s) who developed it.

E-1 Implementation in practice of a solution does not work effectively, efficiently, and/or efficaciously – Due to insufficiently rigorous evaluation of instantiation(s) and theory justification (type 1 error) or other errors in problem formulation, etc., a solution that doesn't work properly is published and used by practitioners with consequential failure or other undesirable circumstances.

E-2 Misunderstanding the appropriate context for and limitations of the solution –

Publications describing the solution may not be clear on what contexts are appropriate (or not) for applying the solution. There may be hype in the publication which over-promotes the solution in terms of its applicability, effectiveness, or efficiency. This can lead to the application of the solution in inappropriate ways or with unrealistic expectations. To some (possibly a large) extent, this may not be the fault of the author, but of the reader, who may be seeking the “silver bullet” or “killer application”. Either way, the outcome can be undesirable for the practitioner, possibly with disastrous consequences.

E-3 Misunderstanding how to make use of (implement) the solution – In this case, the risk may arise because the author does not provide sufficient direction or appropriately anticipate use and what the reader may misunderstand, and therefore communicate ineffectively. However, the fault may equally lie with the reader, who may seek simplistic understandings or not bother to read carefully (or even at all!) and really understand how to implement the solution in his or her particular organisational context.

E-4 Inappropriate handling of adoption, diffusion, and organisational change – The practitioner may do a poor job of addressing these issues, resulting in poor outcomes, even total failure, even though the solution could have been made to work properly.

F. Additions (to the Knowledge Base of Recorded Human Knowledge)

The risks in this area are primarily to the researcher, but also to others engaged in the publication process and even other researchers and eventually the public at large. Different risks include the following.

F-1 Inability to publish or present research results – This occurs primarily if the research isn't significant or rigorous enough. However, it may also occur for other reasons, e.g. if the research is not presented clearly enough or because the knowledge is confidential or proprietary.

F-2 Publication of low significance research – This can occur because the problem is not significant enough, but also because there are other, more effective, efficient, or efficacious solutions already published about which the reviewers and editors are not aware.

F-3 Publication of incorrect research – This can occur due to poor rigour and the introduction of type I (false positive), type II (false negative), or type III (wrong problem) errors. In this case, the risk is to everyone, including other researchers and practitioners, either directly through the use of the research or indirectly through a longer chain of research that uses the incorrect results.

4. Risk Assessment in DSR

For each of the risks identified, the next task is to decide how serious a risk we are talking about. Thus we evaluate the *probability* of occurrence as well as the *consequences* if the risk should occur. This is done for each risk.

The degree of risk can either be assessed in quantitative terms as the probability of unsatisfactory events multiplied by the loss associated with their outcome, or in qualitative terms by referring to

the uncertainty surrounding the design and the magnitude of potential loss associated with project failure (inspired by Barki et al. 1993).

In some project management books it is recommended to evaluate the consequences as money lost when the risk occurs. However, this can be very difficult, so often an evaluation scale from for example 0 to 5 is used instead. An example scale for scoring consequences of a risk is shown in Table 1.

Table 1. Consequences impact scale.

Score	Consequences
0	Ignorable
1	Unimportant
2	Less important
3	Important
4	Very important /serious
5	Catastrophic / critical

In the same way probability can be scored. Here one can use percentages to express likeliness that the risk will occur. Alternatively, one can use appropriate words. In Table 2 we have shown another 0 to 5 scale, with appropriate wording for use in analyzing the probability of DSR risks.

Table 2. Risk probability scale.

Score	Probability
0	Highly unlikely
1	Very unlikely
2	Unlikely
3	Likely
4	Very likely
5	Highly likely

The importance or priority of risks can be calculated by multiplying the consequence score by the probability score. Those risks with higher scores would then have higher priority and need to be managed more carefully. For example, a risk with a consequence score of 3 and a probability score of 3 ($3 \times 3 = 9$) would be of slightly lower priority than a risk with a consequence score of 5 and a probability score of 2 ($5 \times 2 = 10$).

5. Risk Treatment in DSR

Risk treatment falls into four major categories (Dorfman 1997, Jones and Ashenden 2005):

1. Avoidance – which means that you don't do something risky – thereby avoiding the risk.
2. Controlling – means that you do something to reduce or eliminate the risk
3. Self Insurance – a means of accepting the consequences of the risk by dedicating resources toward future risk occurrence
4. Transfer – could for example involve the purchasing of insurance thereby transferring the risk to someone else

These categories are depicted in Figure 3. These categories are developed across two dimensions. Frequency of risk occurrence defines one dimension in terms of lower frequency or higher frequency risks. This dimension is sometimes regarded as probability. The second dimension is developed in terms of lower impact or higher impact risks. This dimension is sometimes regarded as cost of loss. Four general forms of treatment are typically recommended for each of the four quadrants defined by the two dimensions.

		Frequency	
		Lower	Higher
Impact	Higher	Transfer	Avoidance
	Lower	Self-Insure	Control

Figure 3. Risk Treatments.

Control

Control treatments are recommended for settings where there are frequent or likely risks, but the impact of each risk is relatively low. These treatments prevent risks in this category from having an impact on the project. This is the most common category of risk treatment, and these treatments are usually preventative in nature. An example of a control treatment for a DSR project would be the use of frequent or different forms of evaluation for determining if the project is satisfying its goals. Such evaluation detects anomalies and enables corrective actions.

Transfer

Risk transfer treatments are used for risks that are high impact, but rare. These treatments involve distributing all or part of the impact of risks across others. This is probably the second most common category of risk treatment. The usual example of this treatment is the purchase of market insurance. But insurance is not common in DSR. A more common example would be user involvement. While not usually thought of as a risk treatment, user involvement distributes responsibility for problem definition, solution search, etc., from the designers to the users,

Self-Insure

Self-insure treatments are used for risks that are unlikely and low in impact. It is a mistake to regard this category of risk treatment as a “do-nothing” option. This treatment involves various active forms of preparedness. Typical self-insure treatments involve duplication of processing such that a failure is not catastrophic. An example of self-insurance treatments in DSR would be

the production (and possibly the pursuit) of multiple designs. Primarily it means that the search for solutions continues beyond the first satisfactory solution that is discovered. Having multiple designs available can overcome risks arising from poor problem definition or requirements analysis by having alternative designs available.

Avoidance

Avoidance treatments are used for risks that are highly probable and high in impact. These involve the basic decision not to undertake a DSR project. This decision does not necessarily mean that the research project is abandoned. An example of a risk treatment in this quadrant would be the shift from a DSR research design to some other form of research, such as action research or a field experiment.

Risk Treatment Examples in Action

Risk treatments do not have to be adopted at the project level, but most can be adopted according to the risk profiles specific to a particular DSR study. Table 3 lists the different ways in which the examples of risk treatment discussed above can be applied against the seven of the various risks detailed earlier in this paper. For example, if the risk of poor problem formulation is regarded as unlikely, but of high impact, one way of treating this risk is to involve users in the problem formulation. As another example, if the risk of an inappropriate match of requirements with the situation is considered to be of low likelihood and low impact, this risk can be treated by making a second or third requirement - situation analysis and choosing the best fit of the bunch.

Table 3. Examples of specific risk treatments.

Risk	Control	Transfer	Self-Insure	Avoidance
(Example)	(Frequent evaluation)	(User involvement)	(Multiple design solutions)	(Switch research design)
Profile	High frequency Low-impact	Low frequency High-impact	Low-frequency Low-impact	High frequency High-impact
Poor problem formulation	Evaluate problem formulation	Involve users in problem formulation	Make multiple alternative problem formulations	Choose a completely different research approach
Poor articulation of requirements	Evaluate requirements	Involve users in requirements articulation	Make multiple alternative requirements statements	Choose a completely different research approach
Inappropriate match of requirements with situation	Evaluate match of requirements and situation	Involve users in matching requirements to the situation	Make multiple requirements -- situation analyses	Choose a completely different research approach
Improper application of ISDT	Evaluate application of design theory	Involve users in applying ISDT	Make multiple alternative applications of design theory	Choose a completely different research approach
Improper application of design method	Evaluate application of design method	Involve users in applying the design method	Make multiple alternative designs	Choose a completely different research approach
Artifact misaligned with design	Evaluate artifact-design alignment	Involve users in aligning the artifact and the design	Make multiple alternative artifacts	Choose a completely different research approach
Complications in use	Evaluate artifact in use	Involve users in use implementation	Use pilot, parallel, or staged implementations	Choose a completely different research approach

Finally risk monitoring is about regular follow up and asking: Has anything changed in relation to risks. Fixed intervals between continued risk identification (e.g. milestones, end of phases, steering committee meetings) are recommended.

6. Evaluating Risks: A Case Study

SourceIT is a research project that received funding from the Ministry of Research, Technology and Innovation in December 2007. The total budget is the equivalent of 6 mio. US\$ over 3 years. One University, one technology transfer organization, and three companies are participating in the project. The aim of SourceIT is to answer questions like:

- How can a company be innovative while at the same time optimizing sourcing?
- What are the pre-conditions for optimal sourcing in relation to innovative capability?

Sourcing is defined as both in- and out-sourcing, as well as decision about letting customer or client organisations develop part of the IT product. For example one of the participating companies develops an electronic patient journal system. One sourcing decision is how much of the system should be developed or adapted locally in the specific department at a hospital.

The SourceIT project is using a design science research approach to develop a method for sourcing decisions. The approach to be used is a so-called design nexus (Pries-Heje & Baskerville, 2008). It is foreseen that DSR needs to be combined with an action research approach thereby “covering a weakness in both research methods; namely that design science is extended with learning cycles characterising action research thereby ensuring better learning. And action research is extended with a formalised approach to how theory is made explicit; namely in the form of a design artefact” [translated from research application].

In January 2008 project participants from the SourceIT consortia (including and facilitated by one of the authors of this paper) carried out an evaluation using the six areas of potential risks in DSR as an inspiring starting point. A list of 14 risks was generated (see list below). For each risk, the risk area(s) (referring to Figure 1) and the specific potential risk(s) identified in Section 3 are given in parentheses after the risk:

1. It is impossible to define precisely the need for sourcing. (A-6)
2. The needs for sourcing are very different in the three participating companies. (A-3)
3. Dozens of managers have invested their soul in existing sourcing decisions; they will never admit to problems. (A-2)
4. It is hard to get access to organisations in India, to which two of the participating organisations have outsourced. (A-2)
5. There are many diverse and conflicting problem descriptions in hospitals, to which one the participating organisations wants to discuss and decide sourcing (A-3)
6. There is a vast (too much) literature and knowledge on sourcing. (B-1, B-2)
7. It may be impossible to build a Design Nexus due to lack of necessary information. (B-1, B-2, and C-1)
8. It may be impossible to build a Design Nexus because the design problem is not wicked but linear (C-1)
9. Evaluation criteria are not obvious for the appropriate choice of sourcing method. (A-2, A-3, and D-C – see figure 2)

10. It will be very difficult for many diverse stakeholders in sourcing be involved in evaluation (D-1b, D-C, and D-I – see figure 2)
11. It will take considerable time before effects of using the design artefact (sourcing nexus) can be seen (D-C, E-1)
12. It will be difficult to decide how to evaluate whether the sourcing problem is solved (D-C and E-1)
13. There is no guarantee that new knowledge will be obtained. (F-1)
14. The companies participating in SourceIT are not interested in publishing results. (F-1)

The research manager for SourceIT (one of the authors of this paper) then used the scales from 0 to 5 (see section 4 in this paper) to evaluate the consequences and probability for each of the risks in the list above. Then consequence and probability ratings were multiplied and the risk sorted by the result, which resulted in a prioritised list. The top five risks in the list above are shown Table 4 together with the treatment decided (using the treatment strategies in section 4 of this paper).

Table 4. SourceIT risk analysis fragment.

Risk #	Consequence	Probability	Treatment
14	4	3	Control: Make sure that contract for SourceIT gives researchers the right to publish - eventually anonymous
13	5	2	Self-insure: Use pilots and prototypes so it becomes clear very fast what the contribution could be
3	3	3	Control and transfer: Use many diverse problem identification techniques such as document study, observe sourcing-at-work, interview at many levels, etc.
11	2	4	Avoidance. Study effect only in projects that ends within second year of SourceIT project; leaving a full year to study long-term effects.
7	4	2	Control: Place considerable effort in literature study very early

The research manager of SourceIt believes this list of activities to avoid or control risks is extremely valuable. “It is highly likely that this will make the difference between success and failure in the project”, he states. While this last evidence is anecdotal, subjective, and likely biased, combining it with the above demonstration of risk identification and prioritisation does provide some initial evidence of the utility of the approach. Clearly though, further research and evidence is needed, such as following through to determine actual DSR project success. Whether the case will be successful requires longitudinal study and will only be seen in the future. The SourceIt project is planned to be completed in March 2011.

7. Conclusion

Like most human activities, DSR projects present their communities of stakeholders with a constellation of risks. It is one of the general tenets of risk management to evaluate the

probability (frequency) of a risk occurrence together with the consequences (cost of loss or impact). It is also a general tenet of risk management to apply four kinds of treatments of the risk management strategy depending upon the relationship between probability and consequences. Risks of lower frequency and lower impact are treated through self-insurance strategies. Risks of higher frequency and lower impact are treated with control strategies. Risks with higher frequency and higher impact are treated with avoidance strategies. Risks of lower frequency and higher impact are treated with risk transfer strategies.

Applying these general risk management tenets to activities in design science research, we analyze risky DSR activities that include analysis and problem choice, with issues such as poor problem formulation, the poor articulation of requirements, and an appropriate match of requirements with the situation. Other risky DSR activities include improper application of a design theory, improper application of design methods, a misalignment of the artifact with the design, and complications in use.

The main contribution of this paper is the development and exposition of a risk analysis and mitigation framework specifically for DSR. The framework provides a means for understanding and explaining risks in design science research. As illustrated by the case above, the framework can be further used to illuminate risks and thereby lead to extremely valuable treatments that permit practitioners to avoid or control risks. While there is some overlap with risks in other research paradigms, DSR does present a number of unique risks and collecting all kinds of risks for DSR, even when familiar in other research paradigms, has potential value to Design Science researcher.

As yet, the evaluation of this framework is not particularly rigorous, being limited to one case study in which one of the authors was a key participant and evaluator. The framework also has potential limitations and dangers. For example, the checklist of risks may not yet be complete and even when complete may not ever cover all possible risks. This raises the issue of the limitations of any form of checklist – that users may over-rely on the checklist and not consider or look out of risks not included in the checklist. While this is a limitation of the approach, the checklist approach does alert its users to risks that they may not have considered. Awareness of the limitation of the checklist approach seems to be the best solution. Clearly though, further research in this approach to risk management and in particular of this framework for DSR risk management would be useful.

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