3

Research Clarification

This chapter focuses on the first stage of DRM, the RC stage. It discusses an approach and methods to support the initial stage of a design research project or programme. The aims are to identify and refine a research problem that is both academically and practically worthwhile and realistic. This involves obtaining an overview of the available understanding of the area of interest, so that it is possible to plan for the most suitable research to solve this problem. Both the understanding and the research plan will continue to evolve as the project or programme progresses through its various stages, in particular during the DS-I stage.

Referring back to Section 2.6.1, the objectives of the RC stage are:

- to identify the goals that the research is expected to realise; the focus of the research project; the main research problems, questions and hypotheses; the relevant disciplines and areas to be reviewed, and the area in which the contribution is expected;
- to develop Initial Reference and Impact Models, *i.e.*, an initial picture of the existing and of the desired situation;
- to identify a preliminary set of Success Criteria and Measurable Success Criteria against which to evaluate the outcome of the research;
- to provide a focus for DS-I in finding the factors that contribute to, hinder or prohibit success;
- to help focus the PS stage on developing support that addresses those factors that are likely to have the strongest influence on success;
- to provide a focus for DS-II for evaluating the effects of the developed support against the goals of the research.

The deliverables of the RC stage are

- current understanding and expectations:
    - Initial Reference Model;
    - Initial Impact Model;
    - Preliminary Criteria;

- Overall Research Plan:
    - research focus and goals;
    - research problems, main research questions and hypotheses;
    - relevant areas to be consulted;
    - approach (type of research, main stages and methods);
    - expected (area of) contribution and deliverables;
    - time schedule.

3.1 Research Clarification Process

We have divided this stage into six, iterative steps, shown in Figure 3.1.

1. Identifying the overall topics of interest. This involves identifying potential research goals and problems (see Section 3.2) using the initial understanding and expectations represented in the first Initial Reference and Impact Models.
2. Clarifying the current understanding and expectations. This involves developing further the Initial Reference and Impact Models using relevant literature to identify the state-of-the-art with respect to what problems are already solved and what remains to be solved (see Section 3.3).
3. Clarifying criteria, main questions and hypotheses. This involves identifying potential criteria against which to judge the results of the research; and formulating appropriate research questions and hypotheses, based on the Initial Reference and Impact Models (see Section 3.4).

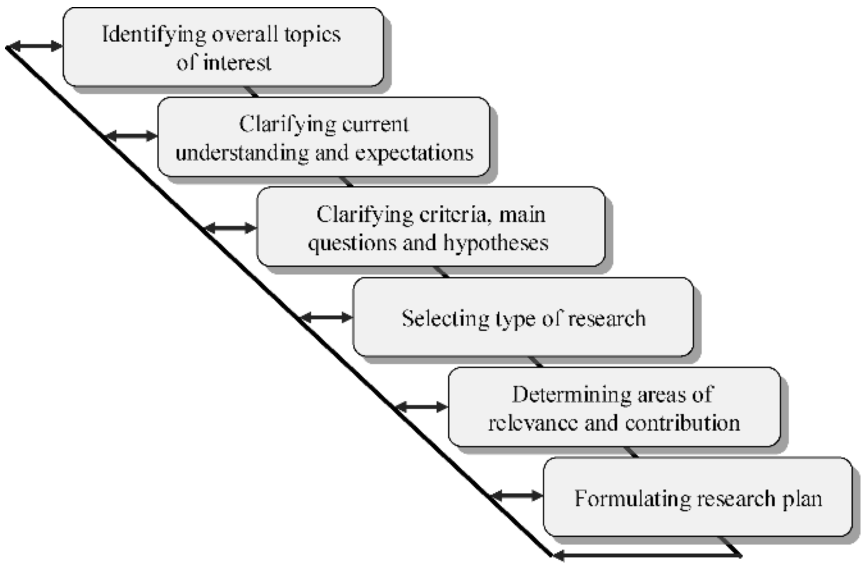

Figure 3.1 Main steps in the Research Clarification stage

4. Selecting type of research. This involves identifying the type of design research to be undertaken in order to solve the research problem (see Section 3.5).
5. Determining areas of relevance and contribution. This involves identifying the relevant knowledge areas and disciplines to be consulted to solve the research problem, and the areas and disciplines to which the research is intended to contribute (see Section 3.6).
6. Formulating Overall Research Plan (see Section 3.7).

The steps blend three strands: a gradual focusing on the research topic and the best approach; a gradual identification of the state-of-the-art and the possible contribution; and a gradual identification of the relevant criteria.

Reliability Example

In this and the next chapters, we will use the following example to illustrate the steps. A university research institute specialising in engineering design is approached by a manufacturer of large mechanical systems, who has serious problems with the reliability of these systems, despite the use of Design for Reliability methods. The company is not interested in a solution, such as the individual improvement of each existing system that has problems, but is looking for a generic solution, *e.g.*, a design support to prevent the reliability problems from occurring. This is in line with the interests of the research group and a meeting is set up to define the research project. A newly appointed researcher is given the assignment and the recommendation to follow DRM to guide his work. It is important to note that the entry point for a particular project can differ from the project used as example, and also the RC stage does not necessarily follow a linear process as this example may suggest (see Section 3.3).

3.2 Identifying Overall Topic of Interest

There are three central aspects that constitute an overall *topic of interest*:

- issue of interest (*e.g.*, reliability of designs);
- activity and/or stage of the design process (*e.g.*, evaluation, embodiment design); and
- area of application (*e.g.*, mechanical design).

The chosen issue, activity, stage and area may be given by an external partner, such as a company, government or a local interest group. The topic may also have been suggested by the researchers themselves, for example, based on the results of earlier projects, on the results of research and developments in other research groups, on personal interests, or on issues raised in the research community. In design research, the main reason why a particular issue in a particular area is considered of interest, is that the researcher, research group or research sponsors believe that this issue has an effect on design practice, although concrete evidence may not exist.

In research, as we see it, the goal is to identify and solve problems of interest that have a degree of generality, so that the fruits of the research contribute to our general understanding and – in design research – are applicable not just to a single product or a single practice, but possibly to a variety of these. Therefore, it has to be verified whether the focus area can qualify as a research area, *i.e.*, whether it is:

- *academically worthwhile*, that is, the problem is sufficiently challenging and generic and its solution is expected to contribute to our knowledge and understanding (as to what constitutes 'sufficient' varies with the available resources, *i.e.*, a three-month Bachelor's project will differ from a 5-year research programme for a group of researchers);
- *practically worthwhile*, that is, the problem has importance for practice beyond the practices of the stakeholders involved in the project, and the solution is expected to be sufficiently beneficial;
- *realistic,* that is, the research needed to address the problem is expected to be of a magnitude that can be tackled within the constraints of the project or programme.

Given our definition of design research, the problem should fulfil all three requirements.

The first suggestion for a topic may not necessarily be academically and practically worthwhile. Discussion with the stakeholders, *i.e.*, the researcher, research group, sponsors and/or practice should help to clarify the boundaries of the topic and to identify the relevant aspects and influencing factors, the most important problems and questions, as well as the criteria of success that are important for the stakeholders. The main aim is to make the beliefs and expectations of each of the stakeholders explicit, in order to obtain a first *shared* picture of the existing and the desired situations, and of the expected criteria against which to judge the research, *i.e.*, the Success Criteria.

The following checklist can be useful to guide the discussion:

- What problems/questions are important for each of the stakeholders to solve/answer? Note that a problem might have been recognised but it might not have been possible to define it.
- What benefits are solving/answering these problems/questions expected to bring to each of the stakeholders?
- What has already been tried to solve/answer these?
- How well did these solutions work? What are the reasons – known or believed – as to why the solutions did not have the expected impact? Which factors might have played a role?
- What (types of) solutions could possibly solve/answer the problems/questions?
- How could these solutions/answers be obtained?

The discussions should not only aim at gathering and documenting the available evidence, but also the underlying assumptions and beliefs. We found that many dissertations we reviewed are based on assumptions related to a particular worldview of design that is often not made explicit: *e.g.*, 'designing is information processing' and 'a more systematic approach is beneficial'. These assumptions have to be made explicit and analysed together with any other views (such as whether computers in design should support or automate) in order to identify their effect on the choice of research problem, criteria, research methods, *etc*

It is useful to start developing models of the existing and the desired situation (the Reference and Impact Models) to arrive at a shared view of the initial understanding and expectations. Discussions like the above result in:

- a preliminary set of influencing factors thought to be relevant;
- those factors that may be suitable Success Factors;
- the believed links between the factors in the existing as well as the desired situation, in particular those linked to the Success Factors;
- research problems worth investigating.

Reliability Example

In our example, a discussion between the company and the research group using this checklist, results in the following shared understanding, which is represented using networks of influencing factors.

- It is important to sort out the reliability problems in these systems.
- These problems are causing large maintenance cost, subsequently leading to loss of profit (for these systems maintenance costs are carried by the company), are putting the company's image at stake, and are believed to affect the market share – at least in the long term (see Figure 3.2, a first representation of the existing situation).

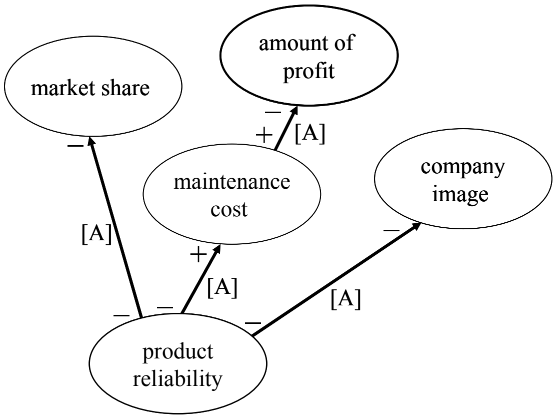

Figure 3.2 The shared understanding of the *existing* situation (all links still based on assumptions [A])

- Design for Reliability (DfR) methods have already been applied in the company, but in their experience, these did not improve reliability (see

Figure 3.3). Reliability is still considered less than what the company thinks necessary to be competitive.[7]

- It is believed that the company needs other methods for improving reliability of its products and that this will turn the existing situation around; one idea is to focus on better ways of *assessing reliability* of its products (see Figure 3.4).

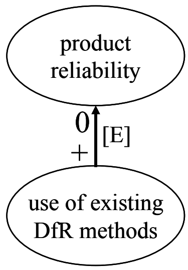

Figure 3.3 Partial Initial Reference Model: The use of DfR methods had no influence on product reliability (based on experience [E])

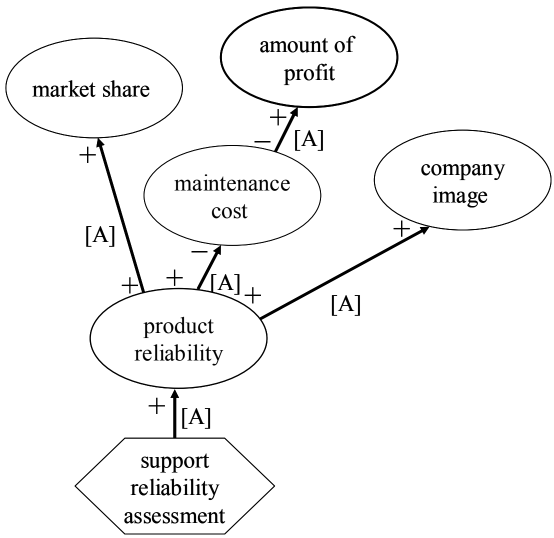

Figure 3.4 The shared view of the *desired* situation, showing the assumed impact of a support to assess reliability

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[7] This example illustrates that statements about existing design support are not only made in DS-II, in which existing support is evaluated. Such statements can also be part of RC or DS-I, as long as the main focus is not on evaluation of the support. If it still remains unclear at the end of the RC or DS-I stage why existing support does not work, and this is considered a key issue, then a DS-II will be required, before new support is developed.

In order to obtain a better feeling for whether the desired situation is realistic and practically worthwhile, it is necessary to better understand the reasons behind the problems in the existing situation. This will also provide important information for the development of support that is supposed to address these reasons, and will help the research group decide whether the problem is sufficiently challenging and generic to be academically worthwhile.

In our example this raises the following questions: Is the desired situation as shown in Figure 3.4 realistic and practically worthwhile, *i.e.*, would developing support for assessing reliability be possible and solve the issues? Why did the applied DfR methods not have the desired effects?

A further discussion between the company and the research group resulted in the following additional understanding:

- If a method does not show effect, the inappropriate *use* of the method can be a reason. If the method is well established and applied with success elsewhere, the problem lies in the way the company uses the method and cannot count as a research problem (see Figure 3.5, left arrow). The methods used in the company are well established and are used correctly according to the company, but they saw little or no effect (see Figure 3.5, right arrow). The question is why these methods do not have an effect in this company.

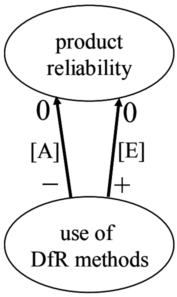

Figure 3.5 Partial Initial Reference Model: Possible reasons for the lack of reliability: existing method not applied correctly (*left arrow*, assumption) or method applied correctly but little or no effects (*right arrow*, according to the experience in the company)

- The company suggests two reasons as to why the application of existing DfR methods did not have an effect. First, the established methods can only be applied in the detail design stage. Discussions with the designers revealed that the methods sometimes *did* identify reliability problems, but that these problems could not be addressed to a satisfactory degree because of the advanced stage of the project. Secondly, the methods are not specifically developed for the types of machine system the company develops and it might be that they therefore do not apply well. The resulting partial model is shown in Figure 3.6. The lines connecting the edges indicate that the result – in this case ‘0’ or ‘no effect’ – occurs when both statements apply.

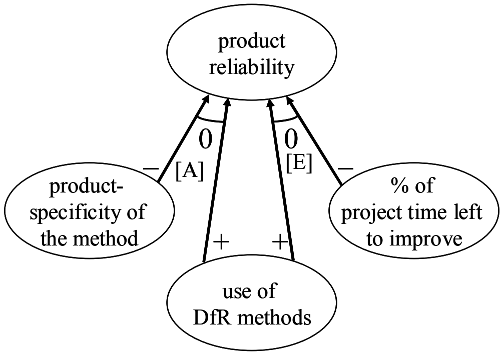

Figure 3.6 Partial Initial Reference Model: Reasons why correct application of a method may not have the desired effect

- The researcher sees an interesting research opportunity in the fact that it seems to be the current applicability of DfR methods in the *detail* design stage, which limits the effect. He suggests to focus on reliability assessment in *earlier design* stages, assuming that earlier detection of potentially unreliable product solutions is not only possible and more effective, but also more efficient. The earlier a problem is identified, the easier it might be to solve. The researcher illustrates this argumentation by expanding the initial description of the desired situation (shown in Figure 3.4) into the Initial Impact Model of which the relevant part is shown in Figure 3.7, indicating the assumptions made at this stage. Note that all possible influencing factors related to the support, such as quality of application, quality of introduction, time needed for application, *etc*., are not included yet.

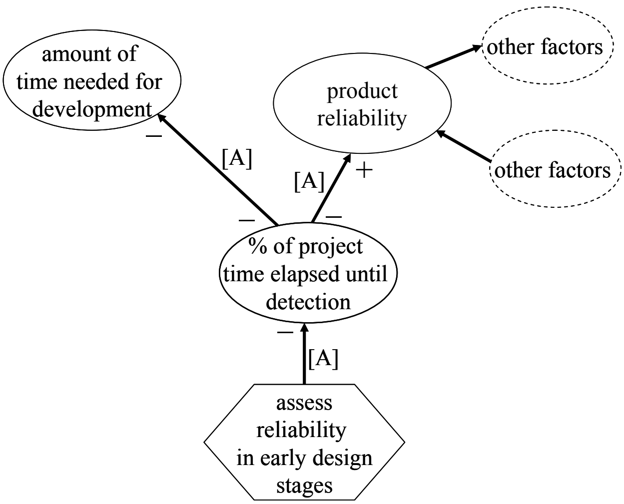

Figure 3.7 Part of the Initial Impact Model, introducing development time as a new factor

Investigating this problem seems very interesting from an academic point of view and the first formulation of the *research problem* is chosen to be ‘How to assess reliability of mechanical systems in an early stage of the design process’. Several issues remain to be resolved, in particular: How early is this possible? And what has already been done to address this issue?

Choosing Area of Research

It is important to choose the area of research carefully. In our example, the choice of research area was strongly driven by a practical point of view and could thus be specified relatively early in the research project, but care has to be taken that the area is also academically worthwhile. Several authors give suggestions for selecting areas from an academic point of view.

- Do not follow the crowd (Oliver 1991). It may be beneficial to learn first what the crowd is doing in a partticular branch of science and spend substantial time visualising what is going on. It will then become clear what the crowd is not doing, and promising, unexplored directions may emerge.
- Take a long-term perspective. Often, it is helpful to see your area in long-term perspectives, and looking at a known area from a fresh perspective may lead to the emergence of new and exciting problems (Oliver 1991).
- It is important problems that lead to significant contributions, and the importance of a problem does not correlate with the difficulty in solving the problem (Thomson 1957).
- To locate problems, one can “play contradictions” – invent possible contradictions and see if they are true – or “play implications” – push the idea to its limits and see if it works (Root-Bernstein 1989). As Kuhn (1970) suggests, revolutions follow the recognition of anomalies.
- “Undertake a project manifestly important and nearly impossible. If it is manifestly important, then you don’t have to worry about its significance. If it is nearly impossible, you know that no one else is likely to be doing it, so if you succeed, you will have created a whole domain for yourself.” (Edwin Land the inventor of the Polaroid process and Land camera, quoted in Root-Bernstein 1989).
- It is important to “rebel but wisely” (Oliver 1991). Those seeking to make major contributions do not serve by adopting unsound positions no matter how unconventional or superficially appealing the position may be. It may be helpful to seek the non-questions – those questions that might have been asked but somehow forgotten or ignored.

The general idea is that the researcher should be able to question the dogmas and preconceptions of the field of enquiry, and challenge the existing boundaries.

3.3 Clarifying Current Understanding and Expectations

The results of the first step are topics and areas of interest shared by the stakeholders, and an initial understanding of the problem and solution directions, expressed in first, tentative models of the existing and desired situations. An exploratory review of the literature is necessary to clarify the current understanding and expectations, among others, by identifying the extent to which the problems are already solved in practice or academia, and what still remains to be solved. Such a 'reconnaissance survey' (Oliver 1991), helps develop an overall understanding, and avoids wasting time on details that have little importance in the overall scheme.

The models of the existing and desired situations are used to guide the literature search and the findings are in turn used to refine the models. The exploratory literature search will reveal the many dimensions of the topic of interest. New topics and problems might be identified that were not originally anticipated, and it might become clear which factors have most influence. The results of this step are an Initial Reference Model and an Initial Impact Model, sufficiently detailed to determine a suitable research plan.

To determine whether a publication is relevant or not, we suggest the following steps for a quick read:

- Read the abstract.
- If the abstract is interesting, then read the introduction and the conclusions.
- If these are interesting and relevant, then read the results.
- If these are relevant read the background, objectives and setup.

This quick read is aimed at determining:

- What (what is the objective)?
- Why (why have the authors done so)?
- How (which research methods were used)?
- Results (what are the findings)?
- How good (what is the quality of the research)?

For details about reviewing the literature in depth, see Section 4.4.

Descartes (Ramon y Cajal 1999) advises researchers not to acknowledge as true anything that is not obvious. This is echoed by others who advise to (1) avoid the false concept that the most important problems are already solved (Ramon y Cajal 1999), and (2) never fully accept any hypothesis, theory, law or doctrine (Oliver 1991). Arhenius goes even further and claims that things that are already said to be impossible are the most important to pursue for the progress of science (Root-Bernstein 1989). It is important to learn to consult work in other languages as this broadens the horizon of knowledge.

The literature is used to check each assumed or experienced link in the models to see the extent to which these have been shown to exist, or can be expected to exist using the available evidence (see Section 2.4 for a description of the symbols and their use). Even if statements in the literature seem obvious, it is important to check whether they have a sound basis or are based on assumptions. Preference should be given to statements that are based on clear evidence, in particular to those that have a similar context as one's own area of interest (see also Section 4.4.2)

The literature is also checked for additional influencing factors and links not considered earlier. Factors that are relevant but fall outside the scope of the research, are represented as nodes with dashed lines or aggregated as 'other factors', as shown in Figure 3.8.

Reliability Example

Taking our example, it is not clear yet whether a research project is required, as the understanding and expectations are based solely on available information and the interest within the research group and the company. For instance, it is possible that the problem has already been solved or has been addressed by other researchers unknown to the research group or company.

An exploratory review of the literature on existing support and support proposals reveals several solutions to address reliability problems: mathematical methods for calculating reliability, descriptions of guidelines and methods for assessing and improving reliability, and evaluations of the use of various support. However, these solutions can only be applied when details of the system are known, not in the early stages; the research problem identified seems unresolved.

The next step is to verify the factors and links within the models and modify these where necessary using an exploratory review of the literature and further discussions with the stakeholder, to determine the kind of research that is necessary to solve the problem. This results in the following changes to the Initial Reference Model, as illustrated in Figure 3.8, showing the relevant part of the model:

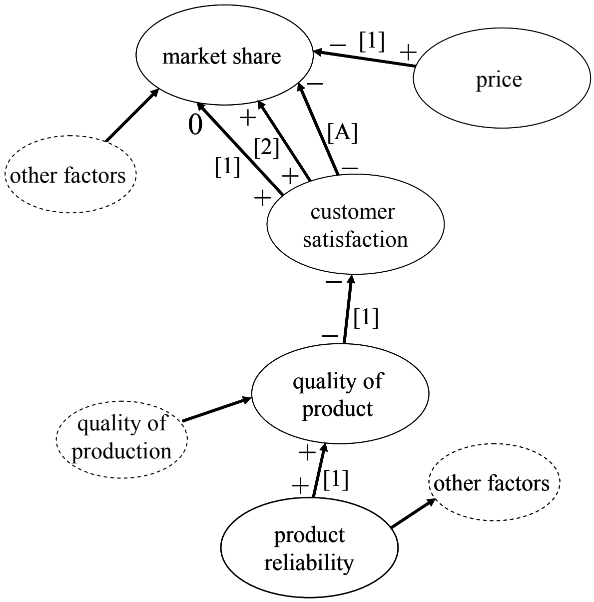

Figure 3.8 Part of the Initial Reference Model based on evidence from literature (all sources are ficticious)

- Earlier (Figure 3.2), it was assumed that low product reliability had a negative effect on the market share. However, no evidence is found for this direct link in the literature. Instead, according to one source, referred to as [1], a link *via* 'product quality' and 'customer satisfaction' exists. Two nodes are added accordingly.

- The statement in source [1] is that a reliable product was found to have a positive influence on how customers judged the quality of the product. Furthermore, a poor quality of the product resulted in customer dissatisfaction. The links in the Initial Reference Model are labelled accordingly.
- Source [1] found no clear evidence that customer satisfaction relates to market share, in contrast to source [2] where a positive effect was found. The latter seems more relevant for the research project, because the evidence found belongs to the same domain, namely machine systems. Note that choosing a link as most relevant, only because it confirms one's own assumptions, is not acceptable. Both links are added.
- Based on the strength and relevance of the evidence of [2], which only dealt with *high* customer satisfaction and its effect on market share, the researcher decides that it could be possible, that poor customer satisfaction has a detrimental effect on market share and adds this link as an *assumption*.
- Source [1] reveals further factors that influence market share, amongst which 'price' seems relevant, as 'maintenance cost' and the 'amount of profit' are influencing factors in the original model of the existing situation (see Figure 3.2).
- Other sources provide evidence that product reliability also affects other factors, and that 'quality of production' influences 'quality of the product', but all of these fall outside the scope of the project.

The partial Reference Model in Figure 3.8 shows a *complete* link based on evidence between 'reliability' and 'market share', *i.e.*, from the factor of interest to one of the factors that constitutes an important goal for the company. A complete link provides a strong basis for a research project. However, there is no further evidence yet about:

- the factors that affect product reliability, *i.e.*, what makes products (un)reliable;
- other, potentially more influential factors that affect the existing situation;
- the key assumptions in the Initial Impact Model (see Figure 3.7): does assessment in the early stages lead to earlier detection of problems and thus improves product reliability because improvements can be made in time. These assumptions (links) need verification if this is to be the basis for the support.

Resolving the above issues requires a detailed review of the literature. The researcher decides, however, that the current understanding and expectations, as represented in the Initial Reference and Impact Models, are adequate to determine the kind of research necessary to address the formulated research problem; the necessary detailed literature review will be the focus of the next stage, DS-I.

Reliability Example with Alternative Outcome

If the initial literature review would have not resulted in sources [1] and [2] but some other publications, such as [3, 4 and 5], the Initial Reference Model would have been different (see Figure 3.9) based on the following alternative understanding of the existing situation:

- No evidence is found about the link between product reliability and market share. The assumption remains as in Figure 3.2.
- One particularly detailed study [3] on reliability in the area of earth-moving equipment showed that low reliability causes high maintenance costs, and that high maintenance costs cause the warranty costs and the operating costs to increase.

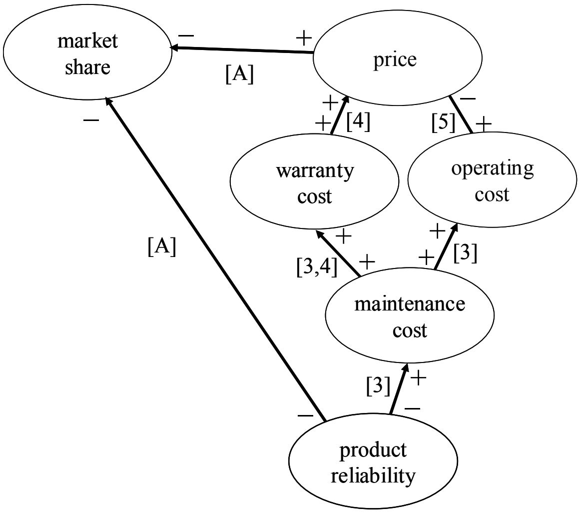

Figure 3.9 Alternative Partial Initial Reference Model

- A publication on warranty costs [4] confirmed the link with maintenance costs and found that warranty costs tend to be calculated into the price of the product, thus increasing the price.
- Publication [5] found a link between high operating costs and low price, but could not find a causal link: some products were sold at a low price to compensate for the high operating costs, other products had to have a low price for competitive reasons and the operating costs were made high, *e.g.*, through insurances or obligatory service contracts, to realise enough profit. In the Initial Reference Model a link without an arrow is added between ‘operating cost’ and ‘price’, to indicate that these factors are linked, but no cause could be established. The combined effect of the two factors ‘operating costs’ and ‘warranty costs’ on ‘price’ is not clear.
- It was assumed that ‘price’ affects ‘market share’, but no real evidence could be found related to mechanical systems.

The resulting Initial Reference Model, part of which is shown in Figure 3.9, shows an *incomplete* link between ‘product reliability’ and ‘market share’, both directly and indirectly (*via* price), in contrast to the model in Figure 3.8. This alternative model is too weak a basis to focus the research on developing a new method to support reliability assessment. If effects of ‘product reliability’ on ‘market share’ (taken as an important criterion for the stakeholder) are not clear, there is no reason to assume that a new method to assess product reliability could improve market share, even if it improved reliability. In this case, the kind of research needed is to obtain a better understanding through a Comprehensive DS-I.

A Note on Different Entry Points

In the example, the impulse for the RC stage was a problem experienced by a company and the wish of this company to focus on developing an effective tool. The research started with a prescriptive goal: the development of support. This is why early on a model of the desired situation could be drawn and some tentative criteria could be formulated (market share and profit). Nevertheless, as the example showed, obtaining a good understanding of the existing situation is crucial. Knowledge about the reasons for the problems experienced and about available support and support proposals helps identify whether there is indeed a need for developing a new support, and if so, which issues this support should address. Maybe a different type of solution than initially anticipated is required. As the example showed, the research plan might have to be changed, in this case, to include research to achieve the descriptive goal of improving the understanding before developing a support.

The impulse for doing research can start from a descriptive goal: improving our understanding of a particular situation, *e.g.*, the way in which requirements and solutions develop within a product development project. In this case, the starting point will be a preliminary model of the existing situation. We believe that most investigations are undertaken with a purpose based on the belief that the understanding gained can be used, ultimately, for addressing a particular problem. It is necessary to make this belief explicit; the initial model of the existing situation should therefore contain the links between the factors of interest and success factors. Only then is it possible to identify which improvements might be most effective and efficient and to develop a vision of the desired, improved situation.

A third impulse for doing research may be that support exists but the results are not known, but assumptions about its use and usefulness exist. The research starts with another type of descriptive goal, an evaluative goal: understanding the effects of a support through its evaluation. Information about the support, as well as its introduction, implementation, training, use, *etc.*, needs to be collected and a model of the desired situation (effects) has to be developed, if no Impact Model is already available. Developing an initial model of the existing situation without the support, if not already available, will show the problems the support was supposed to solve.

A fourth impulse for doing research may be that support exists but the results are unsatisfactory. The research starts with the descriptive goal: understanding the causes of the unsatisfactory results of the support. In this situation too, information about the support has to be collected and a model of the desired situation developed, if not already available. The RC stage focuses on the development of an initial model of the existing situation, *i.e.*, the unsatisfactory situation with the support.

In summary, irrespective of the original research goal, initial models of the existing situation and of the desired situation including the preliminary success factors are necessary to clarify understanding and expectations, and select the type of research.

3.4 Clarifying Criteria, Main Questions and Hypotheses

To determine the focus of the research, it is necessary to identify the criteria that are considered essential to determine whether the results help achieve the aim and to determine the main questions and hypotheses.

3.4.1 Criteria

To be able to judge the existing situation and suggest efficient and effective ways of improvement, our understanding needs to involve a link to success, *i.e.*, there should be a *complete link* between the factors that are of interest – the Key Factors – and the factors (Success Factors) the research project sets out to understand and/or influence as described in the research goal.

In Section 2.5 we defined Criteria as the desired values of the Success Factors. A distinction was made between Success Criteria and Measurable Success Criteria (as well as Success and Measurable Success Factors). Success Criteria relate to the ultimate goal to which the research project or programme intends to contribute and usually reveal the purpose of the research and the eventual, desired influence on practice. Measurable Success Criteria were introduced as those criteria that are linked to the chosen Success Criteria and that can be applied to judge the outcomes of the research given the available resources. They should serve as reliable proxies for the Success Criteria. As to which factors can act as Measurable Success Criteria depends to a large extent on the constraints of the project. The factors and criteria chosen in the RC stage are called *preliminary* to indicate their tentative nature at this stage.

In our example several potential Success Factors were mentioned: ‘market share’, ‘amount of profit’ and ‘company image’, for instance. A discussion with the company reveals that their interest is primarily in ‘improving market share’. This focus seems acceptable from the understanding gained from the literature, and reflected in the Initial Reference Model in Figure 3.8, which showed a complete link between the preliminary Key Factor ‘product reliability’ and this preliminary Success Factor. The outcome of the alternative literature review, shown in Figure 3.9, illustrates that it might not be clear whether such a link exists until further research is undertaken.

The chosen preliminary Success Criterion ‘increased market share’, however, can only be assessed once the product is out in the market, which is outside the timeframe of the project. A factor directly linked to the preliminary Success Factor ‘market share’ is ‘customer satisfaction’. Assessing the value of this factor requires at least a functioning prototype of the product. Assuming that this too will not be possible within the duration of the research project, 'product quality' is chosen as the preliminary Measurable Success Factor, and 'high product quality' as preliminary Measurable Success Criterion. This criterion needs further operationalisation in order to be able to be used, *i.e.*, the terms 'high' and 'product quality' have to be defined in such a way that this can be assessed within the project. For further details on formulating operational definitions, see Section 4.5.2.

Figure 3.10 shows the Initial Reference Model based on the literature sources of the example and the alternative example. The model includes the preliminary Key Factor and the preliminary Success and Measurable Success Factors. Note that this model is simplified. In a research project, a Reference Model is likely to be more complex and to have more than one Success and Measurable Success Criterion.

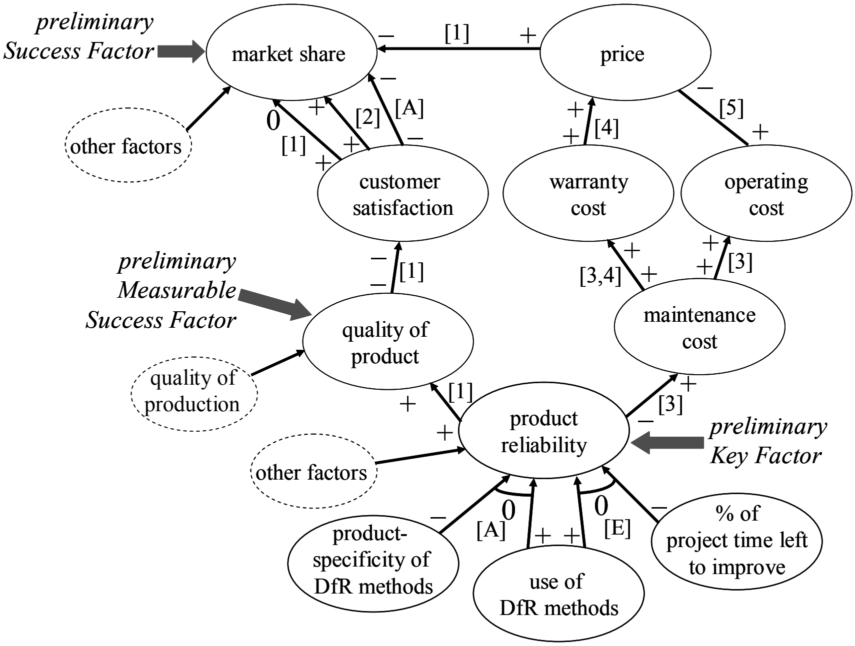

Figure 3.10 Initial Reference Model including preliminary criteria

Based on the Initial Reference Model and the expectations, the Initial Impact Model is updated, and the preliminary Key Factor and preliminary Success and Measurable Success Criteria added, see Figure 3.11.

As the research project progresses, Measurable Success Criteria become more precisely defined and may change. Success Criteria usually do not change. For instance, the support developed may introduce new influencing factors, which may require new Measurable Success Criteria, or the development of a support may prove to be more time consuming than expected. As a consequence less time is available for evaluation, and fewer or even different criteria may have to be chosen. When alternative Measurable Success Criteria are chosen, care should be taken that the corresponding Factors too are as closely linked as possible to the Success Factors.

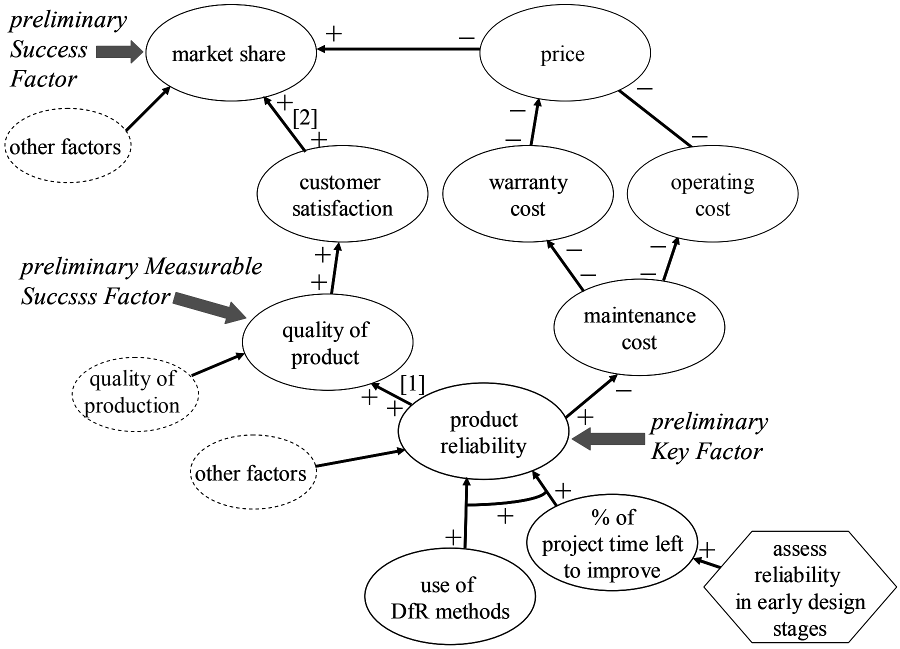

Figure 3.11 Initial Impact Model with preliminary Criteria

3.4.2 Research Questions and Hypotheses

We often observe that PhD students present their work describing what they are doing or planning to do, but fail to state the research questions and hypotheses behind their work. When research questions are formulated, they are often very vague, too encompassing to be answered within one PhD project, and concerned only with the support to be developed (*e.g.*, ‘how to assess reliability’). To undertake research, the formulation of the main research questions and hypotheses early in the project is essential. During the course of the project, these will be refined and elaborated on to focus the various stages of the research process.

A *research question* is a question for which no answer exists yet. Research questions can be formulated in various ways, such as: What are the characteristics of a successful product? How often do designers..? How do designers do..? What are the ways in which..? How long does it take to..? When does…? Why is…? The type of question determines the research approach and, in particular, the methods that can be used. The selection of the most suitable methods is discussed in Section 4.6. In our example, some of the research questions would be: What causes the lack of product reliability? How does product reliability influence maintenance cost? How can we assess reliability in an early stage?

An *hypothesis* is a tentative answer to a research question in the form of a relationship between two or more concepts, or in our case, between two or more influencing factors, including the Success Factors. That is, an hypothesis is a claim or statement about a characteristic of a situation, or a proposed explanation for a phenomenon. Hypotheses are tested as to whether they can be accepted or have to be rejected given the available evidence. In our example, an hypothesis behind the expected effect of the support is that 'If a lack of reliability is detected in an early design stage, sufficient project time is left to improve the product'. Every link can be formulated as an hypothesis, *e.g.*, 'An increase in product reliability will increase the quality of the product' (see Figure 3.11). Because this link is crucial in the Reference Model and based upon a reference from a different area of application, it would be important to investigate this hypothesis.

The main research questions and hypotheses can be derived from the research goal, the Initial Reference and Impact Models, and the related discussions. A detailed discussion on how to formulate research questions and hypotheses, so that they can be answered and verified, can be found in Section 4.5.2.

3.5 Selecting Type of Research

The next step is to identify the type of research suitable to answer the chosen research questions and verify the hypotheses. In Section 2.3, Figure 2.2, the seven main types of design research within the DRM framework were presented (reproduced here as Figure 3.12).

Research Clarification	Descriptive Study I	Prescriptive Study	Descriptive Study II
1. Review-based	Comprehensive		
2. Review-based	Comprehensive	Initial	
3. Review-based	Review-based	Comprehensive	Initial
4. Review-based	Review-based	Review-based Initial/ Comprehensive	Comprehensive
5. Review-based	Comprehensive	Comprehensive	Initial
6. Review-based	Review-based	Comprehensive	Comprehensive
7. Review-based	Comprehensive	Comprehensive	Comprehensive

Figure 3.12 Types of design research projects (iterations omitted)

As discussed in Section 2.3, a review-based study is based on the review of the literature on design or on design support only. A comprehensive study is a study in which the results are produced by the researcher, *i.e.*, an empirical study, the development of support, or the evaluation of support is undertaken by the researcher. A comprehensive study always includes a review-based study. An initial study closes the project and involves the first few steps of a particular stage to show the consequences of the results and prepare the results for use by others. Each research type is discussed in more detail in the following paragraphs.

Type 1. Comprehensive Study into Criteria

This type of project is undertaken when Success and Measurable Success Criteria are little understood, and therefore, a Comprehensive DS-I into understanding these criteria, their links and their relationships with the research problem is to be carried out. The outcome will be a better understanding of what constitutes success and which metrics can be used.

Type 2. Comprehensive Study of the Existing Situation

This type of study is undertaken when the criteria can be established, but a better understanding of the existing situation is necessary to identify the factors that are most relevant to address in order to improve this situation. A Comprehensive DS-I is necessary when the literature review reveals that understanding is:

- non-existent: the literature does not provide links between the factors of interest and the selected Success Factors;
- insufficient: the literature provides links but with insufficient detail; results are inconclusive or contradictory; evidence is based on a different context from the research; evidence is weak because of the small number of cases involved or the research methods applied;
- potentially incorrect: validity of the method(s) used is doubtful.

Once sufficient understanding is gained, an Initial PS is to be undertaken to indicate how this understanding can be used to improve design. This involves determining the factors that, when addressed, are most likely to have a large impact on success, and suggesting ways of addressing these factors.

Type 3. Development of Support

When the understanding of the existing situation obtained from the literature review and reasoning (Review-based DS-I) is sufficient to start the development of support a Comprehensive PS is undertaken if existing support is:

- non-existent: the literature, however, indicates or demonstrates the need to develop support to improve the existing situation;
- insufficient: the literature indicates or demonstrates that existing support is not used, does not work properly, only addresses part of the problem or is no longer effective or efficient in the context of new technologies, requirements and contexts.

The resulting Actual Support will be subject to an Initial DS-II for evaluation.

Type 4. Comprehensive Evaluation

In this case, support already exists. An evaluation of its application, however, is not available. A Comprehensive DS-II is undertaken to evaluate the support. The evaluation is based on a Review-based DS-I to understand the situation the support is intended to improve, and on a Review-based PS to understand the support and the expected effects (the desired situation). These reviews are necessary, because the support can fail due to incorrect assumptions or incorrect development. The evaluation can involve the comparison of multiple support. A Comprehensive DS-II may be necessary when current evaluations are

- non-existent: no formal evaluation of the application and effect on success of the support can be found in the literature;
- insufficient: earlier evaluations focused on Support or Application Evaluation, rather than Success Evaluation; the observed effects are unclear or contrary to expectations; earlier evaluation results were negative and the reasons are unknown;
- potentially incorrect: validity of the method(s) used is doubtful.

A Comprehensive DS-II is followed by suggestions for improvement (Initial PS), or further development (Comprehensive PS).

Type 5. Development of Support Based on a Comprehensive Study of the Existing Situation

A research project of this type is a combination of Types 2 and 3. The aim is to develop support, but the level of understanding of the existing situation is poor. Therefore, the research involves both the development of the understanding (Comprehensive DS-I) and, based on this, the development of support (Comprehensive PS). As with any comprehensive support development, this is followed by an Initial DS-II.

Type 6. Development of Support and Comprehensive Evaluation

A project of this type combines Types 3 and 4. The level of understanding of the existing situation obtained from the literature (Review-based DS-I) is sufficient to develop the support (Comprehensive PS), and the project resources allow formal evaluation of the support (Comprehensive DS-II). Depending on the results of the evaluation and the available resources, this is followed by a revisit of the PS or DS-I stage, either as an Initial study or a Comprehensive study.

Type 7. Complete Project

This is a project in which comprehensive studies are undertaken in each DRM stage. The RC stage will have shown that little has been done in the area of interest, yet indications are that the area has potential. As a result, research projects of this type involve; a comprehensive study of the existing situation (Comprehensive DS-I); development of support (Comprehensive PS); and a formal evaluation of this support (Comprehensive DS-II). This is followed by modifications to the support and understanding where necessary. In certain projects it may be required to start with a detailed investigation into criteria itself (Comprehensive DS-I as for Type 1). As carrying out all these stages in depth requires substantial time and resources, this type of research is more common for the work of a research group, unless a problem with a very specific scope is addressed.

Reliability Example

In our example, the researcher aims for Type 5. When the outcome of DS-I reveals a considerable lack of knowledge about reliability assessment, it might be necessary to change to Type 2.

3.6 Determining Areas of Relevance and Contribution

In our example, the initial literature review (see Section 3.3) focused specifically on reliability, and revealed details about its various dimensions. The aim was to use this understanding to develop the Initial Reference and Impact Models and a preliminary set of criteria, in order to determine the research topic and to select the type of research (Sections 3.3 to 3.5). In other words, the aim was to *identify* the research problem.

In order to *solve* the research problem, however, this initial review is not sufficient. The literature needs to be looked into in more detail, considering all potentially relevant areas, not only those related to the research topic or one's own discipline. It is important to consider a wide range of areas and disciplines. To draw a comparison with product development; no company will develop something without looking for what exists and for interesting ideas in other products. Other disciplines and areas might have undertaken interesting studies, might have developed interesting theories, methods, concepts, solutions, *etc.*, that could be relevant, if looked at carefully in an analogical way. For example, for analysing the icons used in user interfaces and for developing support to develop intuitive user interfaces, Hurtienne used linguistic theories about metaphors (Hurtienne *et al.* 2008). Sometimes the research methods in other disciplines can be very interesting for design research. Breakthroughs in research often emerge at the intersection of areas or where knowledge, ideas and methods have been transferred from one area to another.

In our reliability example, the literature on the following topics can also be relevant: robust design, tolerancing, ageing, wear, information exchange (between service/maintenance and designers), life-cycle costs, maintenance, product liability design methodology, conceptual design, Design-for-Manufacturing, Design-for-Assembly, design thinking, human–computer interfaces, *etc*.

To avoid getting lost in this 'jungle' of the literature it is useful to ask the following questions:

- What are the areas that could be related to the topic in question?
- How directly relevant are these to the topic: which ones seem essential, which ones useful and which ones might be useful?
- In which of these areas is the researcher's contribution likely to be? This area or these areas should be compatible with the researcher's expertise, as well as with the goals of the project.

To help represent the answers to these questions, we developed the **Areas of Relevance and Contribution diagram**[8] (**ARC diagram**). This representation clarifies the foundation on which the research is to be based and the area(s) of contribution of the research. Students have found this diagram very useful for structuring their literature search, for structuring the literature chapter in their publications, for presenting, discussing and reflecting on the areas they consider relevant, and to clarify the area of their contribution.

For developing an ARC diagram, we suggest the following steps.

1. Draw an oval (or any other form) carrying the research title, goal, topic, or main research question.

    - Separate diagrams can be drawn to address additional (sub)goals, topics and research questions, or – as done here – one diagram can be drawn to cover all of the research.
    - Additional, more specialised diagrams can be drawn in each DRM stage to focus on the questions addressed in that particular stage.

2. Draw areas around this central oval, labelled with those disciplines that could be relevant for the research topic in providing possible theories, models, background information, existing methods, results of empirical studies, *etc.*. Figure 3.13 shows an example, based on the diagram developed by one of the PhD students attending our Summer School.

    - Analyse every word in the main oval as well as those in the Initial Reference and Impact Models, in the formulations of the research problem, and in the questions and hypotheses in order to identify relevant disciplines.

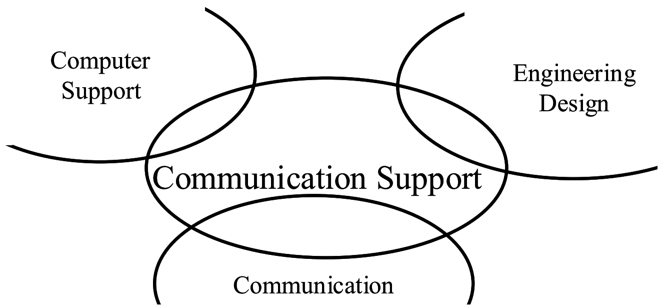

Figure 3.13 Example: Second step of setting up the ARC diagram for the project on 'Analysis and support for communication throughout the design process' (these and the following figures are adapted from the diagram of Thomas Flanagan, Summer School participant, unpublished, with permission).

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[8] In earlier publications, we called this diagram the Theoretical Foundation and Contribution (TFC) model.

- Be as specific as possible if you are familiar with a discipline, *e.g.*, thermodynamics, rather than physics, or cognitive psychology, rather than psychology.

3. Identify the specific areas or topics within these disciplines that seem relevant, and put these in or around the discipline areas (see Figure 3.14).

   - Sub-areas can be represented using smaller ovals or circles connected to the related main area, but further away from the centre.
   - Rearrange the areas such that clusters of areas can be identified easily.
   - Try to be as informed as possible. In order to identify areas in a particular discipline other than one's own discipline, it is useful to look at: websites; handbooks, or lecturing materials of key institutes in the discipline (student editions with high edition numbers are likely to provide commonly accepted descriptions of main research areas and concepts); book series; refereed journals.
   - Consider a broad range, but be selective in the final choice: everything could potentially relate to everything.

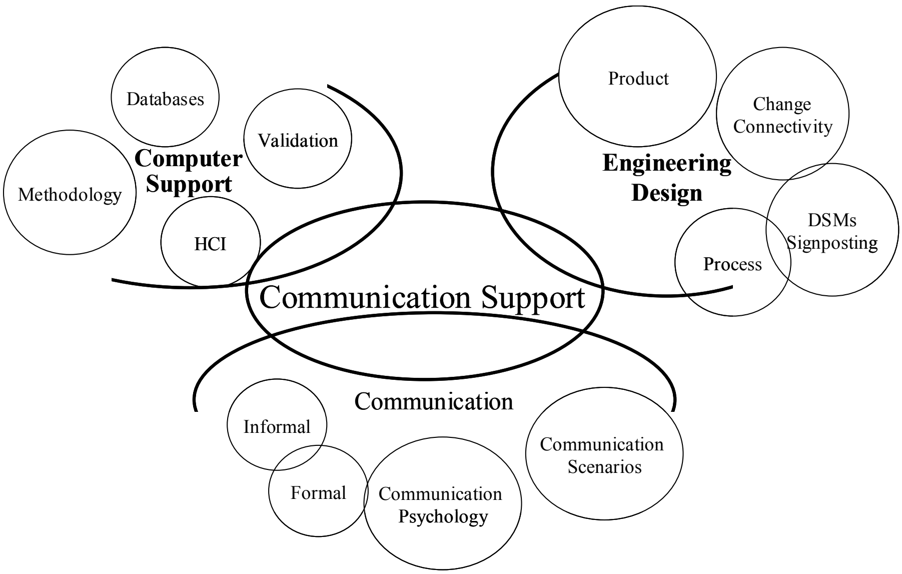

Figure 3.14 Example: Third step of setting up the ARC diagram for the project on 'Analysis and support for communication throughout the design process'

4. Indicate which of the areas seem *most relevant* to your work. Distinguish between essential areas and useful areas, *e.g.*, by colouring or hatching as in Figure 3.15.

   - As to which areas are most relevant may change during the course of the project. Similarly, new areas and disciplines may have to be added as understanding increases, and existing ones may have to be removed if they lose their relevance.

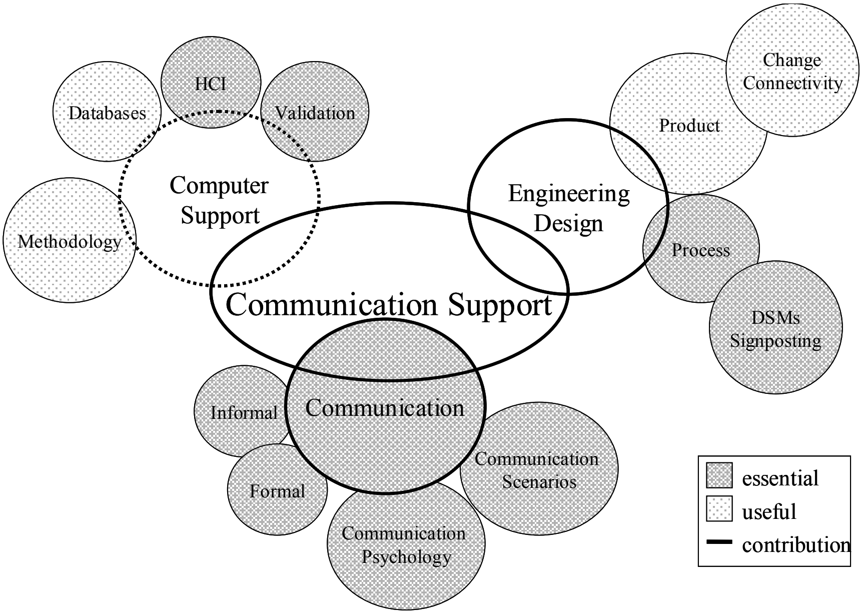

Figure 3.15 Example: Final ARC diagram for the project on 'Analysis and support for communication throughout the design process'

5. Indicate the area(s) to which the research project will *contribute*, *i.e.*, the area(s) in which the research is expected to make the biggest changes. Highlight these areas by, for example, thickening or colouring their borders, as in Figure 3.15.

   - Theories, models, findings and methods from various areas will be relevant, but it is possible to contribute to only few of them. For example, in a project on capturing rationale using a computer-aided design (CAD) system, it might be essential to look into the area of databases, and to use this as a basis for developing the support. However, the contribution will be in the area of engineering design, not in computer science; a new database might have been developed, but probably not a new database concept.

Other graphical representations can be used than the one presented here, such as MindMap™.

3.7 Formulating Overall Research Plan

The final step of the RC stage is the formulation of the Overall Research Plan for the project.

3.7.1 Overall Research Plan

An Overall Research Plan should include the following:

- research focus and goals;
- research problems, main research questions and hypotheses;
- relevant areas to be consulted;
- approach (type of research, main stages and methods);
- expected (area of) contribution and expected deliverables;
- time schedule.

The deliverables are the intended outcomes from the various stages of the research type chosen, listed in Section 2.6.

It is important to note that an initial plan is better than no plan at all; it provides a direction for research, a yardstick for measuring progress, and a sense of achievement to carry on beyond this stage.

The time available for a research project will be constrained by the possible duration of the project and the number of people involved. Since a detailed plan requires knowledge of the specific research questions to be answered, the plan cannot be made very concrete at this stage: the questions to be answered in a particular stage depend on the outcome of the previous stage. However, the results of the RC stage as proposed thus far in this chapter, provide a reasonable indication of the scope of the project, its main stages, and the type of research methods required to address the research problem.

The following chapters provide more information about methods available for carrying out each stage. It is useful to read these chapters before drawing up the research plan. The types of research method give an indication of the required resources and help develop a fairly realistic overall time schedule for realising the research goals. The plan should be monitored, modified and refined on a continuous basis as understanding increases during a research project and unforeseen circumstances and outcomes can occur.

A possible way of representing the aims or questions and hypotheses of the research project against the stages was developed by one of the PhD students who attended our summer school. He aimed to answer the following research questions (Eriksson 2007):

- What noises and enablers are there in product development decision making that effect project performance, and how? (Q1)
- How does decision maturity effect project performance? (Q2)
- How can the decision-making process be supported to continuously increase project performance? (Q3)
- What additional noises and enablers are there when product development projects are distributed and how do they affect project performance? (Q4)

- How can a distributed decision-making process be supported to continuously increase project performance? (Q5)

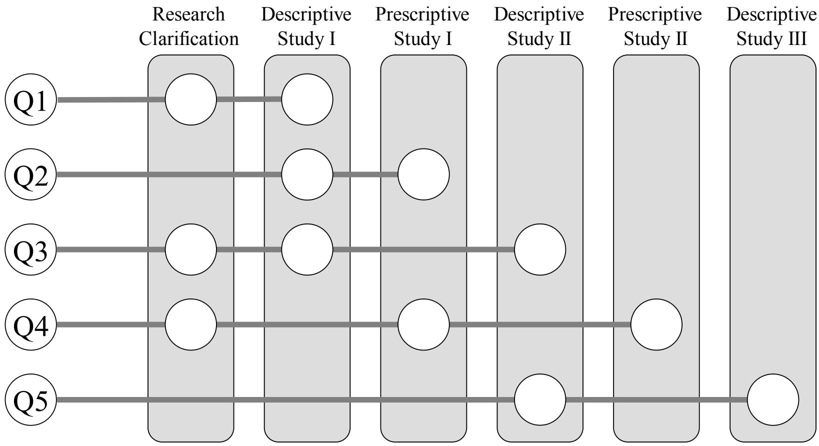

Figure 3.16 Aims against stages, after Eriksson (2007)

3.7.2 Visualisation Exercise

A particular problem we noticed is the ability to communicate the ideas about the deliverables. Even in an early stage, most researchers will have an image of what they want to achieve. However, this often remains implicit, making it difficult to judge the viewpoint and assumptions underlying the identified problem and research plan. The so-called Visualisation Exercise, proposed by Mogens M. Andreasen, provides a very good technique to make ideas and views explicit by visualising the concepts or outcomes mentioned in the research plan, using models, schemes, scenarios, and other graphical means, and to reflect on and discuss the result. In this early stage, it is useful to consider variants of these visualisations to avoid fixation on an initial idea. This exercise should be used in all stages of the research project. Note that the Impact Model shows the impact of the outcome, but not the intended outcome (the support) itself. The visualisation exercise is intended to show the intended outcome.

An *example* illustrates this exercise. A student planned to develop a methodology specifically for designing sports equipment. However, it was not clear from his description how sports equipment and its design is different from other products and design processes and hence requires a special methodology. He was therefore asked the following two questions in order to visualise his assumptions and views:

- Mention a typical sports equipment that has built-in medical, ergonomic, anthropometric, *etc*., knowledge, as this is what you said differentiated sports equipment from other products.

- Try to model how these peculiarities have influenced project strategy, team manning, process (special plans or activities), criteria, organisation, *etc*., in ways that are different from traditional ways.

The visualisation did not show the differences with other design processes. It became clear that there was not sufficient understanding about the typical types of knowledge required for developing sports equipment, and where and how these types of knowledge and the domains involved influence the process. The conclusion that a new methodology was required, seemed premature, and the research focus should be on gaining this understanding.

Other examples of visualisation requests that we have given to students are:

- Show an example of the content of your model of what you call ‘product assortment dispositions’, and show in a scenario how this understanding is used: by whom, for what, in what situation.
- Illustrate a family of products, their commonality and their variations. Illustrate how you can reason ‘cross-family’ concerning manufacturability and life-cycle costs. What are actually life-cycle costs? What insight about the product life do you need to have in front of you?
- Show an example of data merging from different domains and how this would tell about what you call ‘maturity and assembly capability’. If possible, show what more is known if higher maturity is achieved.
- Your guideline on patient safety will contain some type of system model that shall instruct, motivate and orientate the guideline user. This model may become the essential part of your research. Show us such a guideline!
- The ‘form development process’ of automobiles may be seen as a combination of an industrial design process focusing upon appearance (controlled by quality of form, reliability of form, *etc.*) and other processes, mainly technical. Make an activity model showing these parallel processes, to show what goes on in the industrial design processes and the other processes, and to show their goals or criteria. Show where in this model your framework shall operate.

As these examples show, the visualisation questions focus on the terminology and concepts used. The true content of these often remain implicit. Having ‘more knowledge’ about maturity, *e.g*., does not show what sort of knowledge this entails, or at which level of detail. Similarly, a ‘guideline’ may be very generic or very specific. In this stage it is not possible to exactly know the outcomes, but there has to be a vision about the sort of outcome that is expected, knowing very well that this vision might not be correct. Making it explicit at least allows discussion about the vision.

3.7.3 Reflection on RC

Before the RC stage is completed, the following checklist may be useful to reflect on the deliverables of this stage:

- Why do you ask this research question?

- Why do you believe this is a relevant research question?
- Why do you believe that you have or can obtain the competences to answer the research questions and solve, or contribute to solving, the identified problem?
- Where do you believe that you can be original, *i.e.*, your results can bring a contribution to practice as well as to knowledge?
- Why do you believe your Overall Research Plan leads to a result?
- Why do you believe your work is scientific?

Oliver (1991) provides some heuristics of how to recognise an important contribution to science. One is that the contribution will significantly change the ways of thinking or working of others in the area. A researcher can recognise whether their contribution is important, amongst others, by checking if their results not only evaluate favourably to their own data but conforms well to other well-known information, or even more, if it can relate data previously unfamiliar to the researcher. The more diverse and numerous the compatible but previously unknown data is, the greater the chances are of the discovery being a major one.

3.8 General Guidelines on Doing Research

Many authors stress that researchers should have independent judgement and that they should be optimistic. They should nurture concentration – a sustained orientation of all faculties toward a single object of study (Ramon y Cajal 1999). A researcher needs to have devotion to truth and a passion for reputation for being able to discover the truth (Oliver 1991; Ramon y Cajal 1999). As research can often be a long and lonely activity fraught with failures; having enthusiasm for the work and an ability to "enjoy the struggle, not the spoils" is thus essential (Oliver 1991). Patience and observational abilities are often critical in scientific inquiry (Ramon y Cajal 1999).

Acts of creation, which includes research, are opportunistic in nature (Dasgupta 1994). It is important to be flexible (all within limits, of course) and opportunistic, that is, to have the courage to pursue promising, unexpected avenues opened up during research that may lead to exciting new solutions, even though they may not necessarily fit into the existing research goal and plans (Dasgupta 1994). The creative agent is not only knowledge rich, but is able to wander freely about the knowledge space and retrieve whatever seems to relate to the goal at hand (Dasgupta 1994).

Serendipity – accidental discovery – is commonplace in science, but only to those "whose minds are prepared for it" (Pasteur in Root-Bernstein (1989)), *i.e.*, those who show curiosity and perception. Serendipity can be encouraged in the following ways.

- Searching, assimilating and using a wide range of knowledge: creation, at least in the sciences, is knowledge intensive (Dasgupta 1994) and as we discussed it is important to look into, not just the publications of direct interest for the work at hand, but also others.

- Making and recording expected and unexpected observations (Lenox 1985).
- By maintaining flexibility in thinking and interpretation (Lenox 1985). Adams (1993) enlists various kinds of mental blocks that typically prevent us from thinking flexibly, and approaches of how these could be avoided.

Notwithstanding the importance of serendipity: large insights are composed of a possibly intricate but describable network of small steps (Dasgupta 1994). Doing research means working hard and meticulously, rather than waiting for the big moment of inspiration to arrive.

While it may be immensely beneficial to “learn from the masters” (Truesdell 1984), it is important not to be over-impressed by the work of predecessors (Oliver 1991; Ramon y Cajal 1999); this does not mean being disrespectful to other researchers, but being not too reverent to their work. It is important never to believe an hypothesis, law or principle completely, especially because this can lead to indoctrination, and blindfold one from the truth (Oliver 1991). One should beware of pursuing sophistication for its own sake, as this is both a distraction and a waste (Oliver 1991). This is not to say sophistication should not be pursued, but that it should be pursued only when required by the overall purpose of the enquiry.

It is particularly important to have a strong inclination toward originality (Ramon y Cajal 1999); alternative explanations should always be entertained, and evaluated against the yardstick of observation. Often it is useful to think like a child, and to force oneself to see things in a different light (Oliver 1991; Root-Bernstein 1989). Innovators and discoverers often reason by analogy (Oliver 1991). Especially when probing into the unknown, this can be very powerful. An hypothesis need not have its origin in facts or observations, although it eventually has to be validated by these. Speculation and subjective thinking for generating ideas should not be discouraged by over-critical annihilation of initial, bright ideas. “It is often more difficult to identify what is right in an idea than what is wrong in it”, and “one must see the important features of an imperfect idea rather than totally discard it” (Bligh 1990).

There are a number of caveats that are often advised to be avoided. One is the excessive use of jargon (Oliver 1991), which substantially hinders communication, especially in interdisciplinary areas like design research. Furthermore, “There is no limit to what you can accomplish if someone else gets the credit” (Oliver 1991): passing credit freely to whomever and wherever it is due, is both polite and essential in areas like scientific enquiry where knowledge is built by successive addition. Pretence should be avoided at all costs as it is dishonourable and sooner or later it will be detected if the matter is of sufficient interest. If multiple alternative explanations exist, ‘Occum’s razor’ – the principle that the simplest explanation is the best – should only be used when all observations on the matter are considered (Oliver 1991). Returning from time to time to the basic principles and laws in a particular area, is one way of staying on course.

Tunnel vision, *i.e.*, remaining fixated on a single solution or explanation, should be avoided by considering a range of alternatives and evaluating these. As Pauling suggests (N.N. 1977) “Just have lots of ideas, and throw away the bad ones”. The ability to speculate is particularly important: “Be as bold in the conception of the hypothesis as rigorous in their demonstration” (Darwin and Richter in Root-

Bernstein (1989)). Perhaps while suggesting answers, one should not be too reasonable. Freeman Dyson, quoted in Root-Bernstein (1989), claims that "for any speculation which does not at first glance look crazy, there is no hope". However, this craziness should not be haziness (Root-Bernstein 1989). Therefore, the wilder the idea, the better they must be anchored by the accepted methods (Monod 1969). As Fermi, quoted in Root-Bernstein (1989), suggests: "only those guesses should be followed which define the answer to a problem, even if non-specific".

3.9 Main Points

The main points of this chapter can be summarised as follows.

- This chapter provides an approach and methods to support the early stage of a design research project or programme: to identify and refine a research problem, and set up an overall plan for carrying out research for solving this problem.
- This stage has six, iterative steps: identifying the overall topic(s) of interest, clarifying the current understanding and expectations, clarifying criteria main questions and hypotheses, selecting type of research, determining areas of relevance and contribution, and formulating the Overall Research Plan .
- There are three central issues that constitute an overall topic of interest: issue of interest, activity in or stage of the design process, and area of application. The topic may come from researchers, sponsors, research community or a combination, based on the belief this topic has an effect on design practice, although concrete evidence may not exist.
- In design research, the goal is to identify and solve problems of interest that have a degree of generality and application across products and practices. For a topic to qualify as a research area, it should be academically and practically worthwhile, as well as realistic.
- It is important to gather available information about the topic through an exploratory the literature review, and to make the expectations, beliefs and underlying assumptions of each stakeholder explicit, in order to obtain a first shared picture of the existing and desired situations and the Success Criteria. These are documented in the Initial Reference and Impact Models.
- The Initial Reference and Impact Models will indicate the focus of the research, whether this has sufficient research potential, what type of research would be suitable, and the criteria against which the research outcomes should be judged.
- Different entry points are possible. Research could start with: a descriptive goal of understanding a situation, a prescriptive goal of support development, or a descriptive goal of support evaluation.
- Irrespective of the entry points, initial models of both the existing situation (the Initial Reference Model) and of the desired situation (the Initial Impact Model) including the preliminary Success Criteria are necessary to clarify understanding and expectations and select the type of research.

- The exploratory literature review will lead to extension and adaptation of the Initial Reference and Impact Models, which were mainly based on beliefs and expectations. It is particularly important to find supporting evidence for the central assumption that applying support will have the desired effect on practice. There should be a link between the factors of interest (Key Factors) and success (represented by the Success Factors).
- Success Criteria usually remain static throughout a project; however, Measurable Success Criteria often do not. As a research project progresses, Measurable Success Criteria become more precisely defined and may change.
- Determining criteria and topic is an iterative process. Topic and criteria may be redefined several times before they are clear and well connected.
- Research questions and hypotheses can be derived from the Initial Reference and Impact Models. A research question is a question for which no answer exists yet. An hypothesis is a tentative, refutable answer to a research question in the form of a relationship between two or more concepts – or factors, in our definition.
- To determine the type of research to be undertaken, *i.e.*, on which DRM stages the research should focus, depends on the current state of the research. If for a particular stage, results are available, a review of the state-of-the-art is sufficient. If not, a comprehensive study is required, where results are substantially the researcher's own findings.
- Based on combinations of these possibilities, seven types of research are identified covering individual research projects as well as research programmes: four of these types focus comprehensively on one DRM stage only. The rest focus comprehensively on two or more stages.
- In the research project it is necessary to consider all potentially relevant areas, not only those related to one's own topic or discipline. It is important to consider a wide range of areas and disciplines.
- To identify areas of relevance and of contribution, these questions can be asked: what areas relate to the topic, how relevant these are (essential or useful), and in which of these the researcher's contribution is most likely to be. The area of contribution should be compatible with the researcher's expertise and project goals.
- A so-called ARC diagram helps represent the answers to these questions and is a good basis for discussion and reflection.
- An Overall Research Plan should include following: research focus and goals; research problems, main research questions and hypotheses; relevant areas to be consulted; approach (type of research, main stages and methods); expected (area of) contribution and expected deliverables; and time schedule.
- Since a detailed research plan needs knowledge of the exact research questions, the initial Overall Research Plan created in the RC stage cannot be very concrete. However, it still provides a direction for research, a yardstick for measuring progress, and a sense of achievement to carry on beyond this stage.

- The RC stage provides a good indication of the scope of the project, its main stages, and the type of research methods needed to address the research problem. The plan should be monitored and refined continuously as understanding increases.