Why a methodology for practice-oriented research is a necessary heresy

FAREWELL ESSAY BY PROF. PIET J.M. VERSCHUREN

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In the last few decades, practice-oriented research has become important in society as well as in the social, policy and management sciences. However, as it was to a large extent developed during the second half of the last century, existing research methodology bears all the hallmarks of science in an ivory tower, focusing on knowledge for knowledge’s sake. I will argue that a shift in the context of discovery requires a follow-up in the context of justification. What’s more, we need to develop a methodology for the contexts of discovery and implementation, and one that is capable of addressing ethical validity in designing and evaluating practice-oriented research. Contrary to conventional wisdom, a focus on practice-oriented research can be reconciled with the task of building theories in science.

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WHY A METHODOLOGY FOR PRACTICE-ORIENTED RESEARCH IS A NECESSARY HERESY
To my father and mother
Why a methodology for practice-oriented research is a necessary heresy

Farewell speech (in shortened form) delivered on Friday the 4th of September on the occasion of his departure from the Faculty of Management Science at Radboud University Nijmegen

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PREFA CE

This essay was written on the occasion of my farewell on leaving Radboud University Nijmegen in the Netherlands. Part of the text was presented in my farewell lecture on September 4, 2009 entitled: Methodologie voor praktijkgericht onderzoek; een noodzakelijke ketterij. (A methodology for practice-oriented research; a necessary heresy). This lecture was the final piece of a symposium with the title: Praktijkgericht wetenschappelijk onderzoek: ketterij of noodzaak? (Practice-oriented scientific research: heresy or necessity?). When Joan van Aken from the University of Eindhoven in the Netherlands – one of the keynote speakers – coined the term heresy for the title of this symposium, I immediately agreed. This despite the fact that it may look presumptuous and can be dangerous; the heresy of today may easily become the blind doctrine of tomorrow. But its implicit reference to a doctrine may help to alert scientists to the neglect of practice-oriented research as an independent scientific discipline.

In this farewell essay I would like to thank a few people and departments who have played an important role in my academic life. First of all, I would like to express my gratitude to the Radboud University of Nijmegen for offering me a place to work for almost forty years. For me it was a safe working environment, imbued with academic values. I would also like to mention Wageningen University and Research Centre, where I had a second chair in the period 2001-2005, and where I learned what no-nonsense science means.

I'm grateful to Jac Vennix. I could not have had a better colleague. He and my colleague, the dedicated Hubert Korzilius assisted me in organizing the symposium with their ideas, networks, emotional and financial support. And together with my excellent colleague Inge Bleijenbergh, they also edited an intriguing book on the occasion of my departure entitled: Voer voor methodologen (Food for methodologists). Several contributions to this volume relate to issues that I once planned to scrutinize in the future. The authors have now done it for me. It's fascinating, and illuminating to see how they perceive and elaborate on these intriguing subject matters.

I would also like to thank the other colleagues in the Methodology Section, with whom I worked for the last eight years. They not only put a great deal of effort into the book I just mentioned, despite their intensive education programme. They also provided me with a warm working environment, where collegiality was natural. As to the book, it stands to reason that I should include in my thanks all those from outside the Methodology Section and Radboud University who contributed to it.
Finally I would like to express my gratitude to the inimitable Netherlands Institute for Advanced Study in the Humanities and Social Sciences (NIAS) at Wassenaar in the Netherlands. This internationally respected institute twice offered me a fellowship. Here I became acquainted with renowned scientists, several of whom became friends. I mention Theo Kuipers, Walter Müller, Gerhard Hirschfeld, Laszlo Zsolnai, Peter Swanborn, Mineke Schipper, Giora Hon, Martijn Berger and Douglas Hershey. They, together with the Institute with its excellent intellectual climate, warm social environment and unsurpassable support of the staff, greatly contributed not only to my productivity, but especially to my education as a scientist.

Malden, November 2009
'But it [methodology; pv] is simply a matter of persevering! Without hard work there is nothing, except possibly methodological junk food, satisfying for the moment but leading to malnutrition in the end...' (Alvesson & Sköldberg, 2000)

**INTRODUCTION**

For a long time research in the social, policy and management sciences was theory-oriented, characterized by the production of knowledge for knowledge's sake, without a specific external goal. The search for knowledge without any profit was regarded as the best guarantee for finding valuable insights, which sooner or later would appear useful to society and its members. For traditional scientists practice-oriented research is a sin. However, nowadays this type of research holds increasing interest for universities. First of all, the importance of the social and political relevance of scientific research is growing in general. Results should more and more meet the criterion of utility. There is also a tendency for universities to need to earn their own money by means of contract research for external commissioners. And, last but not least, over ninety percent of our students need to deal with practice-oriented research once they leave university. So the latter has a system to responsibility for equipping these students for this type of research. In order to give this equipment a firm and adequate basis, the question that must be answered is whether this shift from 'knowledge for knowledge' to 'knowledge for change' allows the use of the same procedures, methods and strategies as before. In this essay I will defend and explain the proposition that it does not.

Firstly, I will describe the development of practice-oriented research up to now, as well as the current state of the art in this discipline (section 1). Next the question is raised as to which methodological criteria and demands theory-oriented and practice-oriented scientific research need to meet (Box I in Figure 1 below). It will be argued that in practice-oriented research we need to meet, besides the well-known criterion of (epistemological) validity, the criterion of utility of the results. According to van de Vall (1980) I will call this implementary validity. On analysis this criterion appears to be very complex and to contain at least fourteen sub-criteria (section 2). To answer the question what the relevance is of both epistemological and implementary validity for modern social science research, we need to distinguish not only between theory-oriented and practice-oriented research. We also need to differentiate between two categories of the latter, i.e. N-type and n-type research (Box II, and section 3). Both can be focused on two types of practical problems that need to be solved, i.e. improvement problems and construction problems. In order to find out which methodology each of these two types of problems requires, two generic models for addressing these problems are presented, i.e. the intervention cycle and design cycle (Box III, and section 4).

In section 5 a start is made to answer the main question of this essay: whether practice-oriented research needs a methodology of its own, different from the one for theory-oriented research. In answering this question I will follow Figure 1 as a guide-
The next question that needs to be answered is whether all this covers the whole difference between theory-oriented and practice-oriented research. I will explain why the answer must be no. The reason is that the above is confined to the context of justification, with blank spots in the contexts of discovery and implementation and in implementary validity (section 6). Moreover, part of the responsibility of a practice-oriented researcher relates to the moral correctness of research results. It's argued that as a consequence of the shift in contexts already mentioned, we need to introduce ethical validity as an important criterion for practice-oriented research (section 7).

Finally, the question will be answered what can be concluded from the analyses in this essay and to which methodological developments and innovations in the future they hopefully will lead (section 8).
1. THE STATE OF THE ART

Although mainstream research in the social, policy and management sciences is theory-oriented, there has always been a debate about the (need for) practical relevance of science. In the last few decades this debate has taken place under the heading of utilization of knowledge. This discussion takes as its starting point a conceptual use of scientific knowledge (Shadish et al. 1995). This refers to the influence that scientific knowledge has on the ideas and practices of professionals in social life. In this enlightenment function of science there is no question of transfer of knowledge but of a, partly unconscious, diffuse process of influencing. This process not only runs via media such as books and articles, as well as radio, television and internet, but also via education of young people in schools. For instance, much of what once was the result of hard scientific experimentation later becomes part of the teaching material at universities. Next it enters the curricula of high schools, and finally it is offered to children in elementary schools as fixed ideas and truisms.

Since the 1980s there has been a rise of practice and competence-oriented disciplines in the social sciences, such as public administration, policy sciences, organization theory and business administration. Here there was a clear tendency towards an instrumental use of science and its products (Shadish et al., 1995). The aim is to directly influence social and technological reality. Little by little utility rises as an important quality criterion for scientific research. At first this process took place in the above-mentioned competence-oriented disciplines. But, in the last decade the demand of utility has also gained importance in settled social sciences such as psychology, sociology and anthropology. This is part of the 'scolarization' of society. To quote Malhotra Bentz and Shapiro: 'As work in advanced industrial society has become more knowledge-based, social and human science research has moved farther out of the exclusive domain of academia into the world of work and business' (Malhotra Bentz and Shapiro, 1998: 4).

The question may be asked to what extent this change was accompanied by a change in research methodology. One of the first to ask this question in the Netherlands was van de Vall. In a publication in 1980 he broke a lance for developing what he called 'social policy research towards a self-reliant discipline with a methodology of its own'. He was surprised that in the discussion about social science research no distinction was made between the social research at universities on the one hand, and social policy research in practice on the other. He worried that this in his words 'mono-paradigmatic approach' in social science research made unreasonable demands on researchers. 'For instance, on the one hand they are supposed to fulfil epistemological requirements of social science knowledge building, and on the other to meet the implementary demands of social policy development' (van de Vall 1980: 19). With epistemological demands or validity he means '... as truthful and exact a diagnosis of the social problem as possible'. And with implementary validity he means that research must offer a strong foundation for '...designing policy alternatives and developing concrete policy' (van de Vall 1980: 29).
objection comes down to the proposition that policy research at that time mainly used mainstream methodology. The problem with this practice is that since the 1940s this methodology was mainly developed within a scientific practice that may be characterized as science in an ivory tower, with its claim of knowledge for knowledge's sake. This methodology was thus developed within research tradition where resolving theoretical problems and building theory was the primary aim, for which an epistemological validity was the criterion to be met. It was not made for practice-oriented research, designed to solve practical problems and for which an implementary validity was the primary issue.

In the last few decades there have been two domains in the social, policy and management sciences where practice-oriented research played a central role, i.e. policy research in the domain of public administration and policy sciences on the one hand, and organizational research in public and private organizations and in the management sciences on the other. Examples of the first are: Birkland 2001, Cochran and Malone 1995, Coleman 1972, Lynn and Stein 1999, Mayer and Greenwood 1980, Sabatier 1999, Weis 1986. Research literature in the domain of management sciences is to be found under a variety of labels: Business research (Bryman and Bell 2003, Jankowicz 2005), management research (Gummesson 2000, de Beuckelaer 2005), organizational research (Cassell and Symon 1994, Lee 1999), applied research (Bickman and Rog 1998) and marketing research (Baumgartner and Steenkamp 2001, Malhotra and Birks 2000).

However, the methodological basis of these domains remained weak. There are at least three erroneous beliefs of scientists that are responsible for this state of affairs: (a) practice-oriented research is no task for science and universities, (b) no methodologically relevant difference between theory-oriented and practice-oriented research, and (c) practice-oriented research involves applying existing policy and management theories and existing methodology. These three reasons will be briefly elaborated below. (a) Practice-oriented research is not a task for science: The first reason for being behind of research methodology is the belief that practice-oriented research does not deserve an independent position in science and at universities. One part of this view is the belief that practice-oriented research does not belong to the domain of scientific research. The soundness of this position depends on the answer to the question what is meant by scientific. There are three possibilities, relating to the product, the methods or the object of research. The first meaning of scientific is that research provides a contribution to science, i.e. theoretical knowledge. The second meaning is that the research is carried out following scientific criteria and using scientifically approved procedures, methods and strategies. And following the third meaning the research takes human practices, such as strategic management and public policy as the object. Practice-oriented research as it is elaborated here is not scientific in the first sense, but it is in the second and third meaning. That is, practice-oriented research as it is carried out by social scientists, should meet scientific-methodological criteria and procedures, and should take human practices as its object. Many proponents of the view that practice-oriented research
does not belong to the realm of science believe that criteria for practice-oriented research are easier to fulfil than those for theory-oriented research. However, as will be explained in the following sections, the opposite is true; it must fulfil more and more complex criteria than theory-oriented research.

In the opinion of the adversaries of an independent position of practice-oriented research the task of science and of universities is autonomous knowledge production, not hindered by locally defined demands and circumstances. Most probably this view is fed by a deeply rooted double conviction handed down through several generations of scholars. The first is that science only should be involved with epistemological validity, instead of implementary validity. That is, not utility but truth is the primary concern of science, where truth is defined as ‘correspondence with empirical reality’. The second conviction of traditional science during the second half of the last century is the emphasis on external validity as the main criterion for an epistemological validity. One of the arguments that are given for this state of affairs is that general theoretical knowledge is needed to intervene in reality in a responsible way. In this view practice-oriented research that aims at context-bound results is ‘wicked’ (see Swanborn 1989).

This development to a large extent is a remnant of positivism in science during the second half of the last century. This scientific streaming is based on the doctrine of non-normative or value-free science. It states that the results of scientific research should be influenced as little as is possible by the values and norms of the researcher. The background of this claim for value-free science is that normative propositions can’t be tested on empirical grounds. And the latter in its turn is against the positivistic doctrine of taking sensory observation as the basis for science. Thus, in this view, values and normative propositions must be excluded from science.

(b) There is no relevant difference: A second reason why research methodologists have paid little attention to practice-oriented research, i.e. policy research and organizational research, may be the conviction of many scholars that there are no methodologically relevant differences between theory-oriented and practice-oriented research. As a result, most scholars think that traditional (theory-oriented) research methodology can be used without any problem, despite a change in the direction of practice-oriented research. Correspondingly, current education in research methodology at universities is still roughly the same as it was several decades ago, the time that science exclusively aimed at developing a body of knowledge. More details about this will follow in section 6. In this view paying methodological attention to practice-oriented research would be a waste of time and energy, given the availability of an outstanding research methodology, which we should use instead of searching for new methodologies. Briefly the argument is not to reinvent the wheel.

(c) Practice-oriented research means applying know-how: A third important reason that practice-oriented research has a weak methodological basis is the erroneous conception that the characteristic feature of this type of research is to be found in its content
rather than in its form (read methodology). For an explanation and foundation of this belief we need to look at the way policy research and organizational research developed over the last few decades. These were and still are strongly based on knowledge and insights from public administration and organization theory, respectively, knowledge that must be applied. A clear indication of this conception is the often used term applied research for practice-oriented research. This term shows ignorance of the essence of any research, i.e. the production of new knowledge. In this new type of research strategies and methods are applied, rather than adapting these methods or creating new ones. As a result, so far material knowledge from public administration and business administration has been essential for policy and organizational researchers, besides knowledge from existing (theory-oriented) methodology. Correspondingly, when young researchers leave university hardly any specific research methodology for this type of research is part of their toolkit.

The policy analysis approach came across from the United States, where pragmatism was a leading approach since the thirties of the last century. According to Dewey, one of its most important proponents, knowledge is true if it is useful. As a follow-up to the work of Dewey and based on the assumption of manipulability of social and technological reality, scientists such as Lasswell and Dror (1971) developed the policy sciences towards an independent interdisciplinary approach. This was based on applying knowledge from the social sciences, combined with know-how about policy and intervention processes. Also the later developed idea of social engineering of Wildavsky (1992) and Parsons (1995) still makes part of the tradition of applying existing, rather than producing new knowledge.

A clear example of this strongly intrinsic approach to practice-oriented research is diagnostic research. For most professionals in the domain of public policy and organization management, diagnosis means looking at a problematic reality from the perspective of normative or prescriptive theories, i.e. theories that say how reality should be. This reveals itself in the type of chosen objects and perspectives of the problem solver, in the concepts and theories that are used, and in the exclusive use of the intervention cycle or policy cycle as a frame of reference (see Verschuren 2008, and section 4 of this essay). However, in my view this is the task of the professional problem solver. In contrast, a practice-oriented researcher should aim at the production of new knowledge. In the case of diagnostic research this means describing and explaining the problem to be solved. The argument comes down to the search for time and context-bound knowledge and insights of the problem at hand, more than the use of (pre-existing) general theoretical knowledge (see also Verschuren 2008).

We also find this emphasis on intrinsic policy research in van Hoesels' book representing the state of the art of policy research in the Netherlands (Van Hoesel et al, 2005). Policy research in their view is mainly policy analysis. They hardly attribute any specific methodology to this type of research. I do not object to the use of public policy
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theory as such, as an important ingredient of problem solving is the use of what is already known about the problem at hand. My objection is that practice-oriented research as they see it rather can be characterized as policy science and policy analysis instead of as policy research or, more general, as practice-oriented research, for which we need a special methodology (see Verschuren 2006).

Another aspect of intrinsic policy research which negates the need for a special methodology for practice-oriented research is the belief that the use of theoretical, read generalized, abstract and law-like knowledge, is superior to that of practical and context-bound knowledge (see Swanborn 1989). From this belief follows the confusion of practice-oriented research as a discipline with its own objective of (context-bound) knowledge production on the one hand and a strong emphasis on (applying) general theoretical knowledge on the other.

The conclusion is that little has changed in policy research since van de Vall some thirty years ago complained about the lack of research methodology for practice-oriented research, other than an ongoing development of methodology that mainly suits theory-oriented research. The same goes for organizational research. Here too we mainly see a mix of applying organizational and management theory on the one hand, and existing research strategies and methods on the other.

2. THE CRITERION OF IMPLEMENTARY VALIDITY
In this section I will scrutinize the criterion of utility or implementary validity. However, for reasons of both comparison and relevance for practice-oriented research I will first briefly mention settled criteria of mainstream social, policy and management science research. The two best known criteria are internal and external validity. Before I called these epistemological validity, as it is about the question as to whether the knowledge produced is true. Here the touchstone that is used for being true is correspondence with empirical reality. Another word for external validity is the 'generalizability' of the results of research.

Two additional settled requirements are verifiability and cumulativity. The criterion of verifiability involves the demand that it should be possible for people other than the researchers to check what the latter exactly did and whether they followed methodological sound procedures. This in principle means that the research material or data on which the research was based must be open to the public and that the researcher is fully clear about the way he or she designed and carried out the project. Finally cumulativity means that the knowledge produced contributes to building a body of knowledge or theory. In order to achieve this, the researcher needs, for instance, to link the research to existing theories, to use the concepts and their definitions from these theories, and to make use of standardized and validated measurement instruments or scales.

The four above-mentioned criteria are well known in mainstream mainly theory-oriented research. For that reason they are not further elaborated here. The reader is
referred to methodological handbooks (Babbie 2007, Bernard 2002, Black 1999, Judd et al 1991). As these criteria come from inside science, they may be called internal scientific criteria. Although they were mainly developed for theory-oriented research, they also play a role in practice-oriented research. In particular, internal validity and verifiability are important in most practice-oriented research. Less so do the criteria of external validity and cumulativity.

As already argued in the last section, for practice-oriented research with its external goal, i.e. resolving a practical problem, we need the criterion of utility or implementary validity. It’s defined as the extent to which the knowledge produced can help to bring about an intended change in reality, either improving existing reality or constructing a new artefact. In section 4 I will call these two cases improvement and construction problems, respectively. More specifically, the main question that needs to be answered as to implementary validity is: Is the knowledge useful for solving the problem to be addressed, or for creating and making the artefact to be developed function well?

The distinction between implementary and epistemological validity is not obvious. For those who are not convinced of an independent position of practice-oriented research besides mainstream theory-oriented research, there are several possibilities. (a) They don’t see any difference between the two types of validity, (b) they think that epistemological validity is the only important thing for social science and/or (c) they believe that the distinction does not make a difference as a criterion for scientific research. In all these cases the main question of this essay, whether we need a methodology for practice-oriented research that differs from the one for theory-oriented research, must be answered with no. Despite these believes the difference between epistemological and implementary validity appears to be considerable and peculiar as well. An elaboration is postponed towards the end of this section, once we have a better understanding of the complex criterion of implementary validity.

Of course, the criterion of implementary validity or utility is not new; it has been used already for decades in evaluating the results of practice-oriented research. However, it never was thoroughly analyzed by research methodologists. This might be a reason why there are quite often complaints about the utility and actual use of the results of policy research and organizational research. Upon analysis the criterion of utility appears to be very complex. I started my analysis by formulating three main factors that may affect the implementary validity of knowledge as defined above. (1) Characteristics of the object of research. That is, for supporting an intervention or the construction of an artefact with useful knowledge we need to select the right object of research, and do the selection in the right way. (2) The way we look at this object. For instance, we can do this in a holistic or reductionist way. (3) Last but not least, a factor that determines the utility of knowledge is the claims of the selected object. Here we have another clear distinction with theoretical knowledge. This is that in practice-oriented research the target population or stakeholders themselves must be able and ready to use or imple-
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Context of justification

<table>
<thead>
<tr>
<th>Internal scientific (theory-oriented)</th>
<th>External scientific (practice-oriented)</th>
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<tr>
<td>Object of research</td>
<td>Needs of stakeholders</td>
</tr>
<tr>
<td>1 internal validity</td>
<td>5 variability</td>
</tr>
<tr>
<td>2 external validity</td>
<td>6 new phenomenon</td>
</tr>
<tr>
<td>3 verifiability</td>
<td>7 (group) processes</td>
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<td>4 cumulativity</td>
<td>8 interconnectedness</td>
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<td>9 small target population</td>
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Figure 2. Overview of methodological demands for scientific research

ment the artefact or knowledge produced. In the determining factors described above we recognize three distinctive contexts, which are scrutinized in sections 6 and 7 below, i.e. the contexts of discovery, justification and implementation, respectively.

Starting my analysis with these three approaches, I found by means of logical and semantic analysis fourteen sub-criteria and demanding issues for implementary validity. I have divided them into three categories. These are sub-criteria/demands relating to (a) the object of research or the problem to be solved, (b) the needs of the stakeholders as to the research and its results and, (c) the type of knowledge to be produced (see Figure 2).

There is a difference between the second column in Figure 2 relating to the object of research on the one hand, and the rest of the criteria in this figure on the other. This last category (columns 1, 3 and 4) act as criteria or standards that may be threatened by all kinds of disturbing mechanisms, errors, biases, and fallacies. For instance, the internal (epistemological) validity may be threatened by biasing mechanisms such as strategic answers, reactivity, social desirability and ecological fallacy. In contrast, the issues in the second column are not criteria, but features of the object that ask for methodological attention. But what the latter have in common with the criteria in the other three columns is that they all are issues that should be met by, and that put constraints on, the research at hand. Below the criteria and issues in the columns 2, 3 and 4 are explained. Besides, I will give some first ideas relating to the methodological consequences of these issues (see Figure 3). In section 5 these consequences will be discussed at length.

Two remarks should be made before discussing the criteria and demanding issues. Firstly, the reader is warned that several of them are highly interconnected. Examples are the sub-criteria holism, interdisciplinarity and context restraint. Another example forms the issues in the third column of Figure 2. As will be pointed out below they form, looking top down, a cumulative order. This does not detract from the fact that each of these criteria has its own demands, both as to research methodology and its possible contribution to biasing implementary validity. A second remark is that unrav-
### Issue | Criteria and demanding issues | Methodological need for ...
--- | --- | ---
**Object of the research** | 5. variability  
6. new phenomenon  
7. (group) processes  
8. interconnectedness  
9. small target population | → flexible research design  
→ strategy needing little prior knowledge  
→ longitudinal research  
→ independent observation/data gathering  
→ strategy needing only few research units

**Needs of the target population** | 10. comprehensibility  
11. acceptability  
12. legitimacy  
13. research as a learning proc. | → research strategy close to common sense  
→ comprehensibility  
→ acceptability / democratic research  
→ open design approach / flexible design

**Knowledge to be produced** | 14. holism  
15. interdisciplinarity  
16. context restraint  
17. profoundness  
18. manipulability | → open ended, qualitative approach  
→ observ. triangul. / participatory research  
→ ideographic approach  
→ intensive research / qualitative approach  
→ no methodological recipe available

Figure 3. Criteria and demanding issues for practice-oriented research, and one or two of the main methodological requirements that therefore should be met.

elling these sub-criteria, although this was done for the sake of implementary validity, may be also highly relevant for epistemological validity. For instance, one may ask whether reductionist and superficial knowledge, representing the opposite of holism and profoundness (numbers 14 and 17), is detrimental to the epistemological validity of knowledge (see also the end of section 8). I leave this question to other methodologists and researchers.

**The object of research**
The next five sub-criteria relating to characteristics of the object of research may differ gradually, rather than fundamentally, from the situation in a theory-oriented research.

(5) **Variability:** In theory-oriented research we mostly study phenomena such as employability, terrorism, criminality, pollution of the environment etcetera, rather than incidental happenings. Such phenomena most often have a relatively stable character; they do not change every day. Moreover, in this type of research we mostly aim at the commonality of these separate phenomena, and we are searching for patterns and general laws. In contrast, in practice-oriented research we primarily need to look for uniqueness of phenomena and their links to the local context (see point 16). A consequence of this state of affairs is that we need research strategy that is *flexible* and that permits rapid changes in the research design if necessary. An extra reason for flexibility is that in the beginning of research it is often not yet clear what the problem to be solved exactly is.
After a while, the problem at hand may appear not to be what it looked like at first, and the research issue and the resulting technical research design must be changed.

(6) *New phenomenon:* The object of a practice-oriented research often consists of new phenomena that recently came into being. The reason is that just with new objects and phenomena there may raise problems. And in case a newly produced artefact needs to be evaluated; this field is still in its infancy, requiring adaptations and improvements. Research designed to support these and suchlike cases should not need much prior knowledge, for the simple reason that this knowledge does not yet exist. For instance, a quantitative survey must to a large extent be designed beforehand. It thus needs sufficient knowledge in order to be able to take the right decisions. For that reason in general qualitative research and approaches that follow an open design strategy are preferred here (see Verschuren 2009, chapter 12).

(7) *Importance of (group) processes:* For problem solving, which in principle means intervening in social and/or technological reality, it is often important to have knowledge of processes, rather than of the products of these processes. This is especially the case relating to group processes, such as developing criminality in a district, the integration (or lack thereof) of foreigners, insufficient cooperation in work teams, etc. Before intervening successfully in a social group, the interventionist must know the social mechanisms at work.

For instance, in order to achieve better communication and cooperation between group members, it is useful to know how processes of communication and cooperation actually work. For this task longitudinal research is preferable. In general, qualitative research designs are more suited for this task than quantitative designs (see Verschuren 2009).

(8) *Interconnectedness of research units:* In small groups everybody knows each other. This may especially happen in an n-type of practice-oriented research (see section 3). Here the group members often communicate and interact intensively with each other. And most of the time they have strongly contradicting stakes. The result is that interest bonds may come into being with strong in-group and controversial out-group attitudes. All this puts pressure on the fundamental methodological demand of independent observation in evidence building. The latter requires labour-intensive research methods, i.e. qualitative research strategies. There is also a link with holistic research (see point 14 below and section 5).

(9) *Small target population:* Mainstream theory-oriented research in the social, policy and management sciences is mostly based on large samples from still much lager populations, in order to enhance the highly valued external validity of the research results (see section 5 below). However, the target population in practice-oriented research is often relatively small. Moreover, as we often need context-bound knowledge for problem solving, there may be a preference for dividing a large population in several subpopulations (see also point 16 below). Thus we need a research strategy that requires only a
relatively low number of research units. This excludes, for instance, most quantitative research.

Needs of stakeholders
Another set of sub-criteria to be met in order to achieve implementary validity relates to the needs of the stakeholders and/or the target population. These needs result from the fact that the stakeholders or members of the target population themselves often must do something with the knowledge that is being produced. These sub-criteria form a cumulative series, as the process model below shows. In this type of model X \(\rightarrow\) Y means: X must have been done, achieved or realized before Y can be done, achieved or realized. (See Verschuren 2008 for more details about this type of model).

![Diagram](attachment:image.png)

In words this model says: An individual must understand a proposition or a text, i.e. the knowledge or artefact to be produced with a research, before he or she can accept it. The latter in its turn is a precondition for finding it legitimate, which must have been achieved before it is possible to learn from it. Each element in this series adds something to the former.

(10). Comprehensibility: As an example, imagine that the members of a target group need to change or adapt their beliefs, opinions, attitudes and/or behaviour. This may be needed to bring about a desirable change in the functioning of this group. The first thing that is needed then is that the group members understand the results of the research. Often they also need to understand the way these results were produced. The consequence of this is that highly sophisticated research is less useful than approaches that are closer to common sense, such as qualitative research strategies.

(11) Acceptability: The understanding of the research results by the target population is a necessary, but not sufficient condition for their acceptance. The members of the target population should also recognize these results as relevant, as representing their values, beliefs and opinions, and as fitting their needs and demands. For this the researcher must communicate with them openly and intensively, and use for instance reflective techniques such as empathy and introspection. More generally, acceptability once more requires the use of research methods and procedures that come close to common sense. For more information about these subject matters the interested reader is referred to Verschuren 2010 (forthcoming). Another option is action research. This is a form of research where there is no clear difference between the researchers and the researched. For more information the interested reader is referred to Whitehead 1994 and Verschuren 2010 (forthcoming).

(12) Legitimacy: The stakeholders and members of the target population must also perceive the knowledge and the way it is produced as legitimate. In order to achieve this,
the researcher must, for instance, be able to handle differences in power between those involved. He or she should be able to avoid the dominance of powerful stakeholders over the less powerful.

(13) Research as a learning process: As already stated, the members of the target population are often actively involved in processes of change in general and organizational change in particular. It's important that they get feedback on their behaviour, which is an instance of learning. This learning function can often be fulfilled by practice-oriented research, especially in the form of evaluation research. More specifically process evaluation and formative evaluation are at stake here. Besides this form of learning as 'feedback upon action' the people involved can also learn in the form of 'exploration in action'. Here too research may play an important role. More specifically the strategy of action research can be used. Another option is the use of an open design strategy (see Verschuren 2009, section 10.5).

Type of knowledge
A third and last category of sub-criteria for implementary validity relates to the type of knowledge to be produced in the research. Just as in the last category, most of them are interconnected.

(14) Holistic knowledge: In philosophy holism simply means; studying a phenomenon as a whole. However, in empirical science we need to make this concept operational. When it comes to sensory observation, holism bears three aspects. These relate to the way the object is: (a) selected, (b) treated and (c) observed. I will describe them briefly.

(a) The selection of the object: This aspect of holism has two modalities. The first is selection of an entire phenomenon instead of an aspect or part of it. The opposite of this aspect of holism is specialization in science. This is what happened in the last half century, when the empirical domain of scientists became – and is still becoming – smaller and smaller. To an increasing extent scientists know more about less. The second modality relates to the question as to whether we look at a phenomenon itself, or at an artificial created dummy. The second alternative again is an opposite of holism, and is called experimentation. Although building a dummy necessarily means reductionism in one respect, it may contribute to holism in another. That is, in a dummy we necessarily lose details, but we may win holism with respect to the opportunities it offers in the domain of observation (see c below) and evidence building.

(b) The treatment of the object: Here holism means to treat the selected object as a whole, instead of unravelling it into particles and aspects. The opposite of this is analytical reductionism. The latter has two components: (a) the object is analytically divided into observation units (first reduction) and (b) the latter is conceived as a set of variables (second reduction). For instance, instead of looking at work teams as a whole (research units), we look at the individual members of the teams (observation units). And instead of observing these people, the latter in their turn are conceived as a set of characteristics,
the variables. This is what happens in mainstream quantitative social science research. The weak point of this approach is that the whole is more than the sum of its parts. Holistic research then means that we make observations of the teams as a whole. The difference with the former aspect (a) of holism is that this belongs to the context of discovery, whereas the second aspect relates to the context of justification.

(c) The observation of the object: Here too are two closely related modalities. The first relates to data gathering and the second to observation. As to data gathering we can make a difference between data in vivo on the one hand, and recorded data on the other. 'In vivo' means that we gather data directly from the object itself, without an intermediate. In contrast, the sources of recorded data are media and documents that deliver data from or about the object. In accordance with common sense and other things being equal, data gathered in vivo are closer to holism than recorded data do. For more information over these distinctions see Verschuren 2009, chapter 13.

As to the act of observation we need to study an object in its context. This issue also has two aspects; a spatial and a temporal aspect. The first means observation of the object in relationship to its physical and social environment. The second involves following an object in its development through time, i.e. in the past as well as in the future. Not doing either of the two or both must in fact be qualified as a tunnel view.

To sum up, in empirical research holism means studying the entire object instead of an aspect or a part of it (specialization), studying the real life object instead of a dummy (experimentation), looking at the object as a whole instead of at its separate aspects and particles (analytical reduction), gathering in vivo data instead of recorded data, and studying the object in its time and space context instead of studying it isolated (tunnel view). In this series the second alternative indicates instances of reductionism, as the opposite of holism. For more information about holism and reductionism in empirical research see Verschuren 2001.

(15) Interdisciplinary knowledge: Closely related to the argument of holism is the need for an interdisciplinary approach. In order to get insight in all the aspects and parts of the problem at hand and the way they interact with each other, we should approach it from different angles and perspectives. Although interdisciplinarity is a necessary condition for holism, the reverse is not necessarily true. Neither is interdisciplinarity a sufficient condition for holism.

There are three reasons why (the resolution of) practical problems cut across existing disciplines. The first is that most problems have multiple causes. For instance, juvenile delinquency may have economical, social, psychological and biological backgrounds. In principle, each single discipline may contribute to the solution of the problem. However, if the causes interact with each other, which is often the case, then only an interaction of disciplines may solve the problem (see also Ragin 1987, with his idea of multiple conjunctural causation).
A second reason for the importance of interdisciplinary research is to be found in the reason why we define something as a problem. For instance, absence due to illness may be defined as a problem because it harms the well-being of the employee, the atmosphere of the working team or leads to poor organization of tasks. As is well known from intervention methodology, the solution to a problem to a large extent depends on its effects (see Verschuren 2008). So, interdisciplinary knowledge about these different effects may appear to be valuable, especially if the various consequences interact with each other.

A third and last reason for interdisciplinarity is that a solution to a problem must often fulfill several side-conditions. For instance, an intervention designed to reduce pollution may need to meet administrative, juridical, economical and infra-structural restrictions. Exact knowledge about these conditions may then be needed in order to achieve a permanent solution of the problem at hand.

(16) Context-bound knowledge: As already stated, in most cases problems are strongly embedded in and interwoven with the local social, political and technological context. For that reason, different from theory-oriented research with its focus on finding general (read context-free) and law-like knowledge, practice-oriented research should study this context and the way it interacts with the object of the research. This argument seems to deny the possible role that general theories can play in practice-oriented research. However, general theories can, for instance, be used in the first stage of diagnosis, as they can often indicate the direction for finding the causes of the problem at hand. Once it is clear in which domains the causes of the problem at hand may be found, the researcher must look further into the context in order to verify the agency of the cause(s) in that particular case and to obtain details about the process(es) of causation at hand.

(17) Profoundness: In-depth knowledge may be an important condition for solving a problem. It may mean, for instance, the search for deeper causes instead of symptoms. The deeper the causes we tackle, the more sustainable a solution may be, other things being equal (see also Verschuren 2008). It may also mean the study of processes instead of just the outcomes of these processes (see point 7 above). For instance, imagine that we want to improve an intervention or a new artefact once it is executed or implemented. Then, in most cases, we'd better carry out a formative process evaluation of the way the intervention was carried out or the artefact was produced and/or implemented, than just a summative product evaluation of (the effectiveness of) the outcomes. And if we want something to change in the minds of people, we need a profound insight in their motives, perceptions and attitudes, rather than in their characteristics such like age, the amount and type of education, or type of job.

(18) Manipulability: The (epistemological) validity of knowledge is not sufficient for being useful in problem solving. The knowledge should also regard phenomena that can be changed by the researcher or problem-solver. For instance, many researchers have
found that the social class of children has an effect on their performances at school; higher class children in general perform better than lower class children do. Yet the results of the latter cannot be improved on the basis of this knowledge, as social class cannot be changed. Fulfilling the demand of manipulability is simply a matter of selection by the researcher. No methodological measures are available. More about the relation between validity and utility of knowledge in the section to follow.

**Truth versus utility**

Once we know more about what implementary validity means and the considerable number of aspects it contains, the differences between epistemological and implementary validity, or between truth and utility, can be considered. As already stated, this relationship has several peculiar aspects. For a difference between the two, in principle we need to look at the columns 2, 3 and 4 in Figure 2, as these relate to the criterion of utility, whereas column 1 primarily relates to epistemological validity. However, the issues in column 2 are not relevant for defining a difference between epistemological and implementary validity, as they are not linked to knowledge but to the production of knowledge. That is, they represent possible difficulties that especially a practice-oriented researcher is likely to encounter and that he or she must address.

In contrast, the sub-criteria in column 3 are at the heart of this distinction. On little reflection they make clear that objective or inter subjective truth is not sufficient for being useful for resolving practical problems. The target population should also perceive it as true. To say it analogically to a well-known expression: *truth is in the mind of the owner*. The issues in column 3 are necessary (but not sufficient) conditions for achieving subjective truth. In column 4 too are sub-criteria that make clear that truth is not sufficient for successfully intervening in reality. A first issue relates to *reductionism* as the opposite of holism and interdisciplinarity (see point 14 and 15 above). Most aspects of reductionism can be seen as the cornerstones of mainstream social science research. For instance, specialization contributed extremely to the explosion of scientific theoretical knowledge over the last half century, where epistemological validity by far was and is the most important criterion. This was and is still possible because each piece of knowledge contributes to the accumulation of knowledge in theories. However, specialization is unfavourable or even much of a problem, for intervening in social reality, i.e. for implementary validity. For the latter we preferably need insight into the whole object of the intervention, as well as into the way it is connected to its context in time and space. One reason for this is that without this knowledge we will not know what all the side-effects of an intervention or an artefact may be, which can make it hazardous and irresponsible. Another effect may be that we only find out about one or two causes of the problem to be solved. This can be a particular problem if there are several interacting causes of the problem to be solved. Here Ragin's mechanism of 'multiple conjunctural causation' is at stake (Ragin 1987).
Moreover, most problems that are studied by practice-oriented researchers are a problem *tangle*, consisting of several interwoven aspects, and having a strong entanglement with its context. For that reason, designed to achieve a thorough and permanent solution of the problem, practice-oriented researchers should try to produce holistic knowledge as much as is possible. Other things being equal, in general qualitative and participatory research strategies fit better the aim of holism than quantitative and reflective research do (see section 5). And, as to the way of generating the research material, in vivo data and observation in principle produce more holistic knowledge than stimulus-response techniques such as interviews and written questionnaires do.

Another issue to be taken into account is context restraint. There are arguments why general and abstract theoretical knowledge, although it is true, may not be useful in a particular situation. These arguments are closely connected to holism. For instance, causal agencies may need special circumstances in order to get active. In research methodology this is an instance of the well-known mechanism of statistical interaction. The working of (a set of) causes may also depend on the history of the object at hand. Moreover, according to the principle of multiple conjunctural causation, a causal mechanism may consist of a series of interacting causes. Whether this mechanism is active and, if it is, what its specific causal effect will be depends on the specific constellation of this set of causes in a certain context.

Profoundness too may make a distinction between truth and utility. For instance, it may be true that X causes Y. Yet if Y is the problem, it is by no means sure that affecting X will bring about a sustainable change in Y. If X itself has many causes, then a change in X that is brought about by the problem solver may be soon neutralized by deeper causes of X that were not affected and that have a negative effect on X. Another problem may be the existence of other important direct causes of Y, besides X. If they have a substantial effect on Y, affecting X may bring about only a minor change in Y.

The same type of reasoning relates to the difference between 'superficial' causes of human beings' behaviour on the one hand, and deeper motives on the other. Affecting the latter, if feasible and acceptable, mostly is more effective in bringing about changes in human behaviour than 'external' causes do. This is another example showing that qualitative research strategies may be more useful than quantitative strategies. Finally, the difference that manipulability may make between truth and utility, the last issue in column four, has already been explained.

Earlier on, we saw examples of the fact that just because something is true it's not necessarily useful. The reverse is also valid; knowledge need not necessarily be true, read empirically tenable, in order to be useful. An example of this can be found in the concepts of self fulfilling and self denying prophecy. Imagine there is a surplus of carpenters on the labour market. Then wrongly stating that there will be a surplus in the future may cause the surplus to disappear. In general we can say that what people think is true may be more important for bringing about changes in social reality than empirical validity does. It stands to reason that here ethical implications easily come into play.
At the end of this section on the sub-criteria of implementary validity, which distinguishes between practice oriented and theory-oriented research, a few additional differences between the two can be mentioned. One of them is that theory-oriented research is carried out in an academic context, whereas practice-oriented research is done in a political environment. This may cause problems during implementation (see section 6).

A methodological difference between the two is that theory-oriented research primarily makes use of an intellectual and comprehensive rationality, whereas the second on top of this must rely on a political and contingent rationality. That is, in the latter case, arguments and lines of reasoning do not only follow the rules of formal logic and objective observation. Important are matters such as power, bargaining, costs, feasibility, and such like. Besides, the theory-oriented researcher has a much more autonomous position than a practice-oriented researcher. In the first case there is no controlling agent other than the scientific community, which does not care what is studied, neither is it that much concerned with the context of discovery. In practice-oriented research, however, there is a customer with explicit demands who also often has ideas about how the research should be carried out. Usually there are several stakeholders with different conceptions of the problem at hand and contradictory demands. This forces the researcher to find her or his way in this jungle of contradicting demands, to bargain and to reconcile different points of view. Moreover, commissioners and stakeholders often have unreasonable and unrealistic expectations of the researcher, so that the latter must convince them that they genuinely need support with research.

3. N-TYPE VERSUS n-TYPE RESEARCH

In discussing the state of the art of research methodology for practice-oriented research in section 1 a distinction was being made between two broad areas in social, policy and management science research, i.e. policy research and organizational research. This is the realm of (a) public policy and (b) private and public organizations, respectively; the latter divided over for profit and not-for-profit organizations. This distinction appears to have sound methodological consequences, as elaborated below.

There are two main characteristics that make a difference between policy research and organizational research: (a) a public versus private domain and (b) a large versus a moderate or small scale, respectively. Policy research is in the public domain and is large scale. That is, the target population in principle exists of at least thousands of people, and can go up to hundreds of millions of citizens. Organizational research is situated in the private domain, and is relatively small scale. Here the target population varies between some dozens to some ten thousands of organization members. I call this N-type and n-type of practice-oriented research, respectively. The first lies on a macro level, and the second on the mezzo and micro level. In other words, in the mean the object of policy research has a higher level of aggregation than the object of organizational research does. This difference in scale, together with the public-private distinction, leads to many other methodologically relevant differences.
In policy research as well as organizational research, the aim is to produce knowledge, insights and information that may help address a practical problem. The difference between a theoretical and a practical problem to be solved is what makes a theory-oriented and a practice-oriented research the most different from each other. Yet from a methodological point of view, as to practice-oriented research it appears to be important to make a difference between an N-type and n-type context. For that reason, in a discussion about the question as to whether there is a methodological difference between a theory and a practice-oriented research, this distinction between an N-type and an n-type context should be taken into account. There are three categories of differences between the two: (1) sociological differences, (2) differences as to the use and the users of research results and (3) administrative differences. These three are briefly elaborated below.

(1) Sociological characteristics: A well-known distinction in sociology is that between groups and collectivities. There are two main differences: (a) scale and, (b) the amount and type of interaction and communication between the members of the group. In an N-type context people only incidentally interact or communicate with one another, whereas in an n-type context they do this on a structural basis. In the latter case an intensive interaction and communication is often even an explicit prerequisite for adequate functioning, for instance, in manufacturing organizations. A consequence of this state of affairs is developing shared values, perceptions and opinions, whereas group processes get an in-group character. But contradictions, antagonisms, interest bonds, sub-cultures and the like may also arise.

Besides, the members of an N-type group most often don’t know each other, whereas the members of an n-type group do. The latter to a large extent know each other’s perceptions and opinions, habits, behaviour, concerns and often their private circumstances. And they also have explicit expectations towards each other, based on issues such like division of tasks, responsibilities and existing formal and informal group norms and standards. Part of this is the existence of social control amongst the group members. Another difference is that in an N-type context the members of the community live together, whereas the members in an n-type context work together. Moreover, in an n-type context people tend to have a social relationship to each other, either in a formal or an informal sense. An informal organization can be conceived as an interconnected set of social roles and linked role-expectations and patterns of communication. And last but not least, in an n-type context people most often have common goals, and from these goals deduced tasks to fulfil.

The consequence of all this is that, compared to a collectivity in an N-type context, a social group is to be characterized by the phenomenon of interconnectedness, with the related methodological problem of an independent observation (see before). It also means that the researcher must face a small group (point 9 in Figure 2) so that analytical reductionism does not work well, that labour-intensive methods are needed and that the target population itself must apply the research results.
Characteristics of the use and the users: As already stated, in an N-type context there mostly is a policy programme of interventions. This programme is carried out by professionals, without or only incidentally being the objects of the policy themselves. They are trained in the use of a policy research, and also often in carrying it out themselves. Moreover they mostly are also skilled in policy practices. All this means that they are trained in translating the results of a policy research in concrete public administration.

The fact that they are not necessarily themselves the subjects of a policy makes them personally free of engagement, more so than when the group of users of research results coincides with the target population, as is often the case in an n-type context. After all, in the latter case the members of the organization are often themselves the ones to transfer these results in their own behaviour. For instance, research report may describe the poor way communication in an organization takes place. Then the members of this organization have themselves to take serious this result, and correspondingly to change their attitude and behaviour in this respect. This means that an n-type of practice-oriented research is often part of a learning process (see point 13 in Figure 2). In contrast, the citizens in an N-type context normally not themselves come in contact with a policy research and its results. As will be elaborated below all these differences bring about changes in the methodology that is needed.

Administrative differences: Another difference is that, compared to the management of an organization in an n-type context, a public administration in an N-type context has a small span of control. The reason for this is that the power of political and administrative institutions is bounded by a system of democratic and political control, in exchange for participation of citizens in the financing of public policy in the form of taxes. In an n-type organization the span of control mostly is much larger, because there is an explicit goal to which the members contribute in exchange with the wages that they earn. Shortly, they receive money instead of paying it, with being controlled as a price.

This large span of control opens an excellent opportunity for using the experiment as research strategy. The testing variant of the experiment is by far the most powerful instrument for the study of causal effects. This is, for instance, important in an ex post product evaluation of an intervention or an artefact, where a causal relation must be proven between the intervention and the changes that it induces, i.e. its effect(s). And an explorative experiment is a most valid and efficient strategy for an ex ante or ex post process evaluation. (See for these forms of an experimental design section 5 and Verschuren 2010 (forthcoming)).

Besides, there are differences in the way a group (n-type context) and a collectivity (N-type context) is steered. In an N-type context the steering is often characterized by policy programmes that are designed to achieve long-term goals. Changes resulting from policy interventions are relatively small and incremental in nature, rather than radical. The reason for this is that radical and abrupt changes - as well as dramatic
changes in the goals of public administration - are likely to lead to tensions, social turmoil and political instability, so they will be avoided. They undermine the loyalty of citizens and they could even disrupt society (see Shadish red. 1995). In an N-type context the changes brought about by the administration may be bigger, more radical and more frequent. This is made possible by a relatively high span of control. In section 4 it is elaborated that in this respect we can make a distinction between improvement problems and construction problems.

One may ask whether there is a correspondence between the lowest level of aggregation in an N-type context, i.e. a quarter of a town on the one hand, and a private organization, i.e. an n-type context, on the other. Despite a rough correspondence in size of the target population, there remain differences as to the intensiveness of connections between the group members, the span of control, and the type of the problems to be solved (construction versus improvement problems).

It's quite remarkable that there are hardly any links between research and methodology in an N-type and n-type context, i.e. policy research and organisational research. Methodologists and researchers in these domains both make use of their own jargon, and have their proper frame of reference. This may be another consequence of the fact that researchers in both domains are very much-oriented at material theories. As pointed out in section 1 of course policy theories and management theories are quite different from each other. However, there are three reasons why research methodologists should not treat the research within these two contexts separately, despite the differences that were made in this section. The first is that both types of research have much in common with regard to the type of problem to be addressed, the research methodology to be used and the criteria to be met, i.e. an implementary and ethical validity. The second reason is that there is an ongoing privatizing of public life. Many organizations that formerly belonged to the public domain become private organizations. Apart from this there are many situations where the differences between the two domains become vague. Third, researchers and methodologists in both domains can learn from each other, as a consequence of the fact that in the past they independently from each other developed insights. These insights should be reconciled by future research methodologists as much as possible.

4. GENERIC MODELS FOR PROBLEM SOLVING

Practice-oriented research focuses on resolving one or the other practical problem, i.e. a policy problem or an issue of strategic management. There are two types of these problems: improvement problems and construction problems. The first are about an existing situation or development, where by means of a problem analysis and a diagnosis a problem is observed. Then by carrying out an intervention the problem solving team or agency tries to improve the problematic state of affairs. In a construction problem, however, the idea rises that something new can be created, rather than that an existing
object, situation or process can be improved. For a new creation I will use here the term *artefact*, which may be material or immaterial in nature.

The difference between improvement problems and construction problems also has something to do with the attitude of the problem owner. This can be explained by means of the distinction that Ackoff made between *inactivism* and *interactivism* (Ackoff 1974). According to Ackoff, inactivists have *‘a greater fear of not doing something that should be done (errors of commission) than of not doing something that should be done (error of omission). Hence the latter tend to react mainly to serious threats, rather than to opportunities’*. That is, in terms of our distinction they are mainly involved with improvement problems. In contrast, interactivists are characterized by Ackoff as people ‘who want to design a desirable future and invent ways to bring it about. ... They try to prevent, not merely prepare for, threats and to create, not merely exploit, opportunities’ (Ackoff 1974). In our terminology, they are the owners of construction problems.

Despite the differences mentioned above, the distinction between improving an existing situation on the one hand, and creating a new one on the other, is gradual rather than fundamental. Each improvement has something new and each renewal continues aspects or parts of the old. For instance, how new is a new type of cargo plane? Doubtless it has many traits and particles in common with existing types of aircrafts. I propose to distinguish three gradations of renewal. (I) A new material or immaterial artefact, of which no variants did exist before, such as the electric motor or nuclear electricity. (II) A radical change of an existing artefact, such as the invention of the helicopter in the middle of the last century. The renewal was the replacement of aerofoil wings by rotating ones. In business organizations this is the well-known domain of product renewal. (III) A smooth change, which in fact means improving an existing material or immaterial object, situation or process, such as the (material or immaterial) products of profit or not-for-profit organizations. The only difference with an improvement problem is that there is no bad functioning of an existing object, situation or process. Instead there is simply the idea that something new can be produced. This does not detract from the fact that the logic of addressing a construction problem can be used in developing a plan for an intervention as part of the resolution of an improvement problem (see the arrow in Figure 4). Correspondingly to the differences above I will distinguish three types of construction problems: type I, type II and type III.

The problems to be addressed in an n-type context are often of the construction type I or II, whereas in an N-type context the problems at hand are mostly improvement problems. Van de Vall too seems to make this difference between improvement problems and construction problems, where he writes (my translation from the Dutch; PV): ‘Not surprisingly, social policy research is seldom oriented towards a total overthrow of the political or social structure. On the contrary, most projects in social policy research are designed to encourage concrete social change within the parameters of the current social system’ (Van de Vall, 1980: 38).
To address a problem in a systematic and proper way we need to follow a generic process model. There are two such models: one for addressing improvement problems and one for construction problems, i.e. the intervention and the design cycle, respectively (see Figure 4). They will be only briefly described below, as they are extensively described in literature. See for the intervention (or policy) cycle the literature on policy research on page 12. For the design cycle see Verschuren and Hartog 2005, and Verschuren 2008 and 2009. The relevance for our research question in the introduction of this essay is that for answering it we need to think about the role of methodology in each stage of these models. This is what will be done in section 5.

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Figure 4. The intervention and design cycles used to address improvement problems and construction problems

The intervention cycle

1. **Problem analysis**: First of all, the question must be answered what exactly is the problem, why it is a problem and whose problem it is, the so-called W-questions. The what-question relates to the facts and phenomena that are experienced as problematic. I call this the F side of the problem (Facts). These facts must be described in a valid way, in principle by means of scientific research. The why-question mostly involves two sub questions: (a) How does an alternative object, situation or process look like that is either desired or preferred, or to be avoided? I will call this the D-side of the problem (Desire). (b) What are the negative consequences or effects of the actual state of affairs (facts)? Here we have to make a balance between positive and negative consequences or effects. The who-question, finally, may refer to three possibilities: the individual or the department that caused the problem, the one that suffers from the consequences or the person or department that is held to resolve the problem. Instead of a problem analysis, people very often start straight away with a diagnosis or even a plan for solution of the problem at hand. Thus it remains unclear what the problem exactly is, why it is (called) a problem and whose problem it is, with bad results of problem solving as a consequence.

2. **Diagnosis**: Once the problem is clear in the above sense, the question is what are the backgrounds and causes that are responsible for the problem to be solved. Usually there is a chain or even a network of causes. These can be represented by a causal model, also
called a diagnostic model (see Verschuren 2008). In most cases the solution to a problem consists of tackling one or more of the exogenous or intervening variables in this model. In order to know which variable(s) best can be tackled the researcher can make use of prescriptive theories. These theories tell him or her, what an optimal or ideal state of affairs would be, given certain assumptions and circumstances. For more information the reader is referred to Verschuren 2008.

3. Conditions for solution: Which conditions must be fulfilled with the solution to be chosen? There may be four sources of conditions that must be checked by the researcher: (a) the agreement of the target population with the plan for a solution. (b) The problem solvers, i.e. the policy makers in an N-type context or the managers in an n-type context, who have to carry out the plan. Involved are the available time, money, knowledge, experience, attitudes and skills of these people. (c) Existing strategic management or public policy. Usually the plan for an intervention must fit long-term policy or strategic management. (d) The social or political environment (N-type context) or the institutional and transactional environment (n-type context). In the first case there may be all kinds of rules and laws relating to the problem at hand and its solution. In the second case conditions like cultural norms and (gentlemen's) agreements may play a role. All these conditions and restrictions narrow down the degrees of freedom of the designer who wants to build a strategic management or a public policy in stage 4 of the intervention cycle.

4. Plan for an intervention: Then a plan for tackling the problem to be solved needs to be developed. This plan has three generic components: (a) the intervention needed for bringing about a change in one or more of the causes of the problem at hand, (b) an impact model that links the intervention with the problem at hand, and (c) a strategic management or a policy that says what the people who are concerned actually must do. More specifically, the intervention is needed for inducing a shift in one or more of the causes of the problem at hand. The impact model is a transformation of the diagnostic model in step 2, where the causal relations of the type ‘X causes Y’ in the causal or diagnostic model, are transformed in prescriptive relations of the type ‘X must be affected in order to bring about a change in Y’. Here Y stands for the problem at hand. See for more information about prescription and impact models Verschuren 2000, 2008 and 2009, chapter 6. Thirdly, in most cases it is necessary to describe a set of tasks that must be fulfilled for making run the intervention and the impact model in a proper way. This is called a policy (public administration) or a strategic management (business administration).

5. Execution of the plan: Most of the times we want to know whether the plan is carried out properly, with the right efforts of the people involved, and with adequate use of the right means. The activities to achieve this are called monitoring. For an adequate monitoring we need a so-called information system, consisting of a database prepared for continuously adding new data concerning the execution of the plan. This makes a
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Process evaluation at any time possible, on the basis of which the intervention can be adjusted immediately when it's needed.

6. Evaluation: After the intervention has taken place, we need to find out whether an improvement has taken place, an instance of ex post product evaluation. For this it is needed to compare an ex ante and an ex post measurement of the target variable(s), i.e. the problem to be solved. If the difference between the two is nil or insufficient, indicating that progress is unsatisfactory, then further action is needed in a second run of the intervention cycle. There are two ways to find out what is or went wrong in the first run of the intervention cycle, which complement each other very well: (a) a plan evaluation and (b) a process evaluation of the way the intervention is carried out. Often an ex post process evaluation is the best to start with. Here we can make use of the information system that was made in the context of monitoring (stage 5), completed with data that are gathered after the intervention is finished. Some important issues here are a check of the validity of the impact model, as well as of the efforts of those who carried out the intervention. Finally it is possible that already something in the plan of the intervention is responsible for a bad result. Here we need to check at face value the feasibility of the intervention goal, as well as the utility of the chosen means for achieving this goal.

The design cycle

1. First hunch/design goal: The design cycle starts with an initial impetus to create a new material or immaterial artefact. It may be based on the idea of somebody or a team of experts from different disciplines, that the artefact is possible given newly developed knowledge, hardware and/or software that may serve for building, or that may function as modules and half-fabricates, of the artefact to be. Next, one of the very first tasks to steer the process of designing is to formulate a provisional goal that the functioning of the artefact or the use of it must fulfil. Later this goal may be adapted, if new perspectives arise or if the artefact gets another shape than was decided at the start of the process. This makes part of the highly iterative character of a designing process (see for details of an iterative strategy Verschuren 2009, chapter 4).

2. Design demands / assumptions: The next step is to make sure what demands the artefact must fulfil and which qualifications future users should have (assumptions), in order to assure an adequate functioning and use of the artefact. There are three variants of design demands: functional, users and contextual demands. The functional demands relate to the functions that (the use of) the artefact must fulfil in order to achieve or to bring closer the design goal. The users’ demands relate to the demands of the future users of the artefact, for instance, those related to the ease and costs of operation. Contextual demands relate to all kinds of claims stemming from the social, political and physical environment where the artefact will be installed or used.
Conversely, it must be possible for the designer to take design assumptions as a starting point and guideline for designing. These relate to criteria that the future users and context need to meet, i.e. the users assumptions and contextual assumptions. User's assumptions relate to both physical and mental characteristics of the future users. Relevant physical aspects may be sizes of limbs, qualities of sight and hearing, and the motor system of the people at hand. Contextual assumptions may relate to issues such like the presence and availability of all kinds of relevant infra-structural means and conditions.

3. **Structural specifications:** These relate to the pieces and characteristics that the artefact must have in order to fulfil the design demands on the one hand, and to make the design assumptions realistic on the other. They must be logically derived from the design demands and the design assumptions. Here the designer should make sure that often the same demands can be fulfilled by different functionally equivalent objects or processes. Conversely, the same object may have to meet different design demands and assumptions. This offers degrees of freedom for the designer, which may be of use in making all structural specifications to a balanced and integrated whole. Together the design demands, design assumptions and structural specifications constitute the *design on paper*, ready to be realized in a prototype of the artefact in the next step.

4. **Construction of a prototype:** The next step is to materialize (material artefact) or realize (immaterial artefact) the structural specifications into a prototype, taking into account the design goal, the design demands and the design assumptions. In particular, when this costs a long time and/or much money, it may be advantageous to make a first reduced and provisional model of the artefact, the so-called *mock up*. Here the only aim is the functioning of the artefact to be. No effort is being made as to secondary issues such as sustainability and aesthetics. One of the tasks of the designer or the design team is to take care that all pieces and aspects of the mock up or prototype constitute an integrated and balanced whole. It's strongly recommended to act in this stage following the strategy of an explorative experiment (see section 5, as well as Verschuren 2009 and 2010 (forthcoming)). Instead of first assembling the whole prototype and then to test it, the designer or the designing team continuously tests the several aspects and particles of the prototype to be on an experimental basis.

5. **Implementation of the prototype:** The prototype can be tested in either a real-life situation or in an explorative experiment. By means of formative evaluation research the designer or the designing team explores the effectiveness of the prototype, with the design demands and design assumptions as criteria. The evaluation has an iterative character, as the design demands and assumptions, and/or the structural specifications, may change at this stage as a consequence of an iterative approach. As already stated, in order to make an iterative procedure easier and cheaper, the designers can make use of a mock up version of a prototype. In case of a material artefact the mock up may also have the form of a scale model. The result is a definitive prototype, ready for
use and or for production if the artefact is to be reproduced. A by-product of this stage may be instructions for implementing the artefact and a first concept of a manual for the users.

6. Evaluation of the performances: Finally, after the artefact has been installed or used for a while, there is a summative evaluation in order to know whether the artefact behaves properly in the long run and whether it contributes to achieving or bringing closer the design goal that was set at the start. In case this evaluation points out that the artefact does not function well, a formative evaluation is needed in order to find out why this is the case. This type of evaluation requires an evaluation of the designing process in the stages 1 to 5 (see also stage 6 of the intervention cycle). For more information about the series of the design cycle see Verschuren and Hartog 2005, and Verschuren 2008 and 2009.

5. IS PRACTICE-ORIENTED RESEARCH DIFFERENT?

In this section it will be argued that the shift in the context of discovery, i.e. the rise of practice-oriented research, must be followed, amongst others, by a shift in the context of justification. This will be done by means of confrontation of methodological demands from section 2, three main types of research from section 3, and stages in both the intervention and design cycle from section 4 on the one hand, with research strategies to be presented below on the other (see the arrows a, b, c, d and e in Figure 1). These confrontations will make clear: (1) to what extent existing research strategies can meet the demands (arrow a); (2) what in their turn the importance of the latter is for the N-type and n-type of practice-oriented research, compared to (a) theory-oriented research and (b) each other (arrow b); (3) what the importance is of the research strategies for the three main types of research (arrow c), as well as for the separate stages of the intervention and the design cycle (arrow d); and finally (4) to what extent the three main types of research (should) make use of both cycles (arrow e).

Research strategies

Below first seven research strategies are shortly characterized as they are to be found in literature on research methodology, or are to be defined below. For more information about these strategies the reader is referred to Verschuren 2009 and 2010 (forthcoming). Correlational research: This is a quantitative type of research, also called large-scale survey. It is normally based on a relatively large random sample with \( n = \text{approximately} \ 80 \text{ to } 10,000 \) elements or more, taken from a still much larger population. Given the large size, labour-extensive methods are needed, such as a written questionnaire, standardized tests and quantitative content analysis. It has its name because the calculation of correlations between variables is one of the first and most important operations that the researcher makes. They are the input for all kinds of multivariate analyses, such as scaling techniques, regression analysis, factor analysis and analysis of variance (ANOVA).
**Testing-experiment:** This by far is the most powerful strategy for testing causal hypotheses of the form: X has an effect on Y. Two groups (or more), one with a treatment X (the cause) and the other without X or with a neutral treatment, are compared as to a dependent variable Y. If there is a difference as to the Y scores in both groups, then under a set of assumptions as to the absence of biases, it may be concluded that X has an effect on Y. Usually the groups are formed by means of random assignment (randomized experiment), and they are under full control of the researcher in order to make the assumptions realistic (laboratory experiment).

**Q-type of qualitative research:** This is a research strategy using a low number of research units \((n = 1 \text{ to } 40 \text{ roughly})\). Qualitative labour-intensive methods of data gathering (open interview, observation) and of data analysis (confrontation, interpretation and coding) are used. It starts from an empirical rather than a theoretical basis, as is the case in mainstream research. Hence alternative names are *empiry-driven* research and *inductive* research. This primarily aims at internal validity and profoundness, rather than at external validity and width as is the case in a correlational design. The best known example of this class of research strategies is the grounded theory approach taken by Glaser and Strauss.

**q-type of qualitative research:** This class of research strategies is comparable to the Q-type, except the inductive and empiry-driven character. It resembles quantitative research in that it follows the same *deductive* type of reasoning, a *theory-driven* approach and the empirical cycle. It differs from quantitative research and it shares with the Q-type the use of qualitative methods for generating and analyzing the research material. Worth mentioning here are open forms of interviewing, observation and content analysis of textual and audio-visual media and documents. The most well-known variant of this strategy is the comparative case study (see Verschuren 2003).

**Explorative experiment:** This is small-scale research with \(n \approx 1 \text{ to } 20\), designed to follow an individual, a group or an artefact X through time, under strictly controlled circumstances. In principle it uses labour-intensive qualitative methods for observation and data gathering. In contrast to the testing experiment, in an explorative experiment (in principle) only one single research group is used. Its strength is the manipulation and control of the object of research by the researcher, as well as measurement under fully controlled circumstances. Besides developing the prototype for an artefact, this strategy may be used to open the black box of a causal mechanism \((X \rightarrow Y)\) that is found in a testing experiment (see Verschuren 2010 (forthcoming)).

**Knowledge-based research:** Two remaining research strategies I would like to identify as human resource-based strategies: knowledge-based and reflective research. The strategies mentioned above are all data-based, i.e. data are the input for analysis. In contrast, the inputs for human resource-based research are capacities of the human mind. In knowledge-based research the input is knowledge that exists in literature or in the heads of experts. By means of confrontation and analysis of these insights the researcher
tries to produce new knowledge. Examples are literature research, Delphi research, group model building and focus groups with experts.

Reflective research: Here, instead of data or existing knowledge, the mental capacity or brain power of the researcher is the basis. Although these matters of course play a role in any research, here they are cultivated as the only or the main basis for producing new knowledge and insights. Mental techniques that can be used are logical reasoning (deduction, induction, and abduction), introspection and empathy of the researcher, confrontation of ideas and objects, and the mental experiment. Confrontation is the comparison of two ideas or entities, asking what the implications are of the one for the other or for an external object (see Verschuren 2008). In all these cases the researcher him or her self is an instrument for both 'observation' and analysis.

Strategies, demands and types of research
The next question that needs to be answered is how these research strategies behave in front of the methodological criteria and demands in section 2. This question requires a confrontation as is symbolized in arrow a in Figure 1 on page 10. I answer this question by giving qualifications to the research strategies for each of the criteria and issues in Figure 2. The results are represented in Figure 5 below. The qualifications in this figure, although determined at face value, are firmly based on methodological arguments. The interested reader is referred to Verschuren 2009 for an overview of these arguments. Regarding the qualities of human resource-based strategies as yet there is relatively little experience and there has been done little methodological research. This explains why some cells are left open.

Figure 5 is divided into quadrants. On the one hand they are based on the division of the demands in internal and external scientific criteria and issues (see section 2). Roughly they relate to an epistemological and an implementary validity, respectively. On the other hand the research strategies are divided into quantitative strategies on one side, and qualitative and human resource-based strategies on the other. Figure 5 makes clear that the higher scores are found in the quadrants I and IV. It means that the quantitative strategies behave better as to the epistemological criteria, whereas the qualitative and human resource-based strategies perform better relating to the implementary criteria. This pattern is only broken by the high scores of the qualitative strategies for internal validity in quadrant II. The reason for these high scores is to a large extent to be found in the labour-intensive characteristic of these strategies. This makes possible a thorough generation of empirical material needed, as well as addressing problems such as social desirability, strategic answers and reactive behaviour of respondents. Within the qualitative strategies the q-type does slightly better than the Q-type. And the class of qualitative strategies has a slightly higher score than the human resource-based strategies.
The next question that needs to be answered is how important the demands are for the three main types of research, i.e. for theory-oriented, and the N-type and n-type of practice-oriented research (arrow b in Figure 1). To answer this question the scores in Figure 6 were produced, again at face value. For an explanation of these scores see Verschuren 2009; chapter 11. This Figure too is divided into four quadrants. Just as in Figure 5 the methodological demands are divided into the categories of internal and external scientific demands. And for the types of research the dividing line is drawn between theory-oriented research on the one hand, and the two types of practice-oriented research on the other. Again the highest scores are found in the quadrants I and IV. It tells us that the internal scientific or epistemological demands are the most important for theory-oriented research, whereas the external scientific or implementary demands are the most important for practice-oriented research. This, however, with the remark that this counts more for the n-type than it does for the N-type of practice-oriented research. From this it can be concluded that the biggest difference at this point is between theory-oriented research and the n-type of practice-oriented research, with the N-type
in between. Moreover, the scores in quadrant II are only slightly lower than those in quadrant I, telling that the epistemological criteria are only slightly less important for practice-oriented than they are for theory-oriented research. As quadrant III shows and in accordance with common sense, the implementary demands appear to have hardly any relevance for theory-oriented research.

Above we confronted research strategies and three main types of research with methodological demands. This makes possible logical conclusions about the confrontation of the main types with the research strategies. From Figure 6 we know that the epistemological criteria are the most important for theory-oriented research, whereas the implementary criteria are by far the most relevant for practice-oriented research. Moreover, Figure 5 makes clear that quantitative strategies best fit the epistemological demands, whereas qualitative and human resource-based strategies can cope best with the implementary demands. From this it follows that the quantitative strategies are most important for mainstream theory-oriented research, whereas the qualitative and human resource-based strategies are most important for the n-type of practice-oriented research, with the N-type of practice-oriented research in between. In Figure 7 we see scores for the seven research strategies separately. In the last column I have added at
face value scores for the frequency of use in research practice in the social, policy and management sciences. Looking at the first three columns we see confirmation of our conclusion above that theory-oriented and practice-oriented research are opposites, with the N-type of practice-oriented research somewhere in between. That is, the first two types are methodologically most different, whereas the N-type of practice-oriented research has some commonalities with theory-oriented research. The reason for the latter may be the large-scale of the target population in both cases, in combination with an emphasis on the external validity of research results that is strived for. And as we know from section 2 there are quite a few characteristics that are associated with the size of the target population.

Comparing the rows in Figure 7 to each other we can draw conclusions as to separate research strategies, relating to both the utility for the three main types of research and the question as to whether they are over or under-exploited. The utility of the correlational design is highest for theory-oriented research and weakest for the n-type of practice-oriented research, with the N-type in between. Given the fact that it is the most used design in the social, policy and management sciences, we can say that there is a right exploitation of it as to theory-oriented research; utility and actual use are in balance. However, seen from the perspective of the n-type of practice-oriented research there is an over-exploitation of this research strategy.

As to the testing experiment in row 2 of Figure 7, this strategy is useful for the n-type of practice-oriented and for theory-oriented research. It seems to be second best for the N-type of practice-oriented research. As this design is not used very much in the practice of social, policy and management science research, this is an instance of under-exploitation of the n-type research.

Having been developed just for theory building, the Q-type of qualitative research has most relevance for theory-oriented research. As it is little used in practice we can
qualify this as a case of under-exploitation in the domain of theory-oriented research. Quite different is the position of the q-type. This appears to be most relevant for the n-type of practice-oriented research, and only moderately relevant for the other two. As this strategy is relatively little used in practice, this means that it is under-exploited in the n-type of research. A great deal can be gained by using this strategy more often in organizational research.

Roughly the same goes for the explorative experiment. It’s most relevant for the n-type research. For instance, it is an excellent means to test the prototype of an artefact (see below). But it can also be used for formative evaluation of a new policy in an N-type context. This strategy may also have some relevance for theory-oriented research. More specifically it can be exploited for studying causal mechanisms. That is, once a causal effect of X on Y has been proven by means of a testing experiment, the explorative experiment may be used to open the black box of this causal mechanism. It thus acts as a remedy against the limitations of the black box character of the class of testing experiments. As the explorative experiment is hardly known in the social sciences, this is an instance of overall under-exploitation.

Roughly the same goes for the two human resource-based strategies. The knowledge-based research is most useful for the n-type practice-oriented research, and a little less for the N-type. As it in practice is little used this again indicates under-exploitation. The score pattern for reflective research looks like the one of knowledge-based research, albeit that overall the scores of the first are slightly lower. The reason for this may be that the trustworthiness of the results may be a little lower. The results of a knowledge-based research are the product of a team of interacting experts. In contrast, reflective research in principle is based on the input of the researcher only, which gives fewer guarantees as to the reliability of the results. It seems to be under-exploited in the n-type of research.

**Strategies, intervention / design cycle, and types of research**

The next issue to be elaborated in this section about reallocation of research strategies is what specifications of the above conclusions are needed when we look at improvement and construction problems separately (arrow d in Figure 1). I also will briefly evaluate the use that the N-type and n-type of practice-oriented research make of the intervention and design cycle (arrow e). To answer the first question, stages of the intervention cycle and the design cycle were confronted with the research strategies. The results of this confrontation are the scores in Figure 8. For an explanation of these scores the reader is again referred to Verschuren 2009, chapter 11. There are two ways to interpret and evaluate the score patterns: (a) To compare the columns within the intervention cycle and the design cycle separately. This will give an idea of the relative utility of research strategies for resolving improvement and construction problems, respectively. (b) Comparison of columns in the one cycle with the corresponding columns in the
other cycle. This will give insight in the differences in the utility of research strategies for resolving improvement problems and construction problems, respectively.

Comparison (a): A remarkable feature of the upper half of Figure 8 is the weak position of the Q-type of qualitative research. This stands to reason, as this strategy was developed for theory construction. One of the most salient deficiencies of this strategy for solving practical problems is the lack by definition of a clear research issue at the beginning of the research project. In contrast, of all seven strategies the q-type appears to be the most useful for resolving improvement problems. Second best in this respect are the explorative experiment, the correlational research and knowledge-based research. The latter may be of particular use in developing a plan for an intervention. It may also be exploited for an analysis of the problem to be solved. Of relatively little use for addressing improvement problems are, besides the Q-type, the testing experiment and reflective research.

In the bottom part of Figure 8 we see that by far the most useful strategy in addressing construction problems is knowledge-based research. In particular, in the stages (1) first hunch, (3) defining structural specifications, and (4) developing a prototype, this strategy can play an important role. Besides, both the q-type of qualitative research and reflective research offer good perspectives here. Another remarkable conclusion is that empirical or data-based research strategies are of little use here (see the first five columns). This is in accordance with the fact that construction problems are about the creation of a new reality, rather than improving an existing one. Exceptions relate to the stages 2b, 5 and 6. In stage 2b we need empirical research for registration of the design assumptions. And in the stages 5 and 6 we need empirical data for a formative and summative evaluation of the prototype and the artefact, respectively. This relatively weak position of empirical research goes hand in hand with a strong position of the human resource-based strategies. The reason is that logical analysis, deductive reasoning, as well as creativity are important ingredients here (see section 4).

Comparison (b): Comparing the first five columns in the upper and bottom half of Figure 8 shows, in accordance with the last conclusion above, that empirical research plays a much more important role in addressing improvement problems than it does for construction problems. The reason was given above. Conversely human resource-based strategies play a much more important role for construction problems than they do for improvement problems. An explanation again was given above. Another remarkable feature is that the q-type of qualitative research does a good job in both cycles. This is in accordance with an earlier conclusion that this type of research is very useful for practice-oriented research in general. The same goes, although slightly less, for the human resource-based strategies. In particular, knowledge-based research appears to be very useful for both cycles.

The final question that needs to be answered in this section is whether in practice-oriented research thus far is made an adequate use of the intervention cycle and the design cycle (see arrow e in Figure 1). The result of this confrontation is presented in
Intervention cycle | correl. | t-exper. | q-type | Q-type | e-exper. | kn. bas. | refl. res.
--- | --- | --- | --- | --- | --- | --- | ---
1 problem analysis | + | - | + | + | + | + | +
2 diagnosis | + | + | + | + | + | + | +
3 conditions for solution | + | + | + | + | + | + | +
4 plan for intervention | + | + | + | + | + | + | +
5 execution of the plan | + | + | + | + | + | + | +
6 evaluation | + | + | + | + | + | + | +

Design cycle

1 first hunch/design goal | - | - | + | + | + | + | +
2a design demands | + | - | + | - | + | - | +
2b design assumptions | - | + | + | + | + | + | +
3 structural specifications | - | - | - | + | + | + | +
4 prototype | - | - | - | - | - | + | +
5 implementation | - | - | - | - | - | + | +
6 evaluation | + | + | + | - | + | + | +

Figure 8. Utility of research strategies for different stages of the intervention cycle and the design cycle for solving improvement problems and construction problems.

Figure 9. Importance and actual use of the intervention cycle and design cycle in theory-oriented, and the N- and n-type of practice-oriented research. - = not important / not used, ++ = very important / very much used.

Figure 9. At face value and based on logical and methodological considerations, scores were given for the importance of the two cycles for the three main types of research. Besides, at face value estimated scores were given to the actual frequency of use of these cycles. As Figure 9 shows and corresponding to common sense, the cycles are of no importance to the theory-oriented research, for the simple reason that there is no practical problem to be solved. Correspondingly there is neither any actual use of these cycles in this domain of mainstream research in the social, policy and management sciences.

In policy research (N-type) the intervention cycle, also called the policy cycle, appears to be very important. And it rightly is much used here. However, despite the
frequent use of the intervention cycle in public policy, there is some criticism as to developing research methodology in this domain. As Figure 4 shows the intervention cycle has six stages, of which the last is evaluation. The remarkable thing is that research for exclusive support of this last stage, i.e. evaluation research, is very well methodologically developed. A huge amount of books and articles are found in this domain (see, for instance, Algemene Rekenkamer 2002, Furubu et al 2002, House 1993, Mohr 1995, Owen 1999, Patton 1997, Pawson and Tilley 1997, Rossi et al 1998, Shadish et al 1995).

But for the other five stages there hardly anything is to be found in methodological literature. For that reason as yet, much to its detrimental, real policy research (as contrasted to policy analysis; see section 2) to a large extent coincides with evaluation research.

In contrast, the design cycle is of less use for policy research, as it aims at radical changes. As is pointed out in section 2 radical changes in principle are to be avoided in this N-type context. So, in practice the design cycle is rightly little used here.

As to organizational research (n-type) there is very little use of both generic models for problem solving as a starting point and guidance for the researcher (let alone for the research methodologist). Neither there is much usage of or reference to the vast literature on evaluation research here. All of this is detrimental to organizational research.

6. BLANK SPOTS IN RESEARCH METHODOLOGY

Two questions may be asked after the analyses thus far: (a) why there was and is still so little attention for implementary validity in research and research methodology? (b) Does an implementary validity besides epistemological validity cover all of the responsibility of a practice-oriented researcher? The first question will be answered below. Answering the second is postponed to the next section. A first and basic answer to the first question can be found in a broader development in science, i.e. neglect and even banishment of values from science. This is part of the positivistic tradition in science, with its doctrine of non-normative. In this section I describe how this doctrine led to a predominance of the context of justification and a disregard for the context of discovery. Besides, the introduction of a context of implementation is advocated. I will take Figure 10 as a guide line, with its overview of the contexts of discovery, justification and implementation, as well as a number of directly related issues. It shows the connections of these contexts with the design of research and with the different forms of validity as to be met in several parts of the research design. Moreover it shows which deficiencies an over-emphasis on the context of justification may, and actually does cause, as will be argued below.

Predominance of the context of justification

In answering the question what the value is of scientific research, there are at least two sub questions to be addressed: Are we researching (a) the right thing, and are we doing that (b) in the right way? The first sub question lays in the context of discovery and the
second relates to the context of *justification*. Although this distinction is very well known in traditional research methodology, the context of discovery is a blank spot. The same goes for a third newly to distinguishing type of context, the context of implementation (see column I of Figure 10). Its necessity will be argued below.

As columns II and III of Figure 10 show, the three contexts are closely linked to three parts of the design of a practice-oriented research: the *conceptual*, the *technical* and the *implementation* design, respectively. (a) The conceptual design gives an answer to the question what is the object of research. It's divided into the practical problem to be solved and, derived from this, the *knowledge* problem. The latter relates to the research question(s) and their link to existing theory. It is true that in a conceptual design the researcher has to recognize the context of justification, in so far that it must be feasible to carry it out. And designing it in a methodologically adequate way too is a matter of justification; the snake swallows its own tail (see for these arguments Verschuren 2009, chapters 6 and 7). But by far in main part making a conceptual design for a research is in the context of discovery. (b) The technical design tells us how the research will be carried out, divided into the stages of generating the research material (data-gathering) and the analysis of this material (data analysis). These activities relate to the context of justification. (c) The implementation design involves advices as to the usage of the produced knowledge, the process of problem solving, the fit to local conditions and a formative evaluation of the intervention or artefact. The first two sub-designs (a) and (b) are known from literature (see Verschuren 2007 and 2009). The implementation design and context of implementation are new, and are proposed below as important supplements to the methodology of practice-oriented research.

In column IV of Figure 10 are indicated those domains in research methodology that primarily spent time and energy to (some of) the above-mentioned areas. Over the last half century by far most methodology has been developed by qualitative and especially quantitative researchers and research methodologists. However, almost all their methodological publications are in the context of justification, and thus exclusively relate to the technical design of research. Nevertheless, some effort was and is still being spent on the context of discovery. Under the heading of problem structuring quite a bit of work has been done by participatory researchers and methodologists as to that part of the conceptual design that relates to the practical problem that needs to be solved. See Hindle and Franco 2009, Franco 2008, Franco and Meadows 2007, Mingers and Rosenhead 2004, Rosenhead and Mingers 2002, Keys 2006, Weber and Khademian 2008, Winter 2006. But this is only a minor part of the total investment in research methodology in the social, policy and management sciences. In sum this means that little effort was spent by methodologists to subjects such as the choice of the object of research, methods for the analysis of the problem to be solved, i.e. problem analysis, ways of theory building and, last but not least, selection and formulation of research questions and hypotheses.
In the bulk of research methodology, i.e. the methodology for quantitative research, almost all the attention was and is still paid to exploring and developing methods and software for data analysis. Apart from this neglect of the conceptual design, here relatively little effort was spent on methods and procedures for observation and data gathering. In the methodology for qualitative research we see the reverse. Relatively speaking, a great deal of attention is paid to observation and data gathering and relatively little to analysis of the research material (see also van de Vail 1980: 141). The same goes for ignoring methods for adequately reporting research results (van de Vail 1980: 141) and implementing this knowledge for problem solving or the construction of an artefact.

One of the consequences of this overdevelopment of the context of justification is that little has changed in the educational programmes in research methodology at universities over the last forty years. This education consists partly of courses in statistics. In almost all disciplines in the social, policy and management sciences statistics plays a considerable role in the education in the first two years of the Bachelors programmes. Hardly anything has changed in the basics of statistics. In contrast, when it comes to methods of (multivariate) data analysis, a great deal has changed. During the last four decades several methods and software were added or improved for complicated forms of multivariate analysis that can handle problems such as non linearity, low levels of measurement, longitudinal data, autocorrelation and multicollinearity. However, most of them are too sophisticated to be introduced in the first two years of the Bachelor’s
stage at universities. At the same time in most faculties in the Netherlands there is hardly any education in research methodology in the third year. The consequence of all this is that very little of what has been developed in research methodology during the past half century has become part of the curriculum. Research methodology that was presented to me when I was a student in sociology in the 1960s scarcely differs from what we offer students now.

The predominance of the context of justification at the expense of the contexts of discovery and implementation puts limits to the equipment of our present day students. This is all the more a problem because by far most of them will be involved in practice-oriented research once they leave university, rather than in theory-oriented research. And the biggest difference between these two types of research is just to be found in the context of discovery and, by definition, in the context of implementation. Students miss training in problem analysis, in selecting the right theories and concepts, in formulating methodologically adequate research questions, in the implementation of knowledge and related issues, such as manoeuvring in a political context, let alone in using the criterion of ethical validity (see section 7). Most of the effort, by far, is spent on methods of data gathering and data analysis.

*Disregard for the contexts of discovery and implementation*

The predominance of the context of justification not only led to deficiencies in what we teach our students. As we saw above, it also led to blank spots in developing research methodology. This can be further demonstrated on the basis of Figure 10. Column V shows the general criteria to be met. From the predominance of the context of justification we may conclude that there is a predominance of an epistemological validity and a neglect of ethical validity. The reason for this is that the latter mainly belongs to the contexts of implementation and especially of discovery. This does not imply that ethics has nothing to do with the way research is carried out, i.e. the context of justification, on the contrary. It means that it does not belong to the domain of ethical validity of knowledge as such, but to the area of the production of knowledge.

A neglect of ethical and implementary validity is a consequence of positivism in science, with its doctrine of non-normative and its banishment of values from science. An indication for these blank spots is the bare fact that researchers and methodologists use the word validity as by far the most important scientific criterion. It automatically means *epistemological* validity and takes as a touchstone correspondence with empirical reality. Not using an adjective for validity they express the implicit assumption that there is only one relevant touchstone for determining the quality of knowledge. They thus ignore implementary and ethical validity, based on two other important touchstones, i.e. the utility and moral correctness of knowledge, respectively. The focus on sensory observation and epistemological validity is understandable from the perspective of the tough and long-lasting process of detachment of science from religion and
metaphysics in the eighteenth and nineteenth centuries. But, once empirical verification has a firm basis in research and research methodology, there is no longer reason to ignore other criteria for validity besides epistemological validity. Below I will briefly reflect on the neglect of implementary validity, as well as deficiencies in its development. In the next section I will elaborate on the criterion of ethical validity.

As we already saw, almost thirty years ago van de Vail put on the agenda of researchers and research methodologists the implementary validity of knowledge. However, as far as I can see he, totally in line with the overemphasis on the context of justification of mainstream social sciences, only aimed at the context of justification part of it. This is also the part that I discussed in the sections 2 and 5. However, as column V of Figure 10 shows, implementary validity is determined by decisions in all three contexts, i.e. the contexts of discovery, justification and implementation. That is, utility is not only a matter of justification. On the contrary, even more important is selecting the right object of research and asking the right questions about it. So, implementary validity deserves a firm place in the context of discovery, besides one in the context of justification. And this is not all we can say. Apparently utility of knowledge is also a function of the way this knowledge is used and works out in the local context of the problem to be solved. So we still need to address an implementary validity in a literal sense. That is, although the traditional splitting up of the world of research methodology in a context of discovery and a context of justification holds for mainstream theory-oriented research, it does not for practice-oriented research. This is a consequence of the fact that an implementary validity is not simply the result of researching the right thing (discovery), in the right way (justification), it also depends on an adequate implementation and use of this knowledge.

Introducing a context of implementation uncovers another directly related deficiency of traditional research methodology. We still lack a thorough methodology for building an implementation design, besides a conceptual and a technical design (see Figure 10), that tells how the knowledge to be produced must be implemented, and/or how it can be used for tackling the problem at hand. There are several arguments to take the context of implementation to the domain of the researcher, instead of only to the one of the problem solver. The first is that most often the results of a practice-oriented research are not a set of ready-made rules and instructions how to solve the problem. Rather it is knowledge that sheds light on the problem to be solved and on the stages of the intervention and design cycle. So this knowledge needs to be translated into an intervention or an artefact. For this translation it is often important to know what assumptions were made by the researcher, as well as the reach and limitations of this knowledge that he or she perceives.

A second reason is that during the process of carrying out the research the researcher gathers all kinds of tacit knowledge. This is knowledge where he or she is not looking for, but that nevertheless is gained as an unwanted, and often unconscious by product
of the research project. This knowledge, however, in general is important for tackling a problem. A third and final reason is that, if everything works out well, a practice-oriented researcher is trained in intervention and design methodology. Amongst others this relates to the use of the intervention cycle and the design cycle, and insight in the way the knowledge produced fits to the several stages of these cycles.

To sum up, the blank spots in research methodology relate to the contexts of discovery and implementation, and the related issues in the conceptual and implementation design of a practice-oriented research. Moreover, overstressing the context of justification went hand in hand with a neglect of ethical validity of knowledge, the latter primarily being a function of the contexts of discovery and implementation, rather than of the context of justification. More about this in the section to follow.

7. DISREGARD FOR ETHICAL VALIDITY
The banishment of values from science by the positivists was not only detrimental to developing (a methodology for) implementary validity. It also led to neglect of ethical validity as an important criterion for scientific products. Certainly, there never was any ignorance about moral correctness as a criterion for the way the researcher carries out the research (justification). For instance, there are strict norms for doing experimental research. These norms say that experimental subjects should be treated well and honestly, that the experiment should not be to their disadvantage, and that if they run any risk the researcher should follow the principle of informed consent. If he or she does not follow these ethical rules, there will be debate in the media and rejection by colleagues.

It's striking that here once more there is a one-sided attention for the context of justification (see, for instance, Swanborn 2009). As to the moral correctness of the results of scientific research there for a long time has been no debate in the social, policy and management sciences. In the middle of the last century there was such a debate, when nuclear physics was developed. The question was raised as to whether it was permitted for scientists to produce knowledge that could be used to construct an atomic bomb. The outcome of this discussion was the general standpoint that science can't be blamed for this and that it is the responsibility of those who use the knowledge for the production of such an evil device. This argument is based on the distinction between the producer and the user of scientific products. The responsibility was placed clearly on the shoulders of the second.

This separation of the roles of producer and user of scientific knowledge is reasonable for theory-oriented research, as it deliberately has no external goal. But once there is such a goal, as is the case in practice-oriented research, this separation becomes less clear. Here in the end the researcher decides what can or should be done in addressing the problem at hand. What would then be the argument for saying that here the researcher has no responsibility relating to the effects of her or his advice?
This responsibility as to the value of the products of practice-oriented research has two aspects: instrumental and moral correctness. They correspond to implementary and ethical validity, respectively. The instrumental correctness involves the question as to whether the knowledge produced actually contributes to the solution of the problem at hand. There is some tendency in society and in legal practice as well to make this a point of duty for the researcher. However, relating to the moral correctness there is as yet not such a development. The criterion of ethical validity states that the knowledge produced should not contradict profound values, such as the values of freedom, security and absence of violence, sustainability of the environment, health and the like. Below I will elaborate on the role that ethical validity plays or must play in practice-oriented research.

As already stated, a neglect of ethical validity makes part of a more general development in science, i.e. non-normativity and a neglect of values. Below I will briefly describe four well-known issues in social science where values play or should play an important role, and where neglect of this role may easily, and in practice does, cause problems with implementary and especially with ethical validity. These issues are: (a) single-loop versus double-loop learning, (b) goal-based versus goal-free evaluation, (c) wants and demands versus needs of a target population and (d) the F-side and D-side of practical problems. Every issue contains two variants or components, where the second variant or component requires confrontation with values, and where without this confrontation may easily raise problems with ethical validity. These issues show that there is a general reluctance in present day practice-oriented research to consider deeper values and ethical validity. Correspondingly there is more attention for the first variant/component. Before elaborating these issues the reader is warned that they only represent clear and straightforward examples; problems with ethical validity may play a role in many aspects and stages of scientific practice-oriented research.

(1) Single-loop versus double-loop learning: These concepts have a direct link with the context of justification and the context of discovery, respectively. Single-loop learning is about the question: Are we doing things right? Double-loop learning aims at answering the question: Are we doing the right things? Part of the last issue is the question as to whether we are striving for the right goals. An adequate answer to this question is only possible with reference to profound values such as the value of well being, of equality or of freedom. A schematic picture can illustrate the difference between single and double-loop learning (see Figure 1 below).

Three issues are closely related to the phenomena of single and double-loop learning: escalation of commitment, searchlight bias and opportunity costs. The three of them belong to the context of discovery, and they in principle are suspected for causing problems with ethical validity. Escalation of commitment is the tendency to stick tighter to a certain goal with an increase in the amount of investment, even if the investments do not appear to be efficient for achieving this goal. The reason for this behaviour is
that the loss of giving up the goal becomes bigger as the amount of investments increases. It stands to reason that this behaviour easily may counteract ethical validity, as it prohibits debate on both the goals and the allocation of efforts. It requires double-loop learning to uncover this mechanism.

Another important threat to striving for the right goals is what I call the searchlight bias. It’s the bias of looking primarily at those things for which we have suited or cherished methods. This problem is not imaginary, given the overemphasis on the context of justification in present day science. The essence of this problem may be illustrated by the metaphor of the drunk who lost his key at night. He was looking at it in the light of a lamppost, not because he lost it there, but because there was light. This mechanism too may be a threat to ethical validity, as it draws attention away from a critical evaluation of the goals of goal-oriented human behaviour, such like strategic management and public policy.

There is a particular danger of searchlight bias in cases that research strategy appears to be successful for a certain type of goal. This may cause a tendency to use it blindly for all kinds of other goals, even if it proves to be less effective. Another reason for searchlight bias may be the existence of a paradigm. If researchers make part of a group or context where there is a certain strong belief, for instance in quantitative research strategies, it is very difficult to deviate from this norm. For more information on searchlight bias the interested reader is referred to Verschuren 2009. Again we need double-loop learning for avoiding problems with or repairing ethical validity.

A concept that is closely linked to searchlight bias is the concept of opportunity costs from economists. A well-known criterion for the adequacy of an investment is efficiency, which means that we maximally achieve our goals with a minimum of costs, including negative side-effects. However, striving for efficiency is an instance of single-loop learning. Striving for minimizing opportunity costs means that we avoid investments in a goal, whilst striving for another goal would have given a higher gratification or a morally more acceptable or higher prioritized goal. This too requires double-loop learning and consideration of ethical validity.

(2) Goal-based versus goal-free evaluation: Closely related to the distinction between double and single-loop learning is the distinction between goal-based and goal-free
evaluation. Mainstream evaluation is *goal-based*. That is, over ninety percent of all evaluation research is of this type. It involves the judgment of an intervention (public policy programme, strategic management) or an artefact, with the goal of the intervention or artefact as a criterion. The intervention or artefact is said to be a success to the extent that (a) its goals are achieved and (b) this goal achievement is a consequence of this intervention or artefact. This is a matter of effectiveness and implementary validity. Goal-free evaluation, however, takes one or another value as a criterion for evaluation. This value may be, for instance, the well-being of the stakeholders, sustainability of the environment or socio-political stability in society. Here ethical validity is at stake. It stands to reason that a goal-based evaluation may turn out positive, whereas a goal-free evaluation of the same intervention or artefact is negative and vice versa, depending on the chosen value(s). This demonstrates the relevance of ethical validity besides implementary validity, as a criterion for the value of the results of scientific practice-oriented research (see also Verschuren and Zsolnai 2001).

The choice of a value as a criterion for goal-free evaluation is a matter of politics and ethics; it can't be made on purely scientific (read epistemic) grounds. There are two relevant and well-established traditions in ethics, i.e. *consequentialism* and *deontology*. Consequentialism bases the evaluation of an intervention or an artefact purely on its actual consequences. The conclusion of such an evaluation can be positive if the intervention or artefact meets needs, wants or ideals of the stakeholders. Deontology takes as a criterion for evaluation the correspondence of the intervention or artefact with ethical norms. In the first case there in principle is an *instrumental* criterion, whereas in the second case a *normative* standard is taken as a touchstone. The instrumental character of the first may be a problem. This means that in a consequential view an object can have a positive evaluation, and at the same time in a deontological assessment have a negative evaluation.

(3) *Wants* and *demands* versus *needs* of the target population: In the domain of somatic and psychosocial healthcare there is a theory of demands of clients that makes a distinction between needs and wants (Bradshaw 1972, Rijckmans 2005). A need is what professionals think is good for the client, whereas wants relate to what the latter himself thinks is good. And demands are that part of the wants where the client actually asks for. Wants and demands are based on subjective and mostly implicit perceptions, feelings, opinions and evaluations of the client. Needs are based either on professional, i.e. medical and psychosocial scientific, standards or on ethical norms such as health, well-being and fairness. The first case is an instance of consequentialism, whereas the second is in the domain of deontology.

This theory of health care can be used as a metaphor of the relationship between a practice-oriented researcher and a client or target population. If the client defines the research issue differently from what the researcher thinks is needed for the client, the latter has an ethical problem. Shall he or she follow the client, who in the short term
will be satisfied with this consent, or does he or she persevere in what he or she thinks is needed? In the latter case the client will be dissatisfied at that very moment, but in the long run this evaluation may positively change, if the opinion of the researcher turns out to be right. In any case the researcher must follow the principle of informed consent. That is, he or she only accepts the standpoint of the client after he or she has explained what the needs are following professional or ethical standards. There is a strong tendency in practice-oriented research to follow the demands of the client without any debate. A researcher who sticks to professional or ethical needs can easily be regarded as intrusive. See for more information about this metaphor Verschuren 2008.

(4) The F-side versus the D-side of a problem: As we saw in section 4 a practical problem can be defined as a contradiction between a factual (F-side) and a desired state of affairs (D-side). This desire may either relate to something to be achieved or to be avoided or prevented. This has a link with the distinction between an improvement problem and a construction problem. To define something as an improvement problem means the prevention or avoidance of bad results, which is an instance of reactive behaviour. Defining a construction problem means an endeavour to achieve something valuable, which is proof of proactive behaviour.

A serious problem in practice-oriented research is that very often practical problems are only defined in terms of the F-side. For instance, a national government puts the problem of a high rate of immigration on the agenda. However, a high rate of immigration is just a fact. It says nothing about a problem. The question here is what in this respect the government considers to be a good or desirable state of affairs. No immigration at all? A lower rate of immigration? And, if so, how much lower? Should there be other rules for immigration? If so, which rules? A different distribution of immigrants over the country? Less concentration in certain local areas? Other - or more stringent - conditions for immigrants to meet? And which conditions? The reason why the D-side is so often not recognized is again the fact that this requires the choice of a value and thus taking the issue of ethical validity into consideration.

Ignorance of the D-side is a serious drawback for problem solving. The reason is that the way a problem is tackled very much depends on the way the D-side of a problem is defined. For that reason the lack of a definition of the latter sooner or later will hinder a satisfactory resolution of the problem at hand, which actually in practice is often the case. Without this definition it is not even clear what the problem is (see Verschuren 2008). Here too the distinction between an instrumental and a normative touchstone is at stake. That is, the D-side of a problem definition may refer to either instrumental or ethical reasoning. The first case is an instance of an instrumental problem, whereas in the second case I call it a normative problem (see Verschuren 2008).

With the exception of the couple goal-based versus goal-free evaluation, the above issues are especially relevant for the first stage of the intervention cycle, the stage of problem analysis. They all relate to the context of discovery. More specifically they can
be applied for answering the question what will be taken as the object of the research and which research questions will be formulated. The issue of goal-based versus goal-free evaluation of course can be used in the last stage of both the intervention and the design cycle. In all these cases the criterion of ethical validity plays or should play an important role.

A final distinction that can be made in this respect relates to the reach of the criterion of ethical validity. In correspondence with a universal distinction as to (epistemological) validity, we can divide ethical validity in an internal and external ethical validity. Internal ethical validity answers the question as to whether the knowledge comes up to the benefit of the stakeholders. This may also be called the needs of the stakeholders, as distinguished from their wants and demands (see point 3 before). External ethical validity relates to the question as to whether the knowledge produced helps a general well-being and a sustainable arrangement of society and its individual members. This may also be called the social relevance of the knowledge, as distinguished from a theoretical and a methodological relevance. The criterion of external ethical validity is not only relevant for practice-oriented research, but also for theory-oriented research. But the internal ethical validity is a criterion that is specific for practice-oriented research.

8. CONCLUSIONS AND FURTHER DEVELOPMENTS
A last question that needs to be answered is what consequences all this has for research methodology in the domain of practice-oriented research. There are three possible types of consequences to be addressed: (a) Reallocation/adaptation of existing research strategies and methods, (b) Further development/extension of these strategies and methods, (c) Development of new procedures, methods and strategies (see the left column of Figure 13). The first two are improvement problems, whereas the latter is an instance of a construction problem. To explore these consequences four types of information are needed. (1) The importance of existing research strategies for practice-oriented research, which comes down to the opportunity they offer for achieving firm implementary validity. (2) The degree to which these strategies are actually used in social, policy and management science research. (3) The degree to which these strategies are developed and articulated in research methodology. (4) Blank spots in existing research methodology, seen from the standpoint of practice-oriented research. The first and last type of information can be deduced from the sections 5 and 6, respectively. The first three points of information above relate to existing methodology, point 4 relates to developing new methodology.

As to the first three points, I will distinguish four main categories of research strategies: quantitative, qualitative, knowledge-based and reflective research. From the category of quantitative research I exclude the testing experiment. The reason is that it not necessarily is quantitative. Besides, it differs methodologically considerably from correlational research. Thus below quantitative research coincides with correlational
WHY A METHODOLOGY FOR PRACTICE-ORIENTED RESEARCH IS A NECESSARY HERESY

<table>
<thead>
<tr>
<th>Degree of...</th>
<th>Type of research strategy</th>
</tr>
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<tbody>
<tr>
<td></td>
<td>quantitative</td>
</tr>
<tr>
<td>1. importance for implementary validity</td>
<td>-</td>
</tr>
<tr>
<td>2. actual use in mainstream research</td>
<td>++</td>
</tr>
<tr>
<td>3. methodological development</td>
<td>++</td>
</tr>
</tbody>
</table>

Figure 12. Degree of (a) importance for implementary validity, (b) actual use and (c) methodological development of a quantitative, qualitative, knowledge-based, and reflective research strategy. - = low degree, ++ = very high degree

<table>
<thead>
<tr>
<th>Change in relation to methodology</th>
<th>Confrontation</th>
</tr>
</thead>
<tbody>
<tr>
<td>a. reallocation/adaptation</td>
<td>importance → actual use</td>
</tr>
<tr>
<td>b. further development / extensions</td>
<td>actual use → development</td>
</tr>
<tr>
<td>c. development of new procedures</td>
<td>blank spots → needs</td>
</tr>
</tbody>
</table>

Figure 13. Confrontations for inferences about desired changes in research methodology

From the category of qualitative research I exclude the Q-type. The reason for this is that it was exclusively developed for theory-oriented research. Thus its relevance for practice-oriented research is low, or at least fuzzy.

On the basis of the analyses in section 5 the first three categories of information above were provided with scores (see Figure 12). Category (d) concerning blank spots will be elaborated later on. For more detailed scores relating to row 1 of Figure 12, see Figure 5. As this row shows, the qualitative type of research offers the best perspective for implementary validity. Second best is knowledge-based research, third best reflective research, while the quantitative type of research seems to offer the least perspective in this respect. The low score for the actual use of qualitative research in row 2 may strike the reader as strange, but this is the result of eliminating the Q-type. Compared to the other variants of qualitative research, the Q-type is used quite often.

By means of three confrontations an answer can be given to the question what kind of consequences for research methodology can be derived from the analyses in the previous sections. These are indicated in the right column of Figure 13. From the first confrontation we can conclude whether a strategy is over or under-exploited. Because the actual use to a large extent relates to mainstream (theory-oriented) research, this may provide an inference as to the demand for reallocation of research strategies seen from the standpoint of practice-oriented research. From the second confrontation of actual use with the degree of development may be derived whether research strategies deserve further methodological development.
As to the first confrontation, we initially perceive that quantitative research is widely used in mainstream social, policy and management sciences, whereas its importance for achieving implementary validity is very low. Seen from the standpoint of practice-oriented research this indicates over-exploitation. The inverse is true for qualitative research strategies. Their importance is very high, but actual use is low; a case of under-exploitation. The same goes for knowledge-based research; it combines a pretty high importance with a low frequency of use. Much is to be gained by reinforcing both the methodological soundness and its actual use in practice. Finally, reflective research is slightly under-exploited. From Figure 12 it may be concluded that with respect to practice-oriented research some reallocation of research strategies is needed. This comes down to the advice: less use of quantitative research, in favour of (in order of increasing importance) reflective, knowledge-based and qualitative research.

As to the second confrontation, what strikes one first is the similarity of the scores in row 2 and 3 of Figure 12. This indicates that, as one would expect, the methodological development of research strategies is roughly in step with their actual use. This pattern is only slightly broken by reflective research. The reason for this may be that reflection is a universal part of science and scientific research that already has a very long tradition. This does not detract from the fact that existing forms of reflection can be improved and adapted to include a focus on implementary and ethical validity and that new forms could be developed by methodologists. Seen from the perspective of practice-oriented research, actual use should be upgraded to the level of the importance scores in the first row of Figure 12. The scores in the third row suggest that more methodological investment is needed to further develop qualitative, knowledge-based and reflective research strategies, at the expense of investments in the methodology of quantitative research.

So far we have focused on the tendency to reallocate or further develop/extend existing research strategies and linked worthwhile investments by researchers and methodologists. A final question that needs to be answered on the basis of the analyses in the preceding sections is which gaps remain in research methodology and thus which new methodologies are needed. This question is closely linked to convictions in mainstream social, policy and management sciences. That is, as is discussed in sections 6 and 7, there has for decades been an overemphasis on just a few domains in research methodology and thus which new methodologies are needed. The left column in Figure 14 provides an overview of the most important doctrines, seen from the standpoint of practice-oriented research. In the middle column we see their counterparts: important issues for practice-oriented research. In mainstream theory-oriented research these are often regarded as heresies. These heresies precisely indicate which new developments in research methodology are needed, seen from the perspective of practice-oriented scientific research. So columns 1 and 2 in Figure 14 elucidate the third confrontation in Figure 13: blank spots versus needs. At first sight,
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1. context of justification
2. epistemological validity
3. quantitative research
4. reductionism
5. specialization
6. generalizability
7. empirical research
8. the empirical cycle

The number of heresies looks pretty high, but these all are closely related and should be seen as comprising a syndrome.

The right column of Figure 14 shows the consequences for methodology. Here the three categories mentioned at the beginning of this section are used: reallocation of existing methods and strategies, extension of these methods and strategies, and development of new criteria, procedures, methods and strategies, indicated as new development.

A first doctrine relates to the exclusive relevance of the context of justification. Roughly estimated over ninety percent of the publications in settled research methodology are in the domain of data gathering and especially data analysis, including such issues as sampling, external and internal (epistemological) validity and supporting software. In order to reinforce the power and effectiveness of practice-oriented research, more investment of researchers and methodologists is needed in the contexts of discovery and implementation. Progress can be made through analytical philosophical reflection of issues such as: the relationship between truth, utility and moral correctness, i.e. between epistemical, implementary and ethical validity, as well as between specialization, holism and interdisciplinarity, to mention just a few.

Many scientists are convinced or make the implicit assumption that developing methodology, i.e. procedures, methods and supporting software, is only possible in the context of justification, i.e. for data gathering including sampling, and data analysis. This is evidently not true. Perhaps the reason for this erroneous idea is that in the context of justification we can often make use of numerical scores and numbers for counting. However, there is no reason why we can’t create methodological guidelines, procedures, methods and software for conceptual issues as these are often at hand in the contexts of discovery and implementation. The only difference will be that they have the form of heuristics, rather than of algorithms as is the case with quantitative data analysis. There
are two differences between these approaches. In the latter case, the assumption is that there is only one correct solution or outcome, whereas in the case of a heuristic there is no single right answer. Secondly, in an algorithm the steps are fixed in advance and must be followed in a prefixed order, whereas a heuristic is more like a searchlight, offering the researcher much more degrees of freedom.

A second mainstream doctrine is the primacy of epistemological validity. Although its importance can by no means be denied, because of the centrality of its empirical basis, it draws attention away from implementary and ethical validity. For real progress in practice-oriented research, we need more investments in these domains by research methodologists and by philosophers of science specialized in ethics. These criteria must not only be further explored. Procedures, methods, strategies and software that meet these criteria must also be developed.

Another conviction in mainstream science is the primacy of quantitative research and related analytical reductionism in science. This type of research requires the research object to be unravelled into observation units and variables. Although this makes a thorough analysis and internal as well as external (epistemologically) valid conclusions possible, it raises the question as to whether we are losing sight of an overall view. To adequately intervene in reality we need this view of the object as a whole. This is one reason for an earlier conclusion that qualitative research which fits a holistic approach is often more valuable for practice-oriented research than quantitative strategies, other things being equal. However, much greater investment is needed in the methodology of qualitative research (see section 6).

Roughly the same goes for hyper-specialization in science. There’s no doubt that this specialization is a major reason for the strong progress of science in the last half century. However, practical problems that need to be addressed in practice-oriented research have many aspects, causes and consequences. A thorough – and sustainable – solution often requires interdisciplinary research, especially when these causes interact with each other, as is often the case (see section 6 and Verschuren 2008 for details).

The primacy of external validity or generalizability is another sacred cow of mainstream researchers. However, in many practice-oriented research projects the solution to the problem at hand primarily requires profound insight into local processes, people’s habits, belief systems and motivations, as well as context-bound knowledge, much more than general, law-like, theoretical and abstract knowledge.

And, last but not least, since the nineteenth century for most scholars in the natural and social sciences empirical or data-based research as I call it is the only valid form of research. This is a direct consequence of the primacy of epistemological validity. When it comes to implementary and ethical validity, we also need human resource-based research. However, as yet little is known about the epistemological internal and external validity of the results of human resource based strategies. When it comes to the knowledge-based variant, we should also know more about how to select the exper-
tise, and how to train experts in such a way that the return on the research is greatest. One major difficulty at this point is judgement by the experts of research questions following criteria and definitions offered by the researcher. Experts are very much prone to use their own criteria and definitions, which leads either to biased or to irrelevant conclusions. Another challenge to methodologists in this domain is how best to reinforce the interdisciplinary power of this type of research. How can we train the experts in such a way that expertise in different disciplines interacts optimally to create interdisciplinary insights, knowledge and know-how? One issue that can lead to bias here is the competition for dominance among the participating experts. And what are the effects from biases that may be based on differences in power of the participants, from rivalry, from misunderstandings caused by paradigmatic differences, and how can we avoid or reduce them?

With respect to reflective research, reflection has always played an important role in social, policy and management science research. However, given the central place of empirical research in these sciences, it is little used as a specific research strategy. Yet it deserves a strong position, especially in practice-oriented research designed to address construction problems, i.e. developing material or immaterial artefacts. But here too the task for research methodologists in the future is to develop a special methodology for addressing construction problems, i.e. supporting the design cycle.

Another limitation of the primacy of empirical or data-based research is the use of the empirical cycle. Seen from the standpoint of practice-oriented research this is second best. The intervention cycle and the design cycle are preferable when addressing practical problems. On this point too, research methodologists still need to do a great deal of work. There is a vast methodological literature on evaluation related to the intervention cycle. Besides, there are numerous books and articles about problem analysis, which is mostly discussed in terms of problem structuring. However, there are gaps in research methodology as to the other stages in the intervention cycle.

As yet there is little in the literature that relates to the design cycle. In this respect there should be more interaction between the N-type and n-type of practice-oriented research methodology. In policy research (N-type) more use should be made of the design cycle and of an extended literature on problem structuring as it was and is still being developed in organizational research (n-type). Conversely, in organizational research (n-type) more use should be made of an extended and well-articulated methodology for evaluation research, as has been developed in policy research (N-type). Moreover, in organizational research too little use is made of generic models for problem resolution, i.e. the intervention cycle and the design cycle.

Coming to the end of this essay, one last question needs to be addressed. In section 2, two (out of three) settled convictions that encourage a trail of methodology for practice-oriented research were formulated: (a) There is no methodologically relevant difference between theory and practice-oriented research. (b) Practice-oriented re-
search is not the task of science and universities. Imagine that you, after reading the essay, became convinced that there are methodologically relevant differences. What then might be your argument to stick to conviction (b)? The argument might be that in your view the primary task of science and universities is theory building, thus putting practice-oriented research under a ban. There are three counter-arguments against this position, that I will give the form of rhetorical questions. (1) If science and universities are not ready to develop this methodology, who else is going to do it? (2) How can universities say that it is not their task, as over ninety percent of their students come into contact with practice-oriented research once they leave university, rather than with theory-oriented research? (3) Why practice-oriented research would not contribute to theory building? The first two questions are straightforward, but the third needs some elaboration. This is all the more so, as here may lay the main argument against practice-oriented research in the mainstream of the social sciences. To find an answer to this question we have to realize how processes of abstraction, generalization and theory building work.

In mainstream theory-oriented research in the social sciences there are two stages in this process. The first stage is at the micro-level of individual research projects. In this stage generalization occurs by means of the use of (a) abstraction in the concepts to be used and (b) random samples in combination with the use of inductive statistics. The results of such projects can to a large extent be external valid or generalizable. In order to develop abstract and general theories, we need a second stage at the macro-level of a

| Stage I: Micro-process of generalization (not in practice-oriented research) |
| Stage II: Macro-process of generalization |

### Theory-oriented research
- wide populations
- random samples
- results
- theory

### Practice-oriented research
- small populations
- strategic samples
- knowledge
- results
- theory

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Figure 15. Theory building on the basis of theory-oriented and practice-oriented research, respectively
scientific discipline, taking the products of the first stage as an important input, besides mental capacities of the scientist such like deductive and inductive reasoning, abduction and confrontation. In Figure 15 this two-stage process is visualized.

In contrast, in the case of practice-oriented research there is no first stage of abstraction and generalization. That is, in the practice-oriented paradigm there is at the level of individual research projects no endeavour to create abstraction or generalization. On the contrary, there is an endeavour to generate concrete, context-bound knowledge. But why should not we follow here the second stage, involving a process of abstraction and inductive reasoning? This even has important advantages compared to the two-stage process in the mainstream. Firstly theories may become richer and more profound. The reason is that in the first option details are lost already during the first stage. Moreover, in the first option the researcher is forced to use labour-extensive methods and strategies, in order to cover either a wide population or the abstract concepts that are being used, or both.

A second advantage is that in the case of a practice-oriented paradigm there are better opportunities to theorize on new phenomena, as well as on all kinds of processes (see section 2). Thirdly, other things being equal there is less chance for the bias of reductionism, as is argued too in section 2 under the heading of type of knowledge.

Last but not least, a big advantage of a practice-oriented paradigm is the social relevance of science, in reaction to a still growing pressure for more utility of its products, as is elaborated in section 1.

Given the analyses and its conclusions, I would like to balance this essay with the statement that a new paradigm is needed in the social, policy and management sciences at universities, i.e. a paradigm of practice-oriented research. It’s a paradigm with a primacy of the contexts of discovery and implementation, besides the context of justification, primacy of implementary and ethical validity, and a primacy of holism and interdisciplinarity in contrast to reductionism. Each paradigm change in the history of science was accompanied by challenges to strongly-held convictions and doctrines, perceived as heresies by the scientific mainstream of the time. It won’t be any different this time.
REFERENCES

WHY A METHODOLOGY FOR PRACTICE-ORIENTED RESEARCH IS A NECESSARY HERESY


