A1 Principles of Good Research

In principle, there is no wrong or right research. Likewise, there are no wrong or right theories, nor wrong or right methods. There is only good, mediocre or bad research. Good research selects the most appropriate theories and methods for the problem at hand. It links methods to theory and problem definition to problem. Bad research claims more than can be done on the basis of the methods used, or chooses overly quantitative methods for a primarily qualitative problem. Good and bad research in the internet age also applies the rules of 'information literacy': the ability to know when information is needed, to be able to find the information, to evaluate the information and to use the information that has been found (Bothma, 2008) (➔The Challenge). *Always* take the following ten basic rules of good research into account.

1 Dare to build upon the research of others.
2 Dare to make *motivated* choices.
3 Always define the most important concepts.
4 Explain flaws in the research.
5 Make a clear distinction between analytical and normative judgment.
6 Strive for the highest possible integrity.
7 Be critical and creative.
8 Good research is disciplined and realistic.
9 Open up: share failure!
10 The synthesis challenge: creating relevance.

1 Dare to build upon the research of others

Many research questions have been addressed before. And most social science problems are very complex. Each individual researcher is, in a way, an intellectual 'dwarf'. Be brave enough to admit this. You can stand on the shoulders of giant predecessors and if you are also brave enough to take previous ideas into account, you will have a splendid view of the research problem at hand. Science is intended to be cumulative. In the early stages even of a small research project, get an overview of the most relevant approaches (➔A12). Very often there are good textbooks that can give you this overview in a few hours. If you are an inexperienced researcher ask a supervisor, a tutor or even the librarian for clear and *committed* guidance (➔B15).

2 Dare to make *motivated* choices

Research is a continuous choice process. For every problem there is an abundance of solutions, depending on the stakeholder, time and place (➔A11). Likewise, a large number of equally valid approaches, methodologies and perspectives exist. You have to dare to make choices; otherwise nothing will come of your research. The worst thing you can do, besides not daring to make choices, is *not* specifying choices. Revealing *all* your choices is a minimum requirement for a good research project. It enables other researchers to repeat your research steps, making your research results as objective as possible. Basically this is what distinguishes the *scientific method* of research from other methods, for example, those of journalists and often those of consultants (Emory and Cooper, 1991: 15). Ideally, *each one of these choices is explained in the introduction of the research report*. Table A.1 shows the basic choices that you need to make in any research project.

Table A.1a Research choices

Topic	Choice and motivation
The problem	Choice of a problem definition: why have you defined the problem like this?
Research aim	☐ basic ☐ applied ☐ action-orientated ☐ evaluation (➔A2, A3)
Relevance pursued	☐ scientific ☐ societal ☐ managerial ☐ practical ☐ theoretical ☐ a combination
Level of analysis	☐ micro ☐ meso ☐ macro ☐ meta ☐ a particular combination
Theories	If available, make a choice from at least **three** related approaches.
Methods	Specify your choice for one or a combination of methods, e.g. by stating the strengths and weaknesses of each method.
Stakeholder perspective	Whose perspective do you want to take into account in this research project? Make your choice of a particular actor (manager, trade union, government) clear. Be selective.
Sources	What kind of sources did you search for in particular: ☐ primary, ☐ secondary, or ☐ tertiary? What are the strengths and weaknesses of these sources? (➔A11)
Audience	To whom are you addressing your research? Is your audience the same as your stakeholder? Or do you consider the research project valuable for others as well?

3 Always define the most important concepts

Social sciences focus very much on argumentation. Words and concepts that form these arguments can have many meanings. The dictionary definition of a concept is often inappropriate for research, because the dictionary is too general and frequently gives definitions based on circular reasoning. Use definitions from the relevant literature and explain your choice of a particular definition if more than one definition exists (which is often the case). Make the definition operational, i.e. understandable and open to testing. Remember: definitions do not usually develop in isolation. Always consider the context in which a definition is introduced.

4 Explain flaws in the research

If you explain the choices that you have made, you should also make clear the flaws in the research design that you have chosen (➔A16). You should include this information in your conclusion. Do not leave this for the reader to discover. If you do so, it will not only lessen your credibility as a researcher, but also reduce the credibility of your research (see also point 6). If you come to the conclusion that you have chosen a methodology that is not appropriate – although often used in comparable research projects – you should not necessarily have to begin your research again. The fear of having to start again is a mistake that many students make and sometimes results in failure to reveal the choices they have made. To conclude that a particular methodology is not useful can also be an important result of your research effort. Your research aim has only changed to one of lesser advance (➜A3).

5 Make a clear distinction between analytical and normative judgment

Avoid the inclination of many researchers to come to prescription on the basis of a weak or very limited description of empirical phenomena. Conclusions can *only* be based on the results of the research. A limited research project can only lead to limited conclusions. Always specify the conditions under which you think your research results hold true. Since you have stated this in the earlier parts of your research, your conclusion should be the logical end result of your research. *Speculating* on the basis of your research results could be valuable and interesting, but can only be done after the conclusions, and should be explicitly stated as such.

6 Strive for the highest possible integrity

Every researcher should be aware of the ease with which data and figures can be manipulated (➜A12, A16). You frequently make use of data collected by others, for instance, often at high levels of aggregation. So you should adopt a high level of integrity when assembling qualitative and quantitative data. Likewise, always take into account the possibility that data and arguments can be manipulated. The more the researcher uses the criteria for good research, the higher the credibility of the research report. Integrity is valuable even for short research essays. Work on your reputation as a reliable researcher. You should demonstrate your experience of doing research in the research project at hand. If you are inexperienced, state your personal motivation for doing the research in the preface of the report (➜E3). Research integrity first deals with a number of simple rules of thumb (no fraud or plagiarism). But in practice you are faced with a number of more subtle pitfalls that can nevertheless seriously affect your research integrity (➜A16).

7 Be critical and creative

Good research is critical research. A good researcher is not afraid to ask tough questions. Never stop asking the 'why' question: Why is this problem a problem? Why should I choose this particular approach for tackling this problem? Why do I see what I see? As children ceaselessly ask their parents the 'why' question, researchers should be capable of doing the same with the societal problems that they want to address. A skilled researcher should be capable of expressing doubt and asking 'why' questions without annoying the recipient of the research question (as is so often the case with parents and children). As René Descartes already stated in the early 17th century, 'doubt is the basis of wisdom'. Research refers to the effective management of doubt. Apply the ten basic rules of critical thinking (as listed in the box). Critical research sometime necessitates developing creative research approaches, but only when the traditional methods do not hold (➔A4).

Ten principles of critical research

1 Continuously ask the 'why' question (at least three times in building up an argument).
2 Be sceptical of the reliability of sources.
3 Always know the background (either editorial, personal or otherwise) of your sources.
4 Think! Evade obvious questions.
5 Prepare!
6 Always question arguments.
7 Always check the appropriateness of quantitative data.
8 Be realistic about what you can achieve as an individual researcher.
9 Make sure that your research can be replicated and your hypotheses – potentially – refuted (falsified).
10 Be modest regarding what you can know as a subjective human being.

8 Good research is disciplined and realistic

Managing doubt is not easy. You may hope to be inspired by the aims you set for yourself (➔A3). But always keep in mind one final 'rule of thumb', which is applicable to most research projects even if you realise that this rule suggests an exactitude that is difficult to obtain in most real-life research projects:

Good research is the result of 80 per cent perspiration and 20 per cent inspiration.

9 Open up: share failure!

'Innovators don't fail, they learn.'
David Taylor, CEO of Procter & Gamble, Twitter, 12 July 2017

We tend to learn more from failure than from success. But research projects often concentrate on 'best-practices' or success stories. One reason for this is that research objects (firms, people, policy makers) tend to open up more to researchers that are interested in the sources of their success than the sources of their failure. Or in case of failure, they hire a consultant that promises not to publish the results of the research. The fear of exposure is considerable. But the scientific community is also afraid of failure. For instance scientific journals favour publication of 'successful' experiments or confirmation of 'what works' studies rather than publication of those studies that did not find any correlation. It was found that 65 per cent of social sciences studies with a negative outcome were not even shared with others. In medical research less than a quarter of the published findings could be reproduced. Comparable results were found in psychological research (Open Science Collaboration, 2015). There are hardly any public platforms for failed studies or to share research that did not provide interesting results. No results, however, might also be an important result. Besides the selection bias and confirmation bias that this creates (➔A16) implies that many other researchers will – unknowingly – repeat the same mistakes. Or that they will not engage in 'risky' research that involves for instance thinking outside of the box (➔A5) or thinking in terms of 'paradoxes' (➔A6). As a result research becomes a 'low-risk' activity, but probably also with less relevance. Sharing your failure(s) then becomes part of a research integrity challenge (➔A16), whereas taking up more risky research becomes part of addressing more complex societal problems (➔The Challenge; A12). There are many initiatives underway that aim at sharing information on research projects through open source. Open source lowers the costs of failure by sharing information. This requires an open and collaborative mindset of researchers as well as researched.

Learning from Brilliant Failures: inspiring initiatives

- ✔ **The Institute for Brilliant Failures and the Brilliant failure award: http://www.thnk.org/institute-of-brilliant-failures/**
- ✔ **Fuck Up nights: organised in 252 cities spread over 80 countries: https://fuckupnights.com/**
- ✔ **The museum of Failure opened in Stockholm and shows failed products: https://www.museumoffailure.se/**

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10 The synthesis challenge: creating relevance

The third edition of the *Skill Sheets* explicitly confronts its users with a challenge: how to combine cool heads, warm hearts and productive hands (➔The Format)? This ambition presents an individual challenge for each researcher. For research skills the challenge is a classical one: how to combine scientific 'rigor', 'relevance' and 'valorisation' (the applicability) of a research project. A 'cool head' then boils down to selecting the best possible theories and methodologies to address a problem – which can be referred as the principle of triangulation (➔A12). This principle also implies that the researcher accumulates all possible scientific insights (➔C5, C6). A 'warm heart' involves a search for relevant questions and 'real problems', which in practice implies that the researcher does not shy away from complexity. 'Productive hands' finally takes the practical relevance (valorisation) of the research into account. This is of course one of the most heavily debated topics in fundamental/basic science: should a researcher always focus on practical questions or aim at direct applications? The answer is no, not in the least because fundamental research in the past ('research for researches sake') has regularly resulted in revolutionary (out-of-the-box) applications and consequences. So the synthesis challenge provides not an 'either-or' (dilemma) challenge but an 'and-and' (paradoxical) challenge (➔A6). Actionable research is one of many possibilities to improve the practical relevance. Various combinations can be established. For instance the saying that 'nothing is as practical as a good theory'. The search for a synthesis presents the researcher with a combination of three types of relevance:

scientific, societal and practical. For each research topic, the synthesis challenge will be different.

Table A.1b The Synthesis Challenge of Good Research

Research	Cool head	Warm heart	Productive hands
Research orientation	Rigor	Relevance	Validity and 'valorisation'
Possible practice (→A7, A11)	Triangulation: work from multiple perspective	Consider wicked problems and paradoxes	Engage in action research
Thinking hat - boxes	Bigger box	Better box	Inside the box
Thinking hat (→A6)	Trade-offs (deep thinking)	Dreams, dilemmas (intuition, fast thinking), 'personal'	Puzzles (given boundaries)
Search for relevance	'Scientific'	'Societal'	'Practical: organisational/ management'
Possible syntheses	Creating societal relevance through scientifically founded research that can be applied in practice Thinking hat: outside-the-box (→A4) Think in paradoxes (→A6) Co-create research projects with stakeholders (→A11) Create open source science and publication platforms		

A2 Research Aim: Basic or Applied?

The type of research project you want to undertake depends as much on the problem you address, as on the type of researcher you want to be or become. What are your aims in a research project? This is *always the first question* that you should ask yourself when beginning a research project. Two general research aims can be distinguished: basic and applied research. A basic research project is aimed primarily at understanding the problem at hand. An applied research project focuses more on the outcome of the problem, and the design of possible solutions.

Aims of basic research

- **Aim 1: Problem definition**. This type of research deals with semantics and philosophy. You ask questions like: 'What is the *nature of the problem* I would like to address?' 'Which areas of research are involved?' 'Which *keywords* and *concepts* should be considered?' You often need to go beyond the problem experienced by the people who are directly involved, and find out the context of their problem in order to formulate the appropriate problem definition.
- **Aim 2: Diagnosis**. This type of research aims at analysing all the ins and outs of a particular problem. You ask questions like: 'What *causes* the problem to appear?' 'Why is there a discrepancy between the actual and the desired situation of the actors involved?' 'What level(s) of analysis should be considered, in order to find the cause of the problem?' 'Which theoretical approaches (lines of argument) and methodologies are available to analyse this kind of problem?' You can also ask for the *consequences* when the problem persists.

Aims of (more) applied research

- **Aim 3: Design**. This type of research aims at giving advice, but from a distance. You address questions like: 'What kinds of solutions have been developed for this problem?' 'What effect did they have?' 'What other solutions are available for this problem?' 'Is there a better solution?'
- **Aim 4: Implementation**. This type of research aims at active intervention. It is also known as 'action' research. You ask questions like: 'How can a designed solution be put into practice?' 'What are the other possibilities?' 'What could the function of the researcher be in this process?' 'At what points in the process of change should the researcher intervene?'
- **Aim 5: Evaluation**. This type of research not only aims at active intervention, but also at revision of the design if the proposed solution was not successful. You ask questions like: 'How effective has the proposed implementation been?' 'Does the chosen implementation create additional problems?'

A research project can focus on just one of these research aims. For example, if you aim at researching questions of implementation, then you leave the design problem and the problem of definition to others. These 'others' might be other researchers (thus creating a division of labour between researchers) or the actors themselves. These objectives represent a sort of sequence of research goals or aims.

As such, the five aims of research can be grouped together as the five consecutive phases of an ideal research project. Each step accumulates information about the solution to a particular problem and each step provides the input for the next step.

Tension between description and prescription

The biggest challenge for applied research projects is dealing with the tension that exists between problem and design (Figure A.2). To be more specific: a tension exists between the reflective circle for the researcher and the questions posed by the client. Clients ask researchers to give them an answer to the question, 'Is this correct?' or 'What am I doing wrong?' The second question that is often put to the researcher is, 'How would you do it?' Thirdly, clients ask, 'Could you come up with alternatives on the basis of the experience of other organisations?' Basically, clients use the same reflective circle of research and research questions as the researcher but they go in the opposite direction. In the introduction to this Skill Sheet collection this was referred to as the 'adviser's disease'. It boils down to the researcher being too eager to please the customer at short notice, and often deviates the researcher from the basics of good and critical research (➔A1). A good researcher is acutely aware of this tension. Sometimes, aiming at good (basic) research requires a researcher to search for the 'right' customer. Otherwise, the tension becomes too great.

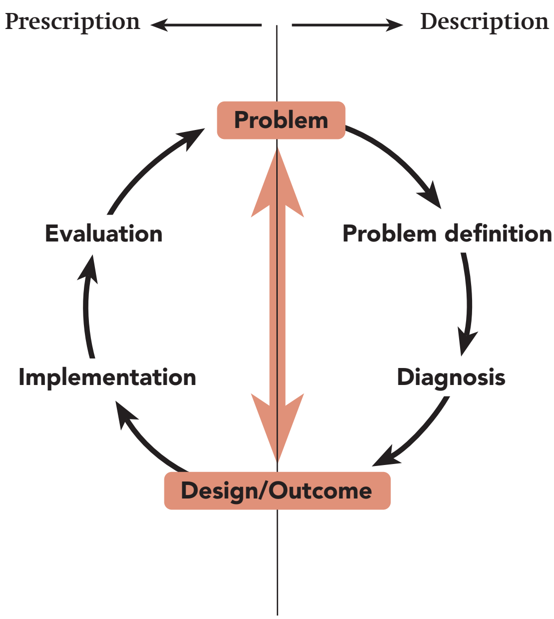

Figure A.2 The tension between prescription and description

Not all steps of the reflective cycle need to be performed by the same researcher. This is what forms the labour divisions between the various scientific disciplines. A too rigid division of labour, however, has often resulted in 'sketchy research'. Every researcher should be able to master all elements of the reflective cycle. But – depending on your personal preference – not all stages need to be carried out with the same intensity. Neither do all steps have to be sequential: you can begin with an implementation problem, and then ask yourself what the consequences might be of the problem definition by the actors involved. In focusing on implementation problems you can build on the description of the problem offered by other researchers. Building on the work of others is common in large organisations, particularly for strategic management researchers. But you have to be aware that the more you take the choices of others for granted (and refrain from applying the first steps of the circle seriously and systematically), the greater the chance that the results of your research will not address the real problem.

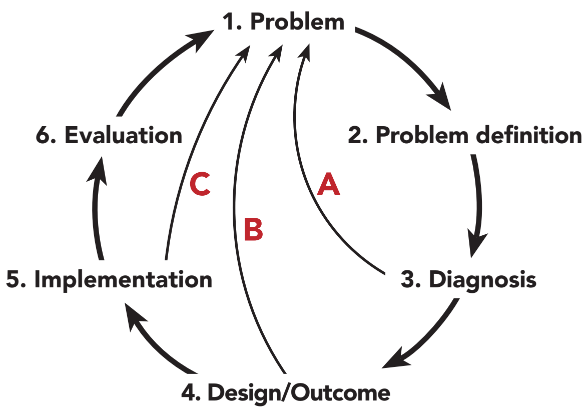

Figure A.3 Reflective cycles for individual researchers

You can go through the cycle in various ways. Fundamental (basic) researchers often focus their attention primarily on the first and most descriptive steps of research (cycle A). They aim at a better understanding of their research object. Systematically coming to a better understanding is what is known as the *heuristic value* of research. More applied (prescription) orientated researchers place less emphasis on the first two steps and move quickly into the B or C cycles. There are, however, never real shortcuts. Even applied researchers have to grasp the problem first. A way to go through these phases of research projects is, for instance, to build further on existing theory and check whether it is applicable to the problem at hand.

When you are familiar with the different research aims and the various cycles that you can go through, you can decide what kind of researcher you would like to be. Taking the reflective cycle into account, there are five ingredients for a personal profile at your disposal. Table A.3 enables you to decide what your preferred profile looks like, by posing a number of critical questions. If you are inclined to answer the question(s) in the affirmative, include this element in your profile.

Table A.3 What kind of researcher do you want to be? A personal check-list

Personal profile	Critical questions
1 'Conceptualiser'	❑ Do you refuse to take the problem definition of the actor, who commissioned the research, as given? ❑ Are you interested in research problems in general?
2 'Desk researcher'	❑ Are you interested in finding out the real source of the problem you are addressing? ❑ Do you want to go beyond the level of analysis of the actors directly involved? ❑ Do you prefer to do research independent of your research object?
3 'Designer'	❑ Are you interested in designing scenarios to tackle the problem at hand? ❑ Are you interested in designing solutions for a (perceived) problem?
4 'Implementer'	❑ Do you want to intervene in the research object? ❑ Are you interested in designing acceptable and feasible solutions?
5 'Involved evaluator'	❑ Do you want to be committed to the organisation that you do research for? ❑ Do you want to become an active internal lobbyist for the solutions proposed?

A good researcher has an affinity with, and knowledge of, all five research functions. You will probably answer many questions in the affirmative. But to avoid losing your way due to a lack of priorities, you will have to specify your preference for a limited number of research profiles. Your priority aims can change from research question to research question (➔A14, A15).

If your research profile looks like:

- 1,2,3,4,5: You want to be a 'basic' researcher!
- 1,2,3,4,5: You want to be an 'applied action' researcher.
- 1,2,3,4,5: You want to be a 'critical action' researcher.
- 1,-,3,4,5: You want to be… yes… what do you want to become?
- 1,2,3,4,5: You lack (research) priorities.
- -,-,-,-,-: You have a problem!