# Management theory development on the basis of the design paradigm : the quest for tested and grounded technological rules

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# Management theory development on the basis of the design paradigm : the quest for tested and grounded technological rules

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# MANAGEMENT THEORY DEVELOPMENT ON THE BASIS OF THE DESIGN PARADIGM
## THE QUEST FOR TESTED AND GROUNDED TECHNOLOGICAL RULES

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## Abstract

Academic management theory has a serious utilization problem. This article argues that such theory has better chances for adoption for instrumental use if it is developed, not on the basis of the paradigm of the "explanatory sciences", like physics, but based on the paradigm of the "design sciences", like medicine and the engineering sciences. The mission of an explanatory science is to describe, explain and predict, whereas the core mission of a design science is to develop "tested and grounded technological rules". This paradigm is applied to management research and the contribution of this type of research to the solution of the utilization problem is discussed.

## 1. Introduction

In the Information for Contributors it is said that this Journal publishes papers "that advance the science[1] *and practice* of management" (my italics). The Academy of Management Code of Ethical Conduct states "Our professional goals are to enhance the learning of students, colleagues, and others and to *improve the effectiveness of organisations* through our teaching, research and practice of management" (Academy of Management, 1995: 573; my italics). So the impact of management theory on the practice of management and on the performance of organisations is seen as an important issue. Yet there are serious doubts with respect to the actual relevance of present day management theory as developed by the academic community. As far back as 1982, Beyer and Trice remark "Recently (....) scholars have expressed concern about why organisational research is not more widely used" (Beyer and Trice, 1982: 591). Somewhat recently, Hambrick (1994) sketches in his Presidential Address to the Academy of Management a dismal picture of the impact of the Academy, concluding that it might have mattered for the world of organisations and business, but that it didn't.
The solution Hambrick proposes is essentially a better presentation of academic management research results to the outside world, to "open up the incestuous, closed loop of the Academy's conferences" (Hambrick, 1994: 13). This may help. However, it is the thesis of this article that the relevance problem of academic management theory is not primarily caused by a poor presentation of such theory but by its very *nature*. Kurt Lewin's well-known adage says "nothing is quite so practical as a good theory". Assuming that "good" does not mean

"practical", but something like "scientifically sound", this article intends to qualify that adage: all good theories are practical, but some are more practical than others.

A practical theory is one that can be and is actually used by the practitioners in the relevant field. In the social sciences the utilisation problem is a well-known one (see e.g. the Special Issue of Administrative Science Quarterly on the utilization of social science of 1982). In management theory it is sometimes seen as a dilemma: the rigour-relevance dilemma. Management theory is either scientifically proven, but then too reductionistic and hence too broad or again too trivial to be of much practical relevance, or relevant to practice but then usually lacking sufficient rigorous proof. March and Sutton (1997) remark that in other disciplines this dilemma is sometimes solved by separating the two contexts. However, in and around business schools "the soldiers of organisational performance and the priests of research purity often occupy not only the same halls but also the same bodies" (March and Sutton, 1997: 703).

In the natural sciences the utilisation problem is of a quite different order. There may be some tension between basic and applied research, including a possible difference in social status and a competition for research resources, but in general there operate two very effective partnerships, i.e. the one between the natural sciences and applied fields like medicine, engineering and modern psychotherapy and another between researchers in a given field and the professionals of that field (often occupying the same bodies). The social sciences have borrowed various models of science from the natural sciences (not without their difficulties, however). The thesis of this article is that the utilization problem of academic management theory can be tackled by using the model of the partnership of the natural sciences and their applied fields. This would lead to research on the basis of the paradigm of what I have named the "design sciences", with as core objective the development of "tested and grounded technological rules". I will discuss the consequences of this for management research and the way in which this may solve the utilization problem.

## 2. The utilization problem of academic management theory

The utilisation problem in the field of management is not only a well-discussed one, but also a thorny one. Researchers in this field operate in two reputation systems (see e.g. March and Sutton, 1997: 703): the academic reputation system, rewarding rigorous research, and the professional reputation system, rewarding relevant research outcomes and the professional training of aspiring managers.

The priorities given to each system vary over time, sometimes like a pendulum. Before the Ford- and Carnegie-foundation reports on American Business Schools (Gordon and Howell, 1959; Pierson et.al, 1959) the priority was on professional training, on the professional reputation system. In those times the scientific community regarded the field more or less as a practice-based craft. The reports mentioned, started a process of "scientization", resulting in a "New Look" for the American Business Schools (Schlossmann, Sedlak and Wechsler, 1987), in which there was a strong emphasis on recognition in the academic reputation system, later resulting again in reactions like the Harvard Business Review papers "The myth of the well-educated manager" (Sterling Livingstone, 1971) or "Managing our way to economic decline" (Hayes and Abernathy, 1988).

The tensions between the two reputation systems are not a typical American phenomenon, however. For instance, in the Netherlands the field of business economics has known fierce debates between the so-called Amsterdam School, primarily interested in the academic reputation system and the Rotterdam School, more interested in the professional reputation system (Van Baalen, 1995)[2].

The tensions between the two reputation systems may have stimulated the idea of the rigour-relevance *dilemma*. However, this article is not based on the idea that this is a dilemma (in which case there exists no real solution), but rather on the idea of Pettigrew's *double hurdle:* management research and theory has to achieve *both* scholarly quality and managerial relevance (Pettigrew, 1996).

The utilization problem discussed here concerns management theory as developed by the academic community. There is an abundance of management literature, which is widely read by managers but which doesn't meet scientific standards (as e.g. discussed by Kay, 1997). There are *craftsman-like* publications, based predominantly on some one's own experience (or on the experience of people he/she knows), for which there is a generalisation problem: what do we learn from this experience for other situations? And there are *methapysical* publications by management guru's (nomen est omen), for which there is a justification problem: on which observations and which logical reasonings are the recommendations based? These publications may perhaps succeed in taking Pettigrew's second hurdle, but fail at his first one. Improving the usefulness of academic management theory should make it a powerful competitor for these two types of literature.

Beyer and Trice (1982) give an in-depth analysis of the process of utilising organisational research results. Among other things they make a distinction between *adoption*, i.e. the decision by decision-makers within the user system to use certain research results, and *implementation*, i.e. the actual use by members of the user system of those research results. Another distinction is the one between *instrumental* and *conceptual* use of scientific knowledge (Pelz, 1978, cited in Beyer and Trice, 1982). Instrumental use involves acting on research results in specific, direct ways, while in conceptual use those results are rather used for general enlightment on the subject in question[3].
In this article the primarily interest is in the *adoption* of management research results and management theory for *instrumental use*. The problems of subsequent implementation are not very different from other problems of organisational change and implementation (and well-researched). I agree with Beyer and Trice, when they state "The predominant use of organisational research probably occurs through graduate seepage's into organisations of new ideas, metaphors, and rationales for explaining human behaviour" (Beyer and Trice, 1982: 615). The conceptual use of management research results is indeed an important outcome. However, I fear that academic management research will keep its utilization problem if this remains its only ambition. If the field takes it mission, as discussed in the introduction, seriously, it should also aim for the more ambitious objective of adoption for instrumental use.

## 3. Theory development on the basis of the paradigm of the design sciences

The thesis of this article is that a major inhibition for the adoption of academic management theory for instrumental use lies in the very nature of that theory. That nature is strongly influenced by the paradigm used for developing the theory. Kuhn's (1962) term "paradigm" is used by him in many different meanings; in this article it is used in his sociological meaning (Masterman, 1970): a system of "scientific habits", used by a group of scientists for the solution of scientific problems. More specifically I mean by paradigm the combination of the types of research questions asked, the allowed research methodologies to answer those questions and the nature of the intended outcomes of that research.

Starting with Burrell and Morgan's (1979) scheme of four different paradigms for studying organisations, much has been said on the possible incommensurability of different paradigms, see e.g. Jackson and Carter who follow Burrell and Morgan's in that "a synthesis between paradigms cannot be achieved" (Jackson and Carter, 1991: 109). Others claim that there is an alternative to paradigm incommensurability (e.g. Hassard, 1988) or that a multiparadigmatic approach to theory building is useful and possible by "bridging across blurred paradigm boundaries" (Gioia and Pitre, 1990: 592).
As said, in this article the term "paradigm" is used to indicate a system of scientific habits. Communication between different paradigms is quite possible, provided that the underlying ontological and epistemological assumptions do not differ (too much). This is predominantly the case for the various paradigms to be discussed below: they are regarded here as being commensurable.

### *Formal, explanatory and design sciences*

On the basis of the paradigms used we can distinguish three classes of scientific disciplines

- the *formal* sciences, like philosophy and mathematics
- the *explanatory* sciences, like the natural sciences and major sectors of the social sciences
- the *design* sciences, like the engineering sciences, medical science and modern psychotherapy.

The formal sciences are "empirically void". Their mission is to build systems of propositions of which the main test is their internal logical consistency.
The mission of an explanatory science is to describe, explain and possibly predict observable phenomena within its field. Research within an explanatory science should lead to "true" propositions, i.e. propositions which are accepted by the scientific forum of that discipline as being true on the basis of the proof given. The typical research product of an explanatory science is the causal model, preferably stated in quantitative terms.
The mission of a design science is to develop knowledge to be used in the design and realization of artefacts, i.e. in solving *construction problems*, or in the improvement of the performance of existing entities, i.e. in solving *improvement problems* (architects and civil engineers deal predominantly with construction problems, medical doctors and psychotherapists with improvement problems). Research within a design science aims at developing knowledge, the application of which should lead to intended results.

Much research within the design sciences is based on the explanatory paradigm, i.e. is research aimed at description, explanation and prediction, in order to understand the setting of construction or improvement problems and to know the properties of the "materials" to be used. However, understanding is not enough, the ultimate mission is to develop design-knowledge. It is important to teach a civil engineer subjects like physics and mechanics, but in designing a bridge he/she needs the design-knowledge developed by his/her discipline, like the properties of various types of bridges. In English the term "science" is often equated with "natural science", leading to the idea that the mission of *all* sciences is just to describe, explain and predict and that such descriptive knowledge is sufficient for practitioners to solve their problems. Science, then , occupies Schön's (1983) "high ground of theory", while practitioners operate "in the swamp of practice". In this article the interest is in the development of design-knowledge, occupying the middle ground between descriptive theory and actual application.

### *The Utilisation of Design-Knowledge*

A design science doesn't develop knowledge for the layman, but for the professionals in its field. Among other things this means that such design-knowledge is to be applied by individuals, which have had a formal education in that field.
A *professional*, such as a medical doctor, an architect, a psychotherapist, a mechanical engineer, a lawyer or an accountant, can be defined as a member of a well-defined group, who solves real-world problems with the help of skills, creativity and with scientific design-knowledge (Freidson, 1973; Schön, 1983).
Every time a professional has to solve for a client, or in conjunction with a client, a unique and specific problem. He/she does so by using the *problem solving cycle*, which roughly consists of: defining the problem out of its "messy" context, planning the intervention (diagnosis, design of alternative solutions, selection), applying the intervention (in conjunction with the client) and evaluation.
The essence of professional work is *design*, the planning of action in advance or during the action ("reflection-in-action", Schön, 1983). In general he/she will make three designs (Van Aken, 1994): an *object-design*, the design of the intervention or of the artefact, an *realisation-design*, the plan for the implementation of the intervention or for the actual building of the artefact, and a *process-design*, the plan for the problem solving cycle of the professional him- or herself, or, put differently, the method he/she will use to solve the problem[4].

In this article the term "design science" is used to indicate that the mission of such a science is to develop scientific knowledge to support the design of interventions or artefacts by its professionals and to emphasise the knowledge-orientation of a science: a design-science is not concerned with action itself, but with *knowledge* to be subsequently used in *design-based action*. The idea of "design science" is strongly inspired on Simon (1969).

### *The Repertoire of Design Knowledge*

In order to be able to make these designs, professionals have a *repertoire* of design-knowledge (Schön, 1983) at their disposal which includes their own experience and that of their immediate teachers, as well as the scientific design knowledge of their design science, which they have acquired during their training and continuing-education. This design-knowledge is general, i.e. valid for *classes* of cases. The problem of the professional, however, is unique and specific. He/she must therefore *translate* general knowledge to his/her unique and specific case. In this way, for example, lawyers use jurisprudence from similar cases for dealing with their specific case and doctors use general descriptions of symptoms, diseases and therapies applied, for the design of a therapy for a specific patient.

Design-repertoires contain three types of design knowledge, according to the three types of design discussed above. For most professionals their repertoire contains predominantly *object-knowledge*, i.e. knowledge on the settings and properties of the artefacts or interventions to be designed, e.g. for a civil engineer the properties of various types of bridges and for a medical doctor the effects of various therapies for a given disease. It may also contain *realization-knowledge*, e.g. for a mechanical engineer knowledge on manufacturing technologies. However, a design repertoire often contains only fairly limited explicit *process-knowledge*, i.e. knowledge on how to tackle the actual design process itself. Most professionals obtain their process knowledge craftsman-like, i.e. by their own experience and by imitation of their teachers and peers. Process-knowledge tends to remain largely tacit; professionals find it often difficult to express their approach to design problems verbally.

Essential ingredients of a design repertoire include design-language, generic models and various types of prescriptions.

A *design-language* is a system of concepts and conventions with which design settings and the designs themselves can be described. A *design* can be defined as a model of a system or process to be realised. A design-language, then, gives the concepts and conventions to be used in making that model[5].

*Generic models* describe the settings of the designs to be made and their general properties, including the *design parameters* - i.e. the choices one has to make in designing - and as far as possible the *design alternatives* for each design parameter (see box 1 for an illustrative example).

*Prescriptions*, finally, have the general format of "if you want to achieve y in situation z, then perform action x". There are *algorithmic* prescriptions, which operate as a recipe, which have often a quantitative format and whose effects can conclusively be proven on the basis of observations through deterministic or statistical generalization. However, most prescriptions in a design science are of a *heuristical* nature. They have the general format of "if you want to achieve y in situation z, then x can help". They have typically a qualitative format and they operate not as a recipe, but must be translated to the situation of application. Their indeterminate nature makes it impossible to prove their effects conclusively, but they can be tested in their context which can lead to sufficient supporting evidence.

There are many types of heuristic perscriptions, including tools and techniques. An important subset of heuristic prescriptions are *design models*. A design model is an in its context tested solution for a design problem, be it an artefact (or part of an artefact) or an intervention. Examples are certain constructions like parts of a building or of a bridge or a certain therapy in medicine. In designing a specific intervention or artefact a professional can choose out of several feasible design models, or adapt a certain design model to his situation, make a combination of two or more design models, etc.

### *Tested and Grounded Technological Rules*

In the explanatory sciences the research object is an "explanandum" (Van Strien, 1986) and the typical research product the causal model: one or more dependent variables are explained in terms of one or more independent variables and knowledge of the values of those variables can be used to predict the behaviour of the dependent variable(s).

In the design sciences the research object is a "mutandum" (van Strien, 1986); these sciences are not too much interested in what *is*, but more in what *can be*. The typical research product is the above-discussed prescription, or, in terms of Bunge's (1967) philosophy of technology, the *technological rule*.

Humanity has a long tradition in developing technological rules. Primitive societies not only develop technological rules to make artefacts, but also to make rain, increase fertility and to avoid natural disasters, etc.. A major break-through came by the testing of technological rules. A *tested technological rule* is one of which effectiveness has been systematically tested within the context of its intended use. The system of interest is treated as a black box, but under certain conditions specific interventions give the desired results (deterministically or stochastically). Traditional Chinese medicine is an example of a system of very powerful tested technological rules.

However, the real break-through came when tested technological rules could be *grounded* on scientific knowledge (Bunge, 1963), including law-like relationships, from the natural sciences.

For instance, one can design an aeroplane wing on the basis of tested technological (black box) rules, but one can design such wings much more effectively (and efficiently) on the basis of tested *and* grounded technological rules, grounded on the laws and insights of e.g. aerodynamics and mechanics. The stunning progress of the design sciences since the (first) Industrial Revolution is based on the effective partnership between (explanatory) natural sciences and the design sciences, leading to systems of tested and grounded technological rules.

### *Clinical Research and the Reflective Cycle*

If the tested and grounded technological rule is the typical research product of a design science, the typical research design is *clinical research*, i.e. research on the effect of interventions or the performance of artefacts done *within their context.*
The causal model of the explanatory sciences typically is developed as much as possible within a closed system (like a laboratory) in order to exclude as far as possible (or control) the influences on the dependent variables from other sources than the independent variables of interest. The technological rule, on the other hand, typically is studied within its intended context in order to be sure - as far as possible - of its effectiveness also under the influence of lesser known sources. The grounding of a technological rule on explanatory laws does not necessarily mean that every aspect of it (and of its relations with the context) is understood. Typically several aspects keep their "black box character" and testing within the context is still very much necessary to account for its effectiveness[6].
The typical research design to study and test technological rules is the *multiple case*: a series of cases is solved, each by applying the problem solving cycle and in each analysing context, interventions and results. Design-knowledge is built up through the *reflective cycle* (Van Aken, 1994): choosing a case, planning and implementing interventions (on the basis of the problem solving cycle), reflecting on the results, developing design-know-how to be tested and further developed in subsequent case(s).

## 4. Management theory on the basis of the design paradigm

A useful definition of a theory is given by Bacharach (1983: 496): "a theory is a statement of relations among concepts within a set of boundary assumptions and constraints". His subsequent discussion suggests that he confines "theory" to the domain of the explanatory sciences: a theory organises observations, so the observer is prevented from "being dazzled by the full-blown complexity of natural or concrete events" (Bacharach, 1989: 496). However, this definition can be applied both to the causal models of the empirical sciences as to the technological rules of the design sciences.

Following the previous discussion on the explanatory and the design sciences, one could make a distinction between *organisation theory* and *management theory*, the former being based on the paradigm of the explanatory sciences and hence having the mission to describe, explain and predict, the latter being based on the paradigm of the design sciences and hence having the mission to develop tested and grounded technological rules in the field of management. The model of the partnership between physics and engineering science and between engineering science and engineers is used for partnerships between organisation theory and management theory and between management theory and practitioners. Management theory uses organisation theory to ground its technological rules, organisation theory uses management theory to generate new research questions and to enrich its understanding of its research object[7] (following Starbuck and Nystrom's (1981) adage "if you want to understand a system, try to change it"; observation without intervention may learn us less than the combination).

Management theorists, then, develop tested and grounded technological rules like design models for planned change of organisations, design models for decreasing the time-to-market for new products, design models for organising patient-oriented general hospitals, design models for building and managing strategic alliances among business firms, design models to share tacit knowledge between high tech firms, etc. etc.
Management theorists may be regarded as March and Sutton's (1997) "soldiers of performance" and organisation theorists as the "priests of research purity". However, usually management theorists will *not* be concerned with the development of (reductionistic) causal models explaining organisational performance in terms of some independent variables. The reason for this is firstly that such reductionistic models tend to be much less informative then heuristic tested and grounded technological rules. Secondly, "performance" is (like "profit" or "new-product-success") a *bottom-line variable*, influenced not only by some variables of interests, but by almost every other organisational and contextual variable as well. Therefore, the result variables against which technological rules are to be tested are usually *intermediate variables*, like the ones implied in the list given above: realisation of a given change, realisation of a reduction in throughput times etc. These intermediate variables are ultimately thought to be linked with performance, but this link typically is not tested.

To summarise this, table 1 gives in black-and-white terms the main differences between Organization Theory and Management Theory along the lines, suggested above.

| | Organization Theory | Management Theory |
|---|---|---|
| dominant paradigm | explanatory sciences | design sciences |
| typical research question | explanation | alternative solutions for a class of problems |
| typical research product | causal model;<br>quantitative law | tested and grounded technological rule |
| nature of research product | algorithm | heuristic |
| justification | proof | saturated evidence |

Table 1 Main differences between Organization Theory (OT) and Management Theory (MT), if one chooses to use the paradigm of the explanatory sciences for OT and of the design sciences for MT.

In this context it is also interesting to look at the more specific management disciplines, like operations management and marketing management. There one has quantitative models, like mathematical production scheduling models or models describing consumer behaviour, with sufficient high internal validity to give them academic respectability. And one has research on real management problems, like the implementation of production control systems or the design of account management systems, usually based implicity on the design paradigm and often having somewhat less academic respectability.

The application of the explanatory sciences - design sciences - professionals model to the field of management is, of course, not without its problems.

One reason is connected with people. For professionals like engineers, lawyers and medical doctors the design-knowledge of their discipline is essential for their professional success. It is on the basis of their mastery of this knowledge that they distinguish themselves from charlatans. Managers, however, may profit from (good) management theory, but they can largely survive without much of it, provided they have the social skills needed and sufficient domain knowledge (knowledge of their industry). Management consultants may be better partners for management theorist, as the knowledge-content of their trade usually is greater than that of most managers.

Another reason is connected with the immaterial nature of the systems to be designed or improved. Social system design and realisation is quite different from physical system design and realisation. Organisations are essentially social constructions (Berger and Luckman, 1966; Gergen, 1985). With respect to a physical system one can distinguish the physical object from its immaterial model; for a social system the model, i.e. the conceptions of that system held by its stakeholders, *is* ontologically the system. Among other things this generates the problems of interconnectedness and of causation.

The *problem of interconnectedness* is generated because a social system is even more difficult to isolate from its context than a physical system, due among other things to difficult to analyse information links and social influences. The problems to be solved are, furthermore, interconnected with their messy context and there is the already alluded to interconnection between stakeholders' models of reality and the change agents' models of reality.

The *problem of causation* is generated by the instability of the causal "mechanisms" within immaterial systems (see also Tsoukas, 1989, on this issue). Like complex physical systems, immaterial systems have complex feed back loops and non-unilateral causal chains, but they show, furthermore, various kinds of learning effects leading to self-defeating or again self-fulfilling statements on causation (March and Sutton, 1997: 699).

The problem of interconnectedness and causation make it difficult to develop causal models. However, these difficulties are much less present in the study of technological rules in their context, provided one accepts the partial "black box character" of their grounding on empirical insights.

## 5. Algorithmic and heuristic technological rules

Management research on the basis of the design paradigm aims at several outcomes, including descriptive and explanatory theory, but its core objective is the development of tested and grounded technological rules. As discussed before, there are two types of rules: algorithmic and heuristic rules. By an algorithmic rule certain outcomes are linked - deterministically or stochastically - with certain interventions. Such a rule has often a quantitative format and follows the same format and logic as the causal law of the explanatory sciences. In engineering algorithms play an important role. In the field of management, however, they are - and will probably remain so - rare.

In this article the interest is more in particular in (tested and grounded) heuristic rules. As said, they have a qualitative format, need translation - not plain application - to the specific situation of use and their effectiveness (i.e. the effect of its application on some result variable) cannot be conclusively proven. However, rigorous testing can lead to sufficient supporting evidence. In the case of algorithmic rules the evidence can be left out after it has been assessed: application and further research can be based the rules themselves. In the case of heuristic rules the evidence remains part of the results. In order to use these results for further research or for professional application, one keeps needing the evidence - either as it is or in condensed form - to assess the effects of the rule *and* to be able to translate the (general) rule to the specific application.

One can make a distinction between *strong* and *weak* heuristical rules. If one has a strong rule, one gets the intended results fairly often, whereas in case of a weak rule these results are less often obtained. "Fairly often" and "less often" may seem unsatisfactory qualifiers for the power of a rule. In practice, the nature of the supporting evidence will make it usually sufficiently clear for the practitioner what he/she can expect from the application.
As said, the effectiveness of a rule cannot be conclusively proven. However, compiling supporting evidence can lead to *theoretical saturation* (Eisenhardt, 1989), when subsequent findings (sufficiently) confirm earlier ones. One can call a heuristic rule for which theoretical saturation is reached a *saturated* heuristic and one for which there is still insufficient supporting evidence an *unsaturated* one[8].

The quest for technological rules, often called prescriptions, is as old as the field of management theory itself, even older than the tradition of description and explanation in this field. The objective of this article is, therefore, not the invention of the idea of prescription but to propose to take prescriptions scientifically serious by rigorous testing and grounding. Three examples will be given to show the consequences of taking technological rules scientifically serious.

The first example is Mintzberg's influential book "The structuring of organisations" (Mintzberg, 1979). He gives in part III 16 *hypotheses*, linking design parameters to contingency factors like age, size and technical system. He suggests that the more effective organisations are structured according to these hypotheses. For each hypothesis much supporting evidence is offered, based on research and/or logic. These hypotheses come very close to heuristic propositions, tested and grounded. However, the use of the design paradigm would lead to differences, in particular to important *rhetorical* differences.

- In stead of "hypothesis" one would use "proposition" or "technological rule". The scientific status of a hypothesis is at best informed conjecture, not a proposition with scientific status. Mintzberg possibly didn't use a stronger term than "hypothesis". because of the idea that only proven algorithm's belong to the domain of science. In order to arrive at a research design, which could lead to propositions in the form of proven algorithm's, one would have to trim down these hypotheses to testable ones and such hypotheses are almost always either to trivial or too broad to have much relevance for practitioners. The idea of the present article is to admit heuristic technological rules to the domain of science by rigorous testing and grounding.
- Some hypotheses would be omitted, others expanded. For instance, hypothesis 1 states "the older the organisation, the more formalised its behaviour" (Mintzberg, 1979: 227). There is sufficient empirical evidence to call this a (heuristic) proposition. However, the independent variable is not subject to manipulation and therefore of less interest for design.
- Propositions would be formulated in an action-oriented format and not in a descriptive one. Hypothesis 3, for instance, reads "the larger the organisation, the more elaborate its structure, the more specialised its tasks, the more differentiated its units and the more developed its administrative component"(Mintzberg, 1979: 230). Again there is sufficient empirical evidence to call this a proposition rather than a hypothesis. It is an important one too and its managerial implications are clear: for instance, if your organisation experiences severe administrative problems after a period of strong growth, it can be beneficial to move in the direction of this proposition. To reformulate this

proposition into technological rules, however, is not trivial. To do so requires much design-oriented research.

- Finally, Mintzberg uses the bottom-line variable "effectiveness" as the result variable, whereas technological rules are - as discussed - preferably tested on intermediate result variables.

The second example is Beyer and Trice's insightful book "The cultures of Work Organisations" (Beyer & Trice, 1993). In the last two chapters they give a number "aphorisms" (Beyer & Trice, 1993: 379 and 399), to be used by practitioners in maintaining or changing the cultures of their organisation. Examples are "capitalise on propitious moments", "change many elements, but maintain some continuity", "select, modify and create appropriate cultural forms", modify socialisation tactics".
The use of the design paradigm would in the first place again lead to a change in rethorics. The scientific status of an aphorism is even lower than that of a hypothesis. On the basis of the design paradigm, these aphorisms could have been presented as propositions or as technological rules. That would have led to a second rhetorical change. The discussion of the aphorisms by Beyer and Trice predominantly serves to explain them. If they would have been presented as propositions the authors might have given more attention to justification and might have organized part of the evidence, given in the preceding chapters, around those propositions.
Another point is testing. The aphorisms are basically derived from observation, they are not based on actual application. It would have been interesting to test their use in actual application through action research (see the next section).

The third and final example concerns Transaction Costs Economics (Williamson, 1985), by many regarded as scientifically "good" theory. Maybe this is an example of "good" theory that is not so practical, as it can be quite dangerous to put this theory into practice (Goshal & Moran, 1996).

## 6. Implications for management research

The choice of a research design, of course, depends on the type of research question or objective. If the research objective is to develop and test heuristic technological rules, clinical research, i.e. research on an research object within its context, is usually the appropriate design, among other things because of the problems of interconnectedness and of causation.

The field of organisation and management has a long tradition in clinical, or case-study research, the famous Western Electric studies being an early example. The quest for causal laws, undertaken in the course of the "scientization process" of the field mentioned before, may have shifted research resources to other types of research, but nowadays there seems to be little doubt that the case-study, and in particular the multiple case-study, can produce and has produced valid research results (Miles and Huberman, 1984; Yin, 1984; Eisenhardt, 1989 and 1991; see also Kirk & Miller, 1986, on the often cited problems of reliability and external validity of case-research).

Eisenhardt (1989) presents a useful process of theory-building on the basis of a multiple case-study. She uses it for description and explanation, but her scheme can equally well be used for developing technological rules. As in theory building, technological rules typically are not given beforehand, but extracted from the research data as the researcher learns more over his/her research object. She subsequently shows that evidence for the developing theory is collected through cross-case analysis on the basis of replication (Eisenhardt, 1991). An important issue in the development of heuristic technological rules is the question of generalisation (or external validity). In the case of rules one can speak of the *application domain* of a rule: to what extent are the results one found in the cases studied also to be expected in other cases. For this one uses analytical generalisation (Yin, 1989; Kennedy, 1979): on the basis of logical reasoning the effects of the rule are generalised to cases which have as many as possible relevant characteristics in common with the examined cases. The claim one can make with respect to the application domain, of course, depends on the set of cases studied: the set has common relevant characteristics which define the application domain, but samples on the other hand as much as possible the variety within that domain ("theoretical sampling", see Eisenhardt, 1989: 537; Kennedy, 1979).

There are two general types of multiple case-studies to develop heuristic technological rules: the *extracting* multiple case and the *developing* one. In the extracting multiple case one studies best and worst practices which respect to the phenomenon of interest (worst or bad practices being often at least as informative as best or good practices). In a series of case studies one "extracts" technological rules from the research data and uses replication logic to validate them. The extracting multiple case uses methods of evaluation research, but with the difference that the program or rules to be evaluated are typically not known beforehand. Box 1 gives an example of such a multiple case-study.

Box 1
An example of an extracting multiple case-study

The study of Thoelke (1998) on product feature management is an example of an extracting multiple case study. Eleven mini-casestudies were executed to explore the subject, followed by eight large scale case-studies to develop the theory. The theory consists of

- a *conception* of the phenomenon of product features as such, including its relations with intrinsic product properties and its rhetorical aspects (both in product feature decision-making and in sales talks) on the basis of discourse analysis
- a *generic model* of product feature management, giving a general description of the process and identifying the major *design parameters*, i.e. the major choices one has to make in organising a product feature management process
- for each of a number of design parameters a set of *design models*, i.e. examples of ways to handle the choice in question, together with their strong and weak points, depending on situation and on management objectives; these design models are the technological rules of this research, tested on the basis of cross-case analysis and member-feed back and grounded on logic and a variety of theories.

In the developing multiple case a rule or a set of rules is developed in successive cases through a kind of action research (see e.g. Elder & Chisholm, 1993 on action research). The researcher applies the rules him/herself or co-operates with the client system in applying the rule. Afterwards he/she evaluates what is learned, adapts the rules if necessary and applies them in a next case until saturation is reached. Box 2 gives an example of a developing multiple case-study.

Box 2
An example of a developing multiple case-study

The study of Verweij (1997; Verweij & Zwegers, 1998) to develop a design language and a participative method to use it for the redesign of the production organisation of SMEs, is an example of a developing multiple case-study.

The design language (PDL, production description language) was developed during a preparatory project by a consortium of two consulting firms, a university institute and three industrial firms on the basis of literature and the codified and tacit knowledge of the six partners. Verweij played also a major role in this preparatory project. PDL included six so-called Production Basic Types, essentially design models for the organisation of the shop floor (like the functional department, the flexible manufacturing system, the multi productline and the flow dock).

Subsequently in a series of fourteen mini-cases the practical usefulness of the PDL for describing and analysing SME-production structures was assessed, where upon the participative method of using PDL for actual redesign of production structures was tested in a series of three developing case-studies (other than the three previous partners). During these case-studies the members of the SMEs in question made, with the support of the researchers and PDL, redesigns of their production structures, combining their tacit knowledge with the codified knowledge contained in the PDL (which also included the weak and strong points of each design model, depending on situational factors).

The PDL-method can now be regarded as a (set of) tested technological rules. Grounding, i.e. understanding its effectiveness, was done on the basis of various theories, including knowledge management theory and design theory.

The use of a certain paradigm has implications for the research questions asked, the allowed methodologies and the intended outcomes. The implications of using the design paradigm for the development of management theory and for management research include the following

- A prime objective of such management research is the development of management theory in the form of tested and grounded technological rules, in particular heuristic rules
- A typical research design is the multiple case-study, in particular the extracting and the developing one
- The evidence given is largely organised around the technological rules (they are taken scientifically seriously)
- The research effort is not only focussed on description and explanation - of course also essential for research on the basis of the design paradigm - but also on the middle ground of design-knowledge, between "the high ground of (descriptive) theory and the swamp of actual practice".

Maybe the most important consequence of using the design paradigm for management theory is one of *legitimation*: it is scientifically legitimate to put forward design-oriented ropositions or rules, for which there is no conclusive *proof*, but for which there is sufficient *supporting evidence* on the basis of testing and grounding.

## 7. Concluding discussion

The main thesis of this article is that the utilization problem of academic management theory is largely caused by its very nature. Academic management theory predominantly is developed on the basis of the paradigm of the explanatory sciences, as is well epitomized by Seth and Zinkhan's statement 'explanation by law is the essence of science' in a discussion of the methodological principles for research in the field of strategic management (Seth and Zinkhan, 1991, p 75). Such theory tends, therefore, to be too descriptive: understanding the causes of organizational on managerial problems is often a necessary, but seldom a sufficient condition for solving them. To overcome the utilization problem it would be beneficial if a research stream would be developed, based on the paradigm of the design sciences and aimed at the development of sound alternative solutions to managerial problems,. i.e. at the development of tested and grounded technological rules.

As discussed, in the field of engineering one has the partnership between the natural sciences and the engineering sciences, the former developing causal models to be used for the grounding of the technological rules, developed by the latter. And we have the partnership between the engineering sciences and the professional engineers, the latter giving feedback on the technological rules developed by the former. In medicine we have similar partnerships. In the field of organization and management one may have such partnership between the social sciences and academic management research and between the latter and management practitioners, i.e. managers and management consultants.

This article has drawn heavily on analogies between engineering and medicine on the one hand and management on the other. Of course not to prove anything, but to illustrate the point and , furthermore, to show the academic legitimacy of praxis-oriented research in other fields. Ultimately the utilization problem may be a problem of attitudes: it may be considered as un-academic to be much concerned with praxis, like a Roman senator was not allowed to be involved in craft or trade. A quest for tested and grounded technological rules, which in the field of management will be predominantly qualitative and heuristic in nature, means trading the priestly beauty of truth for the soldiery glory of performance (to paraphrase March and Sutton, 1997) and that may be too high a price. In medieval times the medical doctor did not soil his hands, but left the butchery to the barber-surgeon. In modern times this barber-surgeon has been emancipated to an academically respectable surgeon. Maybe that will happen too to the academic management researcher, developing tested and grounded technological rules.

## Notes

1. In this article we use the term "scientific" like the German "wissenschaftlich" or the Dutch "wetenschappelijk", meaning "according to sound academic standards"; so its meaning is not confined to the natural sciences.
2. Some, like Child (1995) and Koza & Thoenig (1995), suggest that in our field one might contrast a European tradition more concerned with the academic reputation system with an American one, more concerned with the professional reputation system. This may be true for European scholars with a background predominantly in sociology, but not necessarily so for organisation and management scholars with a different background (as also the present article may illustrate).

3. Pelz (1978) discussed also a third type of use, viz. *symbolic* use of scientific knowledge: the use of that knowledge to legitimate predetermined positions. This type of utilisation is, however, less relevant for the present discussion.
4. Experienced professionals often have internalised so much process-knowledge, that they don't make *explicit* process-designs anymore.
5. Design-language can have a linguistic format, but may also include conventions for graphical designs, schemes, flow charts and so on.
6. This is not to say that *all* technological rules have to be tested within their context. In particular in the engineering sciences it can be possible to isolate certain research subjects from their context without losing essential characteristics.
7. The separation of organisation theory and management theory need not involve a separation as the researchers concerned. However, such a separation might have advantages, as organisation theory attracts researchers from very different backgrounds (see e.g. Hatch, 1997: 5), while management theorists may have more similar backgrounds. For management theory this might lead to the higher level of paradigm development that Pfeffer (1993) advocates, while the more multi-perspective approach championed by Van Maanen (1993) can still be fruitful within the domain of organisation theory.
8. Strong heuristics will in principle get sooner saturated than weak ones. As one has typically not too many observations (Eisenhardt, 1989, suggests four to ten cases), weak heuristics have a low chance of being extracted from the data.
9. An example of this attitude in our field is the following citation "The managerial approaches such as Human Relations perspectives or Organisational Development studies have been heavily criticised in Europe (as elsewhere) as social engineering, not pure science" (Koza & Thoening, 1995: 3-4). "So what", one is tempted to ask.

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