The Field-tested and Grounded Technological Rule as Product of Mode 2 Management Research

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The relevance problem of academic management research in organization and management is an old and thorny one. Recent discussions on this issue have resulted in proposals to use more Mode 2 knowledge production in our field. These discussions focused mainly on the process of research itself and less on the products produced by this process. Here the focus is on the so-called field-tested and grounded technological rule as a possible product of Mode 2 research with the potential to improve the relevance of academic research in management. Technological rules can be seen as solution-oriented knowledge. Such knowledge may be called Management Theory, while more description-oriented knowledge may be called Organization Theory. In this article the nature of technological rules in management is discussed, as well as their development, their use in actual management practice and the potential for cross-fertilization between Management Theory and Organization Theory.

Introduction

A respectable objective for academic research is the development of knowledge for knowledge's sake (Huff, 2000). The key quality criterion for such knowledge is validity, i.e. it is deemed valid by an informed audience – the relevant scientific community – on the basis of the arguments and empirical proof presented (Peirce, 1960). However, for research at professional schools like business schools, one may want to add a second criterion, relevance. A significant part of the knowledge produced by research at business schools should not only take the hurdle of academic rigour but also the one of relevance (to use the double hurdles metaphor of Pettigrew, 2001). It should be relevant for the world of management and business, as the majority of their students may expect that they can use their Business School knowledge in their careers outside academia.

Yet, this relevance of academic research products in the field of organization and management is seen by many as problematic. And, as discussed in more detail below, it is a fairly old problem. Recently a new debate on this issue was sparked off. Among others the *British Academy of Management* and this Journal paid much attention to the relevance gap. It started with Tranfield and Starkey (1998), followed by the Starkey-Madan Report (an abridged version of this report is given in Starkey and Madan, 2001) and subsequently by a Special Issue of this journal (Hodgkinson, 2001). In the UK the debate was strongly inspired by the seminal work of Gibbons et al. (1994), later expanded by Nowotny, Scott and Gibbons (2001), on the distinction between Mode 1 and Mode 2 knowledge production. Mode 1 knowledge production is purely academic and mono-disciplinary, while Mode 2 is multidisciplinary and aims at solving complex and relevant field problems. Mode 2 knowledge production is presented as the example to follow in academic management research to bridge the relevance gap (Tranfield and Starkey,1998, and Starkey and Madan, 2001).

However, these discussions on Mode 2 knowledge production tend to focus on the research process and less on the knowledge produced by this process. One may expect that the intense interactions between researchers and practitioners in Mode 2 research will enhance the relevance of the resulting research products. Nevertheless, it also seems to be worthwhile to take a hard look at the products of such research processes. Do they really produce "knowledge for action" (Argyris, 1993), and especially, do they produce knowledge for action to be used in other contexts than the ones in which it was produced? The present article intends to contribute to the Mode 2 discussion by articulating a possible research product of Mode 2 research, viz. the *"field-tested and grounded technological rule"*, the core of which is a "*solution concept"*. This discussion is inspired by what may be called the "design sciences", like medicine and engineering. The mission of a design science is to develop knowledge that the professionals of the discipline in question can use to design solutions for their field problems. This mission can be compared to the one of the "explanatory sciences", like the natural sciences and sociology, which is to develop knowledge to describe, explain and predict. Much of the academic research in organization and management is based on the approach of the explanatory sciences. One may call the resulting descriptive knowledge Organization Theory and the solution-oriented knowledge, resulting from research based on the approach of the design sciences, Management Theory.

In this article I discuss the nature of field-tested and grounded technological rules in management as products of Mode 2 knowledge production, the development of these rules and their use in actual management practice, and the potential for cross-fertilization between Management Theory and Organization Theory. The basic aim of the article is to support the call for more Mode 2 research in management by articulating the nature of the resulting Management Theory and its use in actual management practice.

Improving the relevance of the products of academic management research

There is a long-standing debate on the relevance of academic research products in the field of organization and management. As long ago as 1978 Susman and Evered remarked: "There is a crisis in the field of organizational science. The principal symptom of this crisis is that as our research methods and techniques have become more sophisticated, they have also become increasing less useful for solving the practical problems that members of organizations face" (Susman and Evered, 1978, p.582). In 1982 the *Administrative Science Quarterly* devoted a Special Issue to this problem

(Beyer, 1982). In this issue Beyer and Trice remarked, "Recently (...) scholars have expressed concern about why organizational research is not more widely used (Beyer and Trice, 1982, p.591). Thomas and Tymon (1982) cite an impressive list of criticisms with respect to the relevance of academic organizational research, while, according to a survey at the time, academics considered only some 20% of well-established academic organizational theories as having a better than questionable usefulness (Miner, 1984). At the launching of their academic journal, *Organization Science,* Daft and Lewin also expressed concern about the relevance of received academic organizational theories (Daft and Lewin, 1990). Mowday (1993) voiced similar concerns with respect to publications in the *Academy of Management Journal*. The relevance issue is present not only in the field of management-in-general, but also in various functional disciplines, like marketing and accounting (Aldag, 1997, p 8). Several presidents of the American Academy of Management have addressed the issue, including Hambrick, 1994, Mowday, 1997 and Huff, 2000. In 2001 the *Academy of Management Journal* published a special issue on the interaction between academics and practitioners, which was also prompted by the problem of external relevance (Rynes, Bartunek and Daft., 2001). In the same year the *British Journal of Management* published a Special Issue on the relevance gap in academic management research and on ways to bridge this gap (Hodgkinson, 2001).

Three approaches to improve the relevance of academic management research

Among the possible approaches to improve the use of products from academic management research three can be mentioned . The first one deals with improving communication with practitioners about these products, assuming that they are valid and relevant, but that they are not adequately presented to the world of business and management. Improved communication is the solution Hambrick (1994) proposes and which still receives much attention, and rightly so, see e.g. Wilmott (1994) and Kelemen and Bansal (2002).

A second approach that receives much attention nowadays, is to look at the process that produces these research products. More particularly, to intensify the researcher-practitioner interaction during this process so the researcher gets a better understanding of field problems, their possible solutions, the needs of practitioners and the intricacies of effective communication with practitioners. As already mentioned this is the case in the collaborative research and Mode 2 research. Of course, in our field this is not altogether new, as intense researcher-practitioner collaboration has been practised in various forms under the umbrella label of Action Research (see e.g. Clark, 1972; Argyris, Putnam and McLain Smith, 1985; Eden and Huxham, 1996; Reason and Bradbury, 2001).

Both better communication and research-practitioner collaboration may well contribute to improved use of academic research products in management. A third approach can be to take a hard look at the nature of these products themselves. As discussed below, Gibbons et al. (1994) are sceptical of the potential of Mode 2 research to produce general knowledge. The emphasis they place on the contextual nature of the knowledge produced, suggests that their main ambition is knowledge production in the context of their immediate application, rather than the production of knowledge that may be transferred to other contexts. The same applies to a significant segment of the Action Research approaches, see e.g. Reason and Bradbury (2001). However, on this issue I follow Eden and Huxham (1996), who contend that in order to label a certain collaborative problem solving activity as research, it should produce knowledge that can be transferred to contexts, other than the one in which it was produced.

In the present article this third approach to the improvement of the relevance of academic management research is pursued.

Descriptive versus prescriptive knowledge

Knowledge produced by academic management research can be of a descriptive or a prescriptive nature. In the first case a given organizational phenomenon is described and possibly explained in terms of some independent variables. Generally, the development of descriptive knowledge is theory-driven, focusing on existing situations. The development of prescriptive knowledge, on the other hand, is rather field problem driven and solution-oriented, describing and analysing alternative courses of action in dealing with certain organizational problems.

The classic authors of our field like Taylor, Fayol and Barnard did not shrink from prescription, but the subsequent scientization of our field has greatly diminished the academic respectability of prescriptive knowledge. This is to some extent due to the sometimes limited justification given for the prescriptions. A more important reason may be connected with fundamental ideas on the mission of academic research. Many researchers feel that the mission of *all* academic research is limited to producing shared understanding, i.e. its mission is to describe, explain and possibly predict (see e.g. Nagel, 1979, and Emory, 1985). Developing prescriptive knowledge is regarded as rather un-academic. Prescriptive literature still abounds, but now mainly in the form of "management literature", dubbed "Heathrow literature" by Burrell (1989) or "literature on principles" (of management) by Whitley (1988). Generally speaking, this type of literature is weak on justification and tends to oversell its contribution to problem solving. It is widely sold, but – understandably – disliked by most academics.

The thesis advanced here is that the relevance of the products of academic management research may be improved if they would also include prescriptive, or solution-oriented knowledge. In what follows the term "prescriptive" is avoided because of its connotations with medical doctors prescribing certain medicine to their layman patients, who in principle have no other choice but to obey the prescription. For similar reasons the term "normative" will also be avoided. Instead the term "solution-oriented" is used, which is more in line with the nature of the researcher-practitioner relationship in the field of management.

In order to gain more insight into the nature of these research products we turn to disciplines like medicine and engineering, for whom the production of solution-oriented knowledge is a natural and respectable objective of academic research. Below I explain why such disciplines may be called "design sciences".

The approach of the design sciences to academic research

In his seminal book *The Sciences of the Artificial* Herbert Simon analysed the fundamental differences between the study of natural systems and the creation of artificial one's (Simon, 1969, 1996). Following this line of thought, a distinction can be made between "explanatory sciences", like physics, biology, economics and sociology, and "design sciences", like medicine and engineering (Van Aken, 1994; 2004). The core mission of an explanatory science is to develop valid knowledge to understand the natural or social world, or – more specific – to describe, explain and possibly predict. The core mission of a design science, on the other hand, is to develop knowledge which can be used by professionals in the field in question to design solutions to their field problems. Understanding the nature and causes of problems can be a great help in designing solutions. However, a design science does not limit itself to understanding, but also develops knowledge on the advantages and disadvantages of alternative solutions.

This distinction between explanatory and design sciences is, of course, similar to the one between the so-called "basic" and "applied" sciences. However, I prefer to avoid these terms as they suggest that sciences such as medicine and engineering merely apply the results of the true "basic" sciences, thus negating the extensive and significant scientific knowledge that these sciences generate themselves. The term "design science" is chosen to underline the orientation on knowledge-for-design (of solutions for real world problems), and not on action itself and the skills necessary for adequate action, which is the domain of practitioners.

Research in the explanatory sciences

In the explanatory sciences academic research can be seen as a quest for truth. It is description-oriented and aims at shared understanding. The typical research product is the causal model, with the natural laws of physics as the example to follow. If such laws are beyond reach, as in the social sciences, the aim at least is to reach shared understanding of causal patterns, shared between the researcher and an informed audience (Peirce, 1960). The students in these disciplines are trained to become researchers which enables them to contribute to the collective understanding of their field.

Research in the design sciences

In the design sciences academic research objectives are of a more pragmatic nature. Research in these disciplines can be seen as a quest for understanding and improving human performance. It is solution-oriented, using the results of description-oriented research from supporting (explanatory) disciplines as well as from its own efforts, but the ultimate objective of academic research in these disciplines is to produce knowledge that can be used in designing solutions to field problems. Their students are trained at professional schools to be professionals, who are able to use the *general* knowledge of their discipline to design *specific* solutions for specific problems. Training researchers is seen largely as a by-product and the professionals are supposed to contribute to their disciplines by reflecting on their cases and publishing their insights so they may be used in handling similar cases. Many academic researchers in these fields started their careers as professionals.

The typical research product in a design science is the *technological rule* (Bunge, 1967) and not the causal model.

Field-tested and grounded technological rules

In his philosophy of technology a technological rule is defined by Bunge (1967, p.132) as "an instruction to perform a finite number of acts in a given order and with a given aim". I use this powerful concept in a somewhat more general way. To me a technological rule is "*a chunk of general knowledge linking an intervention or artefact with an expected outcome or performance in a certain field of application"*. "General" in this definition means that it is not a specific solution for a specific situation, but a general solution for a type of problem. (On the other hand, a technological rule is a mid-range theory, whose validity is limited to a certain application domain). If a rule is "field-tested" this means it is tested in its intended field of application. If it is "grounded" this means it is known why the intervention or artefact gives the desired performance.

A technological rule follows the logic of "if you want to achieve Y in situation Z, then perform action X". The core of the rule is this X, a general *solution concept* for a type of field problem. The remainder of the rule is a kind of user instruction connecting the solution concept with the field problem, including indications and contra-indications, i.e. knowledge on when to use the solution concept and when not to. The solution concept can be an act, a sequence of acts, but also some process or system.

There are *algorithmic* rules which can be used by the practitioner more or less as an *instruction*. Typically, these rules have a quantitative format and their effects can be proven on the basis of observations through deterministic or statistical generalization. But there are other rules with a more *heuristic* nature, which can be described as "if you want to achieve Y in situation Z, then perform something like action X". A heuristic rule is more abstract and should be used by the practitioner as a *design exemplar*. The solution concept embedded in the rule is a well-tested, well-understood and well-documented general solution, which should be used as the basis for the design of a *specific variant* of it for a specific case. Typically it has a qualitative format. Its more indeterminate nature makes it more difficult to prove its effects, but field-testing can produce sufficient supporting evidence.

The application of a heuristic technological rule means that practitioners do not have to design a solution for their problem from scratch, as would be the case for a totally novel situation, but that the design assignment consists of choosing the right solution concept and then designing a specific variant of the solution concept to suit their specific situation. (Variant design is a routine approach to non-radical design assignments in engineering, see e.g. Fowler, 1996, for the use of variant design in mechanical engineering). Choosing the right solution concept and using it as a design exemplar to design a specific variant of it presumes considerable competence on the part of practitioners. They need a thorough understanding both of the rule and of the particulars of the specific case and they need the skills to translate the general into the specific. Much of the training of students in the design sciences is devoted to learning technological rules and to developing skills in their application. In medicine and engineering technological rules are not developed for laymen, but for competent professionals.

An example of an algorithmic technological rule is: in order to treat disorder Y in adult males, you administer 0.3 milligram of medicine X during 14 days. An example of a heuristic rule is: in order to treat disease Y in elderly people, use a combination of radiation therapy and chemotherapy and adapt the dosages to the condition of the patient. Using such a heuristic rule implies that you still have a lot to do in designing your specific course of action.

Technological rules do not have to be formulated in the format given above; this format is only given to describe the intervention-outcome *logic* of a technological rule. The actual description of a rule may fill an article, a report or even a whole book. For instance, in mechanical engineering a set of drawings of a certain transmission system with a description of its application domain and its advantages and disadvantages can be seen as a technological rule: use this solution concept if you want to achieve these advantages in this application. I use the terms "technological rule" and "solution concept" here to designate the general knowledge one can use to design a specific intervention (or series of interventions or system or process) to produce a certain desired outcome or performance in a given setting.

Breakthrough by testing and grounding

Mankind has a long tradition of developing technological rules. Early man developed rules to produce artefacts like bows and arrows, and more advanced societies developed rules for building the complex irrigation systems along the Tigris and Euphrates, the medical insights of Hippocrates and the building insights of Vitruvius, to name but a few.

Over the centuries engineering and medicine made significant progress, but the real breakthrough came with scientization after the Enlightenment. Scientization transformed these fields from practice-based crafts into research-based disciplines. They used the research methods and products of the natural sciences to develop field-tested and grounded technological rules. Their rules were *tested*, using the methods of the natural sciences and *grounded* on the laws of nature and other insights produced by these sciences. It is, for instance, possible to design a successful aeroplane by trial and error, as did the Wright brothers, but the design of further improvements is much more effective and faster if they can be grounded on the research results from fields like aerodynamics and materials science. At first, the actual development of technological rules in these design sciences was done predominantly by professionals, but later on and increasingly so, by academic researchers (to which might be added that academic recognition of the design sciences involved quite a lengthy struggle, see e.g. Noble, 1977, for the example of engineering in the US).

Medicine deals predominantly with *improvement problems*, i.e. with designing treatments to improve the well-being of patients. Such treatments are interventions in a given "natural system"; the interventions are designed and applied to change some of the existing operational processes of this natural system in order to improve its well-being. In engineering one also finds improvement problems, like optimizing the output of a chemical plant, but the typical engineering problem is a *construction problem*, i.e. designing a new artefact – process or structure - to certain specifications, such as performance and cost. For improvement problems the realization of a design means changing an existing entity; in construction problems the realization of the design means building a new artefact out of previously unrelated materials.

I now turn to management, which deals with both types of problems.

Field-tested and grounded technological rules in management

The core of a technological rule consists of a general solution to a type of field problem. As already mentioned, the general solution can be in the form of a particular intervention, a series of interventions, or a management system or structure, to be used if one wants to achieve desired results in a given setting. For example, if your company is up against one or more very powerful competitors, use a niche strategy. Or, if your company is confronted with significant uncertainties with respect to future dominant technologies in your industry, use technology road-mapping. Or, invest in winning the trust of a potential partner before entering into contract negotiations. Of course, actual technological rules give much more information than shown here. In management technological rules and solution concepts should be given with “thick descriptions” (Geertz, 1973) to aid their understanding and to facilitate their translation from the general to the specific context. These “thick descriptions” should be based on the field-testing and grounding of the rule. Technological rules in management can be related to improvement problems, like a desired increase in sales or a desired reduction of costs, as well as with construction problems, like the design and implementation of a new organizational structure or management system.

Typically, these rules are developed through multiple case studies with cross-case analyses driving the generation of knowledge (see Eisenhardt, 1989 and 1991, Parkhe, 1993 and Numagami, 1998, on the power of the multiple case study, and Brown and Eisenhardt, 1997, for a good example of a multiple case study). There are two types of multiple case studies. In the *developing* multiple case study a series of problems of a particular type is solved in collaboration between the researcher(s) and the local people (in a kind of Action Research, see e.g. Eden and Huxham, 1996). In the *extracting* multiple case study best practices in solving problems of a particular type are analysed. After the initial series of cases technological rules are developed by reflection and induction and subsequently tested and refined by adding more cases (Van Aken, 2004). The multiple case study operates as a learning system: step by step one learns how to produce the desired outcomes in various contexts.

Technological rules can also be developed on the basis of large scale quantitative studies, like the rule that one should use related rather than unrelated diversification in designing and implementing growth strategies (Rumelt, 1972). In this instance it would also be very interesting to do case studies and to make cross-case analyses and to come to a real understanding of what goes wrong and why if one tries to set up unrelated diversification and furthermore, to come to a more general understanding of the indications and contra-indications for diversification (see e.g. Bettis, 1981).

Besides individual research projects or programmes, technological rules can also be developed through the meta-analysis of sets of previous studies. Systematic review, briefly discussed below, can be a powerful approach to do this.

Justification through field-testing

A key element of a technological rule resulting from academic research is justification. It is obtained through testing the rule in its intended context. At first, during the development of the rule by the researchers themselves through a series of cases, and subsequently by third parties to obtain more objective evidence. Third party testing counteracts the “unrecognized defences” of the researchers (Argyris, 1996), which may blind them to flaws or limitations of their rules. This idea is borrowed from software development where third party testing is called *beta testing* - see e.g. Dolan and Matthews (1993) - and alpha testing by the software developers themselves. Beta testing can be seen as a kind of replication research (see e.g. Hubbard, Vetter and Little, 1998, and Tsang and Kwan, 1999), but its design-orientation makes that it has more in common with evaluation research of social programmes (see e.g. Guba and Lincoln, 1989, and especially Pawson and Tilley, 1997).

The alpha and beta testing of technological rules can offer further insight into the intended as well as the unintended consequences of their application, in their indications and contra-indications, and in the scope of their possible application, their application domain. For algorithmic rules testing can lead to conclusive proof, or at least to conclusive internal validity. The more indeterminate nature of heuristic rules – and in management technological rules will often be heuristic - makes such proof impossible, but alpha and beta testing can lead to “theoretically saturated” supporting evidence (Eisenhardt, 1989).

Grounding on generative mechanisms

Without grounding the use of technological rules degenerates to mere “instrumentalism”, i.e. to a working with theoretically ungrounded rules of thumb (Archer, 1995, p 153). In engineering and in medicine grounding of technological rules can be done with the laws of nature and other insights from the natural and the life sciences (as well as from insights developed by these design sciences themselves). In management grounding can be done with insights from the social sciences. These are not given in the form of general laws. Here the concept of *generative mechanisms* can be used, a concept taken from Pawson and Tilley (1997). They developed this concept in their evaluation research of social programmes, like educational programmes and rehabilitation programmes.

Pawson and Tilley’s point of departure is what they call the basic realist formula *mechanism + context = outcome.* Any social programme can be seen as a coherent set of interventions, applied in some context by some body of actors in order to produce particular desired outcomes. The generative mechanism is the answer on the question “why does this intervention (in this context) produce this outcome?” Pawson and Tilley discuss the example of a programme to improve the safety of a car park. The proposed measures include closing the car park to the non-parking public and introducing closed-circuit television (CCTV) cameras. The generative mechanism for the first measure is that it will become more difficult for potential wrongdoers to enter the car park. For the second one the possible generative mechanisms include that it will deter potential wrongdoers because they believe that this measure will increase the chance of their being apprehended. Insight in the generative mechanisms can help to design improved interventions. In the case of the CCTV cameras it is important that the cameras are very visible and that there are conspicuous notices in the car park drawing attention to their presence. If present knowledge is insufficient to validate certain putative generative mechanisms, further evaluation research and cross-case analyses can be used to do so.

In principle there are two types of generative mechanisms. The first one is a simple stimulus-response mechanism, the effect of the intervention (or system of interventions) being the direct response of the subjects in question to that intervention. Using Woolworth's classic stimulus-organism-response (S-O-R) model (Woolworth, 1921), one can call the second type a S-O-R mechanism, the effect of the intervention depends on the cognitive processes the subjects use to choose their response to the intervention. The generative mechanism producing the outcome of the first measure (the closing of the car park) has a S-R nature, while the generative mechanism of the second one (introducing CCTV) has a S-O-R nature, the expected behaviour of the target group depends on their cognitive processes.

Grounding technological rules in management

Likewise, in management one can ground technological rules in the generative mechanisms that will produce the desired outcomes. Again, these mechanisms are the answer to the question: "Why will this intervention in this context produce this outcome?" Of course, this "why-question", strongly resembles the key question in description-oriented research, leading to some kind of causal model. The biggest difference is that the question is asked of a solution instead of a problem. A further difference is the nature of the independent variable. In description-oriented research this is often a characteristic that is already present in the organization, while in solution-oriented research it is a carefully designed intervention. Moreover, it may be redesigned on the basis of lessons learnt from testing and grounding (and subsequent testing). In description-oriented research the dependent variable is often some operationalization of overall organizational effectiveness (which is notoriously difficult to explain in terms of a limited number of independent variables, see e.g. Lewin and Milton, 1986 and March and Sutton, 1997). In solution-oriented research the dependent variable is often related to some more operational objectives, like increased brand recognition or reduction of overall stock.

As in evaluation research on social programmes discussed above, in the field of management the answers to the "why-question" may be given in terms of stimulus-response mechanisms or in stimulus-organism-response ones. Usually the establishment of a S-O-R mechanism involves an interpretative analysis (see e.g. Aram and Salpante, 2003). An example of a S-R generative mechanism can be found in stock control. If demand and replenishment processes cannot or are not influenced by the stock controller, the performance of the stock control system – delivery performance and stock holding cost - is determined by physical-like, "automatic" processes, i.e. by the application of the stock control rule and the distribution of demand and of replenishment times. An example of a S-O-R generative mechanism can be found in strategic decision-making. According to Wooldridge and Floyd (1990) involving middle management contributes to higher performance. This outcome can be grounded on two social mechanisms. Firstly, middle management can provide sound input to the decision-making process through their more immediate knowledge of the opportunities and threats facing the company. Secondly, it increases the level of consensus on and understanding of the new strategy, which facilitates implementation. One can also say that participation facilitates the second redesign, which is discussed below.

Field-testing a rule can provide insights both in driving and in blocking generative mechanisms. Cases where the rule works less well can be just as interesting as successful ones, since they provide insight in the blocking mechanisms or in the limitations of the application domain of the rule.

The use of technological rules in management practice

"Management is the art of getting things done by people" (as Mary Parker Follett has said). Most managers are able to get things done without much designing, relying on their social skills and on improvisation driven by intuition and experience. Nevertheless, the sound design of interventions and of management structures and systems (called "management action" from now on) can be a great help in getting the right things done at the right time, at the right cost. The development of technological rules is based on this idea of managing as designing or at least as reflection-in-action (Schön, 1983).

However, in the design of management action technological rules are not to be used as *instructions*, but rather as *design exemplars*. As in other design sciences practitioners have to choose a technological rule (or solution concept) for their organizational problem and then they have to translate this general rule to their specific problem by designing a specific variant of it. And, as in other design sciences, the effective use of a technological rule needs considerable expertise: a thorough understanding of the rule with its indications and contra-indications, a thorough understanding of the local situation, cognitive skills in translating the general to the specific and social skills to mobilize the organizational actors to act according to the design. (Academic doubts on the applicability of prescriptive knowledge in management are often based on the – usually implicit – idea that such knowledge should be applied as an instruction to be followed unquestionably, instead of as a general basis for the design of specific management action by competent managers, see e.g. Beer, 2001).

In organizational settings the redesign from the general to the specific by one or a team of practitioners (the change agents) is to be followed by a *second* redesign, i.e. the design of their behaviour by the organizational actors themselves and also their collective construction of new organizational realities like new department boundaries (see also the discussion on prescriptive knowledge to inform the self-design by organizational actors in Mohrmann, Gibson and Mohrmann, 2001). In the field of organization and management designs are not made for innate material or for robots, but for individuals and groups that possess self-organization and self-control. If all goes well, the design by the change agents – usually confined to redesigning the formal organization – conditions the subsequent design of behaviour and creation of organizational realities through the self-design by the members of the organization.

The biggest consequence of this second redesign is that in organization and management the change agents or designers have much less control over the realization of their design than in other design sciences. This has advantages as well as some disadvantages. The disadvantages include the uncertainties the second redesign introduces and the risks of suboptimal realization. The advantage is that the change agents do not have to design everything in detail (as they have to do in case the design concerns the behaviour of robots), but that much can and should be left to self-design. The actions of the change agents can even trigger effective emergent designs and strategies (like in the well-known Mintzberg-scheme – see Mintzberg, 1987 – a realised strategy is usually a combination of a part of the designed strategy and of emergent strategies). This second redesign is, of course, not unmanaged. Usually it is monitored and action may be taken if the redesign process does not proceed satisfactorily.

Organizations have a hybrid nature, they are at once *artefacts*, created by the conscious designs of their founders and of subsequent change agents, and *natural systems*, developing naturally through the social interactions between the various internal and external stakeholders and through their learning processes. As stated earlier, in engineering there are construction problems to be solved by the design and subsequent building or assembling of an artefact. In organization and management one may have construction-like problems, like the design of a stock control system or the redesign of an organization structure. However, in essence the problems in organization and management have more in common with the improvement problems of medicine, which are to be solved by intervening in a natural system, after which the processes of this natural system have to realise the desired improvement. Moreover, in the case of the above-mentioned construction-like problems much still needs to be fleshed out by subsequent self-design.

This dual redesign implies that there is only a long-standing relationship between the formal technological rule and ultimate performance. The redesign from the general to the specific is a feature of every design science, but the vagaries of the second redesign and the fact that in most settings external factors have much more impact on ultimate performance than is usual in engineering and medicine, necessitate one to be modest with respect to the contribution of the formal technological rule to ultimate performance. One might compare the formal technological rule with a map for a South Pole expedition. It is a valuable asset to realize eventual success (reaching the South Pole and returning home safely), but success is not guaranteed. The quality of the people involved, leadership and resources, perseverance and luck also play a part. Nevertheless, a good map is still highly valued by the members of the expedition. Similarly, valid management knowledge produced by academic research could be very valuable, but is is no guarantee to success either.

Three examples of research aimed at technological rules

Some examples may help to clarify this presentation of the development of technological rules in management. The first one concerns the development of a system to identify and manage the risks of New Product Development (NPD) projects by Halman en Keizer (Halman,1994; also reported in Halman and Keizer, 1994). First they developed a version of the system through a series of developing case studies and then they had it beta-tested and reined under their supervision by students in various settings. The system involves the identification and assessment of technological, commercial, financial and operational risks of a development project, to be carried out at the end of the feasibility phase – before major development resources are committed – by a variety of internal and possibly external experts, both in individual and in group settings. Subsequently, a plan of action is developed to handle the risks. Further work on this method has been published in Keizer, Halman and Song (2002). It concerned the company-wide implementation of the method by a large multinational company.

A second example is the development by Verweij (1997) of a participative design method for the redesign of the organization of the shop floor of industrial SMEs (small and medium sized enterprises), that use small-batch assembly operations. SMEs tend to have limited access to the extensive literature on production organization and control and tend to have only limited funds to hire consultants to do the redesign job. Verweij's method intended to make this literature available to SMEs with minimal use of external facilitators. On the basis of the literature and a recent EU project in which he had participated, Verweij developed a so-called PDL (Production Description Language), in which the present and possible future set-up of the shop floor can be represented and analysed. The PDL included descriptions of six different state-of-the-art solution concepts for the organization of small-batch assembly operations, together with their indications and contra indications. The PDL was tested by experts and by a number of mini field-cases. Then he proceeded to develop the PDLM (PDL-method), a participative way in which PDL can be used in actual re-design projects. PDLM was subsequently tested and refined in three re-design projects in three different SMEs and proved to be successful. Grounding can be done in the same vein as participation in strategic decision-making, discussed above: on the one hand the participative process and the collective use of PDL elicited the – largely tacit – knowledge of the people on the shop floor on the present operations and their problems and on the other hand the common understanding and feeling of ownership of the new set-up paved the way to implementation.

The third example can be seen as a kind of beta-testing of the approach of developing field-tested and grounded technological rules, as the present author was not involved in this research project. It concerns the development of a method for the valuation of the intangible assets of a company by Andriessen (2003). His – descriptive – research objective was "to develop knowledge about the valuation of intangible resources, especially about the characteristics and purposes of valuation and the use of valuation methods", and his – solution-oriented – design objective was "to develop and test a method for the valuation of the intangible resources of an organization, as well as a plan for its implementation" (Andriessen, 2003, p12-13). The project started with a review and analysis of twenty five existing valuation methods. Subsequently, on the basis of these analyses a new valuation method was developed by a design team in which he participated. This was done in four consecutive iterations, each version being "desk tested" by the team and by outsiders. Then his method was field tested in six case studies. Interestingly enough not every test was entirely successful, which led to further refinement of his method and to increased insight into the indications and contra-indications for its use. He also made an effort regarding grounding by answering the question "why does the method work and – if not – why not". As Andriessen was not directly involved in some of the case studies, his field testing also included beta-testing. All in all, Andriessen's work can be regarded as a good example of developing field-tested and grounded technological rules.

The relations between Management Theory and Organization Theory

A significant part of the researchers in our field of organization and management feel that academic research has a predominantly explanatory mission, a mission to develop theory to describe, explain and possibly predict organizational phenomena. Many years ago now this induced Miner "to suggest a change in the title of the major professional organization to the Academy of Organization Science and in its publications to the *Academy of Organizational Science Journal* and *Academy of Organizational Science Review"* (Miner, 1984, p 304). This rather revolutionary suggestion was not acted upon. However, in the present discussion on the distinction between an explanatory and a design-orientation for academic research, Miner's suggestion can be put to good use by calling descriptive theory in our field Organization Theory and solution-oriented theory Management Theory. As I explain below, there is much potential for cross-fertilization between the two.

A technological rule can be seen as a design proposition (Romme, 2003), linking a certain intervention (or system of interventions) to a certain outcome while an untested technological rule can be seen as a preliminary design proposition. Such a proposition shares important similarities with a causal proposition, resulting from descriptive research, explaining the behaviour of one or more dependent variables in terms of the behaviour of an independent one. However, in order to be a design proposition, a proposition should satisfy three conditions:

1. the dependent variable must describe something of value to the organization, like financial performance or some operational performance indicator like in NPD the development throughput time and in operations the inventory level
2. the independent variables must describe something that can be changed or implemented by the designers, not something like the age of the organization
3. the proposition must have been tested in the intended context of application.

Generally the results of descriptive research - Organization Theory - can be translated into technological rules - Management Theory - provided that they satisfy conditions 1 and 2. Examples are:

– if you want to realize a large-scale, complex strategic change, use a process of logical incrementalism (Quinn, 1980)
– in strategic decision-making substantive/cognitive conflict can improve the effectiveness of the process, while interpersonal/affective conflict decreases this effectiveness. So try to defuse the latter conflict and create room for the former (Amason, 1996; Eisenhardt, Kahwajy and Bourgeois III, 1997)
– if you want to decentralize decision-making to the level of business units, do not coordinate their activities through direct supervision, but rather through "standardization of output" (Mintzberg, 1979).

However, results as these are still preliminary design propositions as long as they have not been field tested as such, thereby fulfilling the third condition. Almost invariably descriptive research is research with hindsight, while testing technological rules (be it alpha or beta-testing) is research with foresight. Descriptive research tends to be analytical, breaking down complex phenomena into their component parts. It may be partial, focusing on some aspect or component of a whole. By contrast, a design proposition must be tested holistically, each and every part and aspect of it and of its context of application may effect its performance. Besides, its application will often have both intended and unintended consequences. Holistic field testing, implied in the third condition given above, is essential as even solidly grounded technological rules retain a black box character to some extent, with both known and unknown factors contributing to its performance. This holistic relation between an intervention or system of interventions and its outcome has been called "design causality" by Argyris, as opposed to "component causality" (Argyris, 1993, Appendix).

An important additional result from field testing is that it enables one to develop more sophisticated and elaborate technological rules. In the words of Argyris (Argyris, 1993, Appendix), it can support the conversion of *applicable* descriptive knowledge into *actionable* design knowledge. For instance, it is one thing to suggest that giving room to substantive conflict and defusing interpersonal conflict may increase the effectiveness of strategic decision-making, but quite another to put these rules into practice. Grounding, and the evidence from field testing, can then lend much needed support in the process of translating the rule to specific interventions in a specific situation.

Organization Theory research can produce important input for Management Theory research by providing profound understanding of organizational phenomena which can be used to formulate tentative technological rules and to establish the generative mechanisms that produce their outcomes. Conversely, the testing of technological rules may lead to new research questions which may be answered through explanatory research. Such a collaboration between Organization Theory and Management Theory may be compared to the collaboration between the life sciences and medicine and between the natural sciences and engineering. Organization Theory will predominantly result from Mode 1 research and Management Theory from Mode 2 research. The call for more Mode 2 research in our field does not imply that one should do away with Mode 1 research (Hodgkinson, 2001). On the contrary, both are needed and it is the combination of the two that will make our field move forward.

An effective way to exploit the collaboration between Organization Theory and Management Theory can be the use of *systematic review.* See e.g. Pawson, 2002, and Petticrew, 2001, on the general idea of systematic review, and Tranfield, Denyer and Smart, 2003, for its application in the field of organization and management. The latter give a detailed account of systematic review as a method to question the literature, relevant for a specific research question, in a systematic way and on the basis of formal criteria, established beforehand. In such a systematic review a wide variety of literature can be included and used to synthesize all the valid as well as tentative conclusions on the issue in question. A "hierarchy" of evidence can be used, as results with evidence low in the hierarchy can still be important enough to include, both for designing action and for further research. The output of a systematic review can consist of these conclusions, in terms of understanding the issue in question and in terms of alternative technological rules to do something about it. Another important output consists of research questions, for further understanding and for further testing and development of technological rules. In this way a sound systematic review can provide an effective platform to synthesize Organization Theory and Management Theory results, and to generate research questions for both.

One word of caution, however. Systematic review was originally developed as the basis for evidence-based medicine, where evidence is sought on the efficacy of various medical treatments (Hunt, 1997). According to Pawson, in medicine the intended output consists primarily of “best buys”, i.e. which treatments work best. In the context-sensitive field of management, where, furthermore, the effects of interventions are dependent on the cognitive processes of subjects (the S-O-R mechanisms), such a limited intervention-outcome approach may be too sterile. Therefore, in the social sciences Pawson calls for systematic review, aimed at developing theories about why certain interventions work (Pawson, 2002, p212 and 214). In other words, aimed at the development of knowledge on the generative mechanisms (both S-R and S-O-R), connected with interventions.

Discussion

The development of field-tested and grounded technological rules in organization and management may raise a number of issues, three of which are discussed in this article.

In the first place the term “technological rule” may suggest to some a technical, rather mechanistic approach to management, while the idea of managing as designing may evoke associations with the so-called design school in strategic management (Mintzberg, 1990), in which rational planning is the essence of (strategic) management. Hopefully, such concerns have been taken away by the discussion of the use of technological rules in actual management practice, including the redesign from the general to the specific, the opportunities and problems of the second redesign by the organizational actors and the interpretative analyses needed to establish S-O-R generative mechanisms.

The issue of generalization across situations

Next there is the frequently quoted issue of the generalization problem. Or, rather the problem of transfer as MacLean, MacIntosh and Grant (2002, p202) put it : “the real issue is not that of generalizability but that of transfer”. In our field the development of technological rules implies the transfer of context-sensitive knowledge from the context in which it was produced to other contexts. In this respect I follow the position of Eden and Huxham (1996) on Action Research, who claim that an activity may only be called “research”, if it produces knowledge with validity outside the context in which it was produced. However, some authors see the possibilities of such a transfer as problematic: knowledge is not a “thing” that can be transferred. They reject a linear view on the transfer of knowledge. Instead they prefer a constructionist view of knowledge transfer (see e.g. Gergen, 1982, and Gibson, 1999). Gibbons et al.(1994), who initiated the discussion on Mode 2 knowledge production, seem to take a similar epistemological position. According to MacLean, MacIntosh and Grant (2002, p193), Gibbons et al. hold a “view of theory as context-specific and transient in mode 2”. Aram and Salipante (2003, p202) feel that Gibbons et al. are “indifferent or at best sceptical about generalizing across situations to create theory” and that they “make an explicit point of suggesting that knowledge created from Mode 2 processes is highly personalized and not codified”. A similar indifference to generalizing across situations is often found in Action

Research (see e.g. Reason and Bradbury, 2001). Aram and Salipante (2003) give a good example of this epistemological debate between the potential of the general versus the contextual. They describe a discussion between Ansoff and Mintzberg on the nature of strategic knowledge, in which Ansoff stands for a general principles position and Mintzberg holds a contextualist position. General principles are assumed to be transferable, which context-sensitive knowledge is not. In addition to this epistemological debate, one might also pose the more fundamental ontological question whether there exist general phenomena in the social world on which one might develop general knowledge.

The reluctance of Mode 2 and Action researchers to aim for the general is only natural, as the nature of their research makes them much more sensitive to the contextual nature of the actual use of knowledge, than say, the builder of a quantitative model based on a survey. However, the design perspective on the use of knowledge intends to transcend this epistemological antithesis between the general and the contextual by saying that a general statement is actionable to the extent that it can be translated to the contextual. In the physical world one has universal mechanisms that operate across situations and that can be described by general laws, take Ohm's law, and that are valid across situations. With respect to the social world I follow Numagami (1998). In this world there are no universal mechanisms, human consciousness and reflexivity make general laws impossible. But there are observable stable patterns in social phenomena, which are reproduced by human conduct, consciously or unconsciously, and supported by stable shared knowledge and beliefs. Such stable patterns can be used as the basis for general statements. Technological rules and solution concepts are general statements based on observable patterns of behaviour, that can be transferred and made contextual through the process of redesign from the general to the specific, as discussed at length in the present article.

"Knowledge becomes 'relevant' when it is context specific" write Aram and Salipante (2003, p190). This statement can be extended to "general knowledge is 'relevant' to the extent that it is known how it can be translated to specific contexts". This implies that a certain chunk of general knowledge can be relevant for certain contexts and not, or less so, for others. This relevance is not a dichotomy but rather a continuum from very relevant to hardly relevant. Pelz (1978, p349) distinguishes between conceptual knowledge (knowledge-for-understanding) and instrumental knowledge (knowledge-for-action). In general, instrumental knowledge will be more relevant, or actionable, than conceptual knowledge, but conceptual knowledge can also serve as input for the design of management action.

Writing on evaluation research of social programmes Pawson (2002, p 214) remarks that "…programmes are not portable, ideas are". Programmes are context-specific and are, therefore, not portable as such, whereas the ideas on types of interventions and generative mechanisms behind these interventions are. Likewise, specific management interventions that have worked in context A do not necessarily work in context B. But ideas on types of interventions (or on solution concepts) and especially insights in the generative mechanisms connected with these interventions or solution concepts, developed on the basis of analyses across a number of contexts, can indeed be portable.

"If management could be driven as a design science, why isn't it already one?"

Finally, there is the intriguing question, raised by an anonymous reviewer of an earlier version of this article: "if management could be driven as a design science, why isn't it already one? (…) is it just that we are too young and that nobody has thought of it yet, or could it be that it isn't a design science because it can't be?". This question is the more intriguing when placed in historical context. The scientization of our field was triggered by the Ford and Carnegie Foundation reports on American Business Schools (Gordon and Howell, 1959; Pierson and Others, 1959), which concluded that the field at that time was little more than an experience-based craft. At the onset of the scientization process, which – and this is no coincidence - took place in the heady years of the successes of rational approaches to science and society (system theory, cybernetics, the Planning Programming and Budgeting System (PPBS), man's first steps on the moon), many felt that the new science of administration should be a design science. For instance, in his essay for the first issue of *Administrative Science*, the editor James Thompson described his ambitions with respect to the new administrative science as a design science (without actually using this term), and added "An administrative science will be an applied science, standing approximately in relation to the basic social sciences as engineering stands with respect to the physical sciences, or as medicine to the biological" (Thompson, 1956, p103). So it is not as though nobody had thought about it before. In the course of the following decades, however, design-orientation did not become the mainstream of academic research; instead explanatory research became the mainstream , following in the footsteps of most of the other social sciences. A late example of this shift from design to explanation is the move of one of the leading academic journals in the field, the *Academy of Management Review*. As recently as 1999, this journal dropped its reference to some form of prescription by changing its aim from publishing articles that "advance the science *and practice* of management" (italics added) to one of understanding by publishing articles "that challenge conventional wisdom concerning all aspects of organizations and their role in society" (see its instructions to contributors, before and after 1999).

This tendency of academic research towards the basic, the explanatory, is not confined to our field. Herbert Simon writes "The movement toward natural science and away from the sciences of the artificial proceeded further and faster in engineering, business and medicine than in the other professional fields I have mentioned (..) Such a universal phenomenon must have had a basic cause. It did have a very obvious one. As professional schools (..) were more and more absorbed into the culture of the general university, they hankered after academic respectability" (Simon, 1996, p112). This again caused a tendency to try to become like the dominant academic disciplines, to also become an explanatory science. Nevertheless , according to Simon, there is hope. "It is the thesis of this chapter that such a science of design is not only possible but also has been emerging since the mid-1970s" (Simon, 1996, p113).

In this last quote Simon is referring especially to engineering. However, the present article was also written on the basis of the idea that it is possible to drive academic management research as a design science. The answer to the question why this is not the case already, seems to me to be threefold. To begin with, nowadays, many researchers in our field feel that the mission of *all* academic research should be explanatory, aimed at describing, explaining and predicting. By so doing they perhaps overlook the potential of the design sciences approach to produce respectable academic research products. In the second place expected rewards may play a role. In our field following the explanatory approaches of the dominant academic disciplines is still the highway to academic reputation with its accompanying rewards in research and other funds. As yet, taking the somewhat more unorthodox approach of the design sciences is a more uncertain road to publications in top journals. The third reason may have to do with training researchers. In medicine and engineering researchers are normally trained as professionals before becoming researchers. Researchers and professionals alike share norms and values with respect to solving field problems as well as familiarity with such problems. This is not automatically the case in our field. Nevertheless, there are a number of measures one can take to alleviate this problem. These include doing case research, which exposes researchers to the heat of management problems; doing collaborative or Action Research, which increases this exposure; and of course, doing consultancy work, which is often a good test of the relevance of one's ideas on management. Executive PhD programmes, like the Stockholm FENIX programme reported by Starkey and Madan (2001), can also be a powerful means of infusing the research community with practitioner knowledge and views.

In my view our field is not a design science yet because we are young and because nobody has thought about it; not that it can not be done. Rather, it is because in our field the explanatory orientation has become the highway to academic reputation, because of - see the Simon quote –common academic tendencies. Still, the byway of the design approach can be attractive if one is *genuinely* concerned about the relevance issue. And it can also be deeply rewarding, both intellectually and otherwise, to produce relevant contributions to the solution of relevant organizational problems.

Conclusion

In the old days the classic writers of our field did not shrink from prescription and at the start of the scientization of our field Thompson (1956) described his ambitions with respect to the "new administrative science" in terms of a design science. In the meantime, however, academic interest in prescription has largely disappeared, which to my mind is one of the main causes of the relevance problem of academic research in management.

I used examples from the design sciences medicine and engineering to show that prescriptive or solution-oriented research can deserve academic recognition and I used Bunge's (1967) philosophy of technology and the work of Pawson and Tilley (1997) on generative mechanisms to develop the idea of the field-tested and grounded technological rule as a product of academic research in management with the potential to tackle the relevance problem.

The aim of this article is to call for more research aimed at such solution-oriented research products – Management Theory – to complement the more descriptive Organization Theory. This call is in tune with the calls for more collaborative and problem-driven Mode 2 research in our field (Tranfield and Starkey, 1998, and Starkey and Madan, 2001). It complements this call with an articulation of a possible research project, the field-tested and grounded technological rule, in an effort to stimulate interest in general solution-oriented research products resulting from generalizations across situations.

The scientization of medicine and engineering has turned these disciplines from practice-based crafts to research-based design sciences. The scientization of our field has as yet not followed suit, not because it cannot be done but because this road is more difficult to negotiate. A well-known adage in planned change is that people will change if they are faced with the *combination* of a problematic present and a promising future. As discussed above, for a long time now the old and thorny relevance issue constitutes for many a problematic present. I hope that my presentation of the potential of Mode 2 research aimed at the development of field-tested and grounded technological rules provides a sufficiently promising future to induce change. The combination of descriptive Organization Theory with solution-oriented Management Theory, resulting from Mode 2 research, could be a powerful means to remedy the relevance problem of our field.

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