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The Practical Relevance of Management Research:

Turning the Debate on Relevance into a Rigorous Scientific Research Program

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Abstract
How and to what extent practitioners use the scientific results of management studies is of great concern to management scholars and has given rise to a considerable body of literature. In this paper, we provide a systematic overview of the different streams of this literature, highlighting its strengths and

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shortcomings. We distinguish between two types of literature. On the one hand, there is the programmatic literature, comprising of studies that take the perceived lack of practical relevance as their main point of departure and suggest particular ways of dealing with the issue of relevance in management research. On the other hand, there is the largely neglected descriptive literature, which examines the interplay between management research and management practice. Despite the interesting insights both bodies of literature provide, progress in this area of research is seriously hampered by the fragmentation of the research and the lack of scientific rigor that characterizes many of the studies it comprises. We argue that, in order to advance research on the practical relevance of management studies, it is necessary to move away from the partly ideological and often uncritical and unscientific debate on immediate solutions that the programmatic literature puts forward and toward a more rigorous and systematic research program to investigate how the results of scientific research are utilized in management practice.

Introduction

The practical relevance of management research is an ongoing concern for many management scholars. Broadly speaking, research results can be said to be practically relevant if they influence management practice; that is, if they lead to the change, modification, or confirmation of how managers think, talk, or act (Astley & Zammuto, 1992; Nicolai & Seidl, 2010). Considering that management studies is commonly understood as an "applied" science, this concern for relevance is far from trivial and has fuelled intense debates. As a consequence, a sizeable body of literature on the subject of practical relevance (also referred to as "relevance literature") has accumulated over the years. It is difficult to identify the starting point of the debate. While the number of articles on how to "bridge the relevance gap" has increased considerably since 2000 (Bartunek & Rynes, 2014), management researchers have been concerned with the question of relevance since the early days of the "business school" (Augier & March, 2011, p. 215). As in many other applied disciplines (Stehr, 1992, p. 28), the relevance literature in management studies does not represent a finely structured discourse; the various works are spread across the different sub-disciplines of management studies and only loosely connected to each other. Wensley (2007, p. 9) expresses his surprise about the fact "that to a considerable extent the debate has failed to be cumulative over time but [exhibits] many signs of bald repetition".

At the heart of the relevance literature is the programmatic relevance debate, which consists of publications focusing on the development of suggestions for dealing with the issue of relevance in management research. Apart from a small subset of works by critical management studies (CMS) scholars (Fournier & Grey, 2000), this body of literature takes the lack of practical relevance that is widely perceived to characterize management research (also referred to as the "relevance problem") as its main point of departure. These works try to identify the causes of the relevance problem and suggest various solutions: for example, fostering research in which managers and practitioners collaborate is put forward as a solution to the perceived problem of researchers addressing questions that do not relate to the questions that practitioners need to tackle. Many solutions of this kind were first proposed long ago (e.g. Donham, 1922; Hilgert, 1972). However, the "relevance problem" remains as prominent as ever (Bartunek & Rynes, 2014).

A closer examination of the programmatic relevance literature reveals some fundamental problems. As various scholars have pointed out, the debate is often conducted in an essayistic style (Nicolai, 2004) and represents largely, as Nyilasy and Reid (2007, p. 440) put it, "a collection of ad hoc observations and recommendations without positive support". Bartunek and Rynes (2014) found that the vast majority of publications on the relevance debate are neither empirical nor theory-building, but typically consist of normative opinion statements; a point reiterated by various other researchers (Corley & Gioia, 2011; Wensley, 2007). Augier and March (2011, p. 219) note that the debate on relevance has little chance of being resolved: "A few stylized 'facts' are presented from time to time and some crude empirical summaries are put forth, but support for one side or the other turns less on facts than on perspectives and prior prejudices." Moreover, many of the suggested solutions are based on the somewhat uncritical transfer of concepts from other disciplines to management studies, without a serious analysis of the fundamental differences between the disciplines in question. Another point is that hardly any studies in this body of literature have examined the actual impact of the various suggestions on how research should be conducted or the actual extent to which the suggested solutions increase the practical relevance of management research. On the whole, the programmatic relevance literature largely lacks scientific rigor.

Apart from the programmatic literature, there is another group of more rigorous works on the topic of relevance that can be referred to as "descriptive". This body of literature comprises an array of largely unconnected theoretical and empirical works that examine how management practice deals with the output of management research. It includes studies on subjects such as the "performativity" of scientific theories (i.e. how a theory that is meant to describe the world creates a world that is in line with the theory), the political dimension of science-based advice, or how academic knowledge can serve as a means of "credentializing" (i.e. legitimating) people, skills, or work domains. The "descriptive" literature has generated many important insights, such as the idea that practical relevance can take many different forms (e.g. Astley & Zammuto, 1992; Nicolai & Seidl, 2010), that scientific knowledge is typically reinterpreted when it is applied in a practical context (e.g. Mulkay, Pinch, & Ashmore, 1987), that practice may draw on scientific knowledge selectively (e.g. Nicolai & Dautwiz, 2010), that the adoption of scientific knowledge in practice may also have detrimental effects (e.g. Ghoshal, 2005), and so on. While all these insights illuminate important aspects of the issue of practical relevance, these studies have been largely ignored in the "programmatic" literature.

While the relevance literature as a whole has undoubtedly produced many interesting insights, progress in this area of research is hampered by certain serious shortcomings. As already indicated, the literature is not only highly fragmented but many of the studies it comprises also lack scientific rigor. However, the relevance literature has not been assessed systematically to date. Against this background, this paper sets out to provide a systematic overview of the different streams of literature that have addressed various aspects of this issue, to highlight the strengths and shortcomings of the different contributions to this debate, and to outline the direction that future research on practical relevance should take. We argue that, in order to advance the research on the practical relevance of management studies, it is necessary to move away from the partly ideological and often uncritical and unscientific debate on immediate solutions that the programmatic literature puts forward and toward a more rigorous and systematic research program that investigates how the results of scientific research are utilized in management practice. A systematic research program would not only illuminate the relation between management research and practice, but also serve as a basis for evaluating existing and developing novel suggestions for increasing practical relevance. Considering that there are many indications that most existing suggestions—particularly those on instrumental forms of relevance—are unlikely to have the intended effects, a systematic research program would lead to a more realistic assessment of the opportunities to increase practical relevance. Thus, rather than merely replacing programmatic research with studies on research utilization, we argue that the latter is a precondition for the former.

The remainder of this paper is structured into three sections. In the first section, we will review the programmatic relevance literature. We will organize the various works into several "streams of thought", outlining the causes of the relevance problem that each stream identifies and the solutions it suggests and summarizing the criticism it has drawn. We will close that section with a discussion on the general shortcomings of the programmatic relevance literature. In the second section, we will review the descriptive relevance literature focusing on the different lines of argument that can be discerned and highlighting the insights that the respective works contribute to the general relevance debate. On that basis, in the third section, we will outline a new research program on the utilization of scientific results.

The Programmatic Relevance Literature

The programmatic relevance literature comprises contributions that suggest particular ways of dealing with the issue of relevance in management research.

These contributions are often formulated as an appeal to the scientific community. Although the programmatic relevance debate is fragmented, it is possible to identify—with considerable overlap—different groups of literature representing common "streams of thought" on the subject. Particularly the newer contributions are often grouped explicitly around broader concepts or labels, such as "Mode 2" or "evidence-based management", which helps define different streams. The streams we identified in our literature review are popularization view, institutional view, action research, Mode 2 research, design science, and evidence-based management.

Table 1 summarizes the main arguments and provides a list of the main references to works that support or critique those arguments.

Popularization View

The popularization view can be considered the most traditional approach in the programmatic relevance debate. Proponents of this view are concerned with how existing academic knowledge can be transferred to practitioners. They regard the inaccessibility of research and the use of academic jargon as the most important barriers to relevance (e.g. Bansal, Bertels, Ewart, MacConnachie, & O'Brien, 2012; Duncan, 1974; Hambrick, 1994; Ryan, 1977; Steffens, Weeks, Davidsson, & Isaak, 2014). While the advocates of this view do not doubt the practical value of academic research—"We *could* help" says Hambrick (1994, p. 15; italics added)—they identify a problem in transporting the valuable knowledge from academia to practice. Accordingly, popularization is the key to narrowing or "bridging" the relevance "gap".

Identified reasons for the lack of relevance. One of the earliest contributions to the popularization view is the editorial of the inaugural volume of the *Harvard Business Review* (*HBR*) in 1922, in which the editor deplored the inaccessibility of academic research and named it as a reason why it was not picked up by practitioners (Donham, 1922, p. 3). In the 1970s, according to a little survey by the management researcher Duncan (1974, p. 50), both academics and managers agreed that "ineffective communication and technical vocabulary" were the biggest barrier to the relevance of management research. Lorsch (1978, p. 176) spoke about a "Tower of Babel" situation, while practitioners criticized the "'semantic swamp' of academic jargon" (Charan, Aines, Ball, Knoepfel, & Lancey, 1979, p. 505). Many years later Brennan and Ankers (2004) conducted a survey among managers that was similar to that conducted by Duncan (1974). They also found that the "greatest concern was to do with language" (Brennan & Ankers, 2004, p. 18). The more recent relevance literature reiterates this concern: "We try to impress our colleagues with our intelligence and, in doing so, begin to adopt a jargon that no one outside our immediate circles could understand" (DeNisi, 1994,

Table 1 Programmatic Literature on Practical Relevance

Stream of thought	Causes of the relevance problem identified in the literature	Suggested solutions in the literature	Analytical focus	Exemplary contributions	Counter-arguments in the literature	Critical contributions
Popularization view	• Impossible for practitioners to understand academic research papers • Lack of channels for "transferring" scholarly research to practice • Popularization considered a low-status activity	• Translate (e.g. avoid jargon, include more informative sections on implications for practitioners, use case examples) • Simplify the arguments • Strengthen the transfer infrastructure (e.g. publish articles in bridging journals, hold demonstrations in teaching and consulting) • Increase the prestige of popularization measures	Language and diffusion channels	Choudhury (1986), Cohen (2007), Duncan (1974), Hambrick (1994), Ryan (1977)	• Transfer infrastructure exists but has little effect • Research results get "lost before translation" • Model of linear transfer is too simplistic • Narrow focus on instrumental relevance	Dehler (1998), Nicolai (2004), Shapiro, Kirkman, and Courtney (2007), Schulz and Nicolai (in press)
Institutional view	• Business schools are detached from practice • Teaching staff at business schools lack practical experience • Narrowly specialized research according to disciplines	• Intensify contact to practitioners: for example, partnerships between business schools and the industry, practical internships for business professors • Increase multi-disciplinarity of research	Structure and leadership of business schools	Behrman and Levin (1984), Bennis and O'Toole (2005), Grayson (1973), Hilgert (1972), Oviatt and Miller (1989), Porter and McKibbin (1988)	• Incentives and pressure for relevant knowledge already exist • Lack of links between university and industry is not the real cause of the relevance problem • Risk of going back to the "trade school model"	Augier and March (2007), DeAngelo, DeAngelo, and Zimmerman (2005), Nicolai and Seidl (2010), Zell (2001)

Table 1 Continued

Stream of thought	Causes of the relevance problem identified in the literature	Suggested solutions in the literature	Analytical focus	Exemplary contributions	Counter-arguments in the literature	Critical contributions
	• Too few incentives for practical engagement • Lack of strategic foresight	• Orient management research to medicine or engineering • Change the business schools' incentive systems • Strategic planning			• Academic management research lacks relevance, not business schools as such	
Action research	• Focus on "positivistic" science means that the uniqueness of organizations is overlooked • Misfit between disciplinary structures and the structure of practical problems • Low status assigned to "useful" knowledge	• Address "real-life problems" • Researchers should participate in problem-solving in practice • Valid solutions are workable solutions ("workability criterion")	Research methods	Coghlan (2011), Foster (1972), Levin and Greenwood (2001), Reason (2006), Susman and Evered (1978)	• Workability criterion does not reconcile rigor and relevance • Local problem-solving does not generate general knowledge • The minor influence of action research is a consequence of its poor quality	Kieser and Leiner (2009), McKelvey (2006), Salipante and Aram (2003), Chi Vo, Mounoud, and Rose (2012)
Mode 2 research (and related approaches)	• Focus on traditional research ("Mode 1") and on the wrong; that is, self-produced, problems	• Address "real-life" problems	Context of knowledge production and evaluation	Huff (2000), MacLean, MacIntosh, and Grant (2002), Mohrman, Pasmore, Shani, Stymne, and Adler (2008), Salipante and	• Mode 2 is not supported by rigorous research but is tinged with political commitment	Bartunek (2011b), Beech, MacIntosh, and MacLean (2010), Grey (2001), Kieser and Leiner (2009)

Table 1 Continued

Stream of thought	Causes of the relevance problem identified in the literature	Suggested solutions in the literature	Analytical focus	Exemplary contributions	Counter-arguments in the literature	Critical contributions
	• Misfit between disciplinary structures and the structure of practical problems • Academic peer-review functions as elite academic gate-keeping	• Involve multiple stakeholders in the production of knowledge • Collaborative research • Transdisciplinarity • Integrate practitioners into the peer-review process		Aram (2003), Starkey and Madan (2001), Van de Ven and Johnson (2006)	• Pluralistic criteria for quality do not add up to better science • Mode 2 is not new and suffers from similar problems as action research • No evidence on the advantages of collaborative research • Mode 2 has not lived up to expectations	
Design science	• Management research is oriented to the natural sciences • Lack of academic interest in developing prescriptions • Historically, design-oriented knowledge production have moved from business schools to sites in the economy	• Add a design mode (works/does not work) to science mode (true/false) • Focus on technological rules/design propositions • Use engineering as model for development and evaluation of solutions • Collaborative research	Nature of usable management knowledge	Pascal, Thomas, and Romme (2013), Romme (2003), Romme and Endenburg (2006), van Aken (2004, 2005)	• Misinterpretation of Simon's work (1988) • Management research cannot expect to deliver "technological rules" • Premature analogies to engineering	Pandza and Thorpe (2010)

Table 1 Continued

Stream of thought	Causes of the relevance problem identified in the literature	Suggested solutions in the literature	Analytical focus	Exemplary contributions	Counter-arguments in the literature	Critical contributions
	• Management research does not focus enough on instrumental knowledge					
Evidence-based management	• Managers rely on personal experience, popular bestsellers, or consultants • Lack of systematic collection of evidence	• Take evidence-based medicine as a model for management studies • Synthesize evidence • Educate practitioners	Science system and practitioners' attitudes	Briner, Denyer, and Rousseau (2009), Denyer, Tranfield, and van Aken (2008), Frese, Rousseau, and Wiklund (2014), Rousseau (2006, 2012), Rousseau and Boudreau (2011), Rousseau and McCarthy (2007), Rousseau, Manning, and Denyer (2008)	• Randomized controlled trials are more difficult in management research • Conflicting findings in management research make it hard to provide reliable recommendations • Evidence-based management invites political maneuvers • Evidence-based management is not based on evidence	Arndt and Bigelow (2007), Hodgkinson (2012), Reay, Whitney, and Kohn (2009), Tourish (2013)
Miscellaneous	• Researchers and practitioners live in different "worlds" • Researchers choose wrong theories and methods	• Acculturation to reduce gap between the words of researchers and practitioners	Diverse	Churchman (1964), Corley and Gioia (2011), Daft and Lewin (1990), Donaldson (1992),	Diverse	Diverse

Table 1 Continued

Stream of thought	Causes of the relevance problem identified in the literature	Suggested solutions in the literature	Analytical focus	Exemplary contributions	Counter-arguments in the literature	Critical contributions
	• The peer-review system is too conservative and too slow for delivering creative and timely solutions • Pressures for immediate relevance and "recipes for success" impede the provision of useful information for practitioners • The notion of relevance is too narrow	• Avoid theories whose assumptions rule out practical application • Use better empirical methods (more qualitative/better quantitative studies) • Pursue more transparent and open editorial policies to allow for novel forms of research that might be of higher practical relevance • Accelerate the review process to allow for more timely publication of research results • Reduce the unproductive pressure for immediate relevance to allow for the generation of more useful knowledge. • Maintain a critical distance to managerial interests		Fournier and Grey (2000), Sandberg and Tsoukas (2011), Splitter and Seidl (2011), Willmott (2012)		

p. 157). There might be "many fabulous ideas in more traditional academic research just waiting to get out to a practice-oriented audience [. . .] but often the faculty member himself or herself is the worst communicator of these ideas" (Walsh, Tushman, Kimberly, Starbuck, & Ashford, 2007, p. 151) or the researchers "struggle to translate the scientific message to reach managers" (Roux, Rogers, Biggs, Ashton, & Sergeant, 2006, p. 26). In the popularization view, especially the "academic writing conventions" (Kelemen & Bansal, 2002, p. 98) of top-level scholarly journals are subject to criticism: "the information presented in such articles may be potentially useful to practitioners, but it is inaccessible in the present form" (Buckley, Ferris, Bernardin, & Harvey, 1998, pp. 35–36). According to proponents of the popularization view, not only jargon, but also the empirical methods emphasized in scholarly journals alienate practitioners. "Because [practitioners] do not understand the mathematics and statistics that characterize most contemporary research, many of the articles published in academic research journals today might as well be written in Greek" (Leisenring & Johnson, 1994, p. 76).

In addition to that, various scholars (e.g. Cohen, 2007) have stressed that research articles in leading academic journals cannot be expected to be directly accessible to practitioners. They thus attribute the relevance problem mainly to the lack of an appropriate transfer infrastructure that can "bridge" the relevance gap (e.g. Rynes, Bartunek, & Daft, 2001) and thereby "weld the worlds" of the researchers and practitioners together (Latham, 2007, p. 1027). "It is the breakdown in academia's knowledge distribution system that causes the divide" (Nyilasy & Reid, 2007, p. 428).

Suggested solutions. According to most works in this stream of thought, popularization is the key to overcoming the knowledge-transfer problem. On the basis of a unidirectional sender–receiver model, these authors argue that academic jargon needs to be simplified and "translated in practitioner language" (Kelemen & Bansal, 2002, p. 97) and that researchers have to learn "how to communicate more effectively with practitioners" (Rynes, Giluk, & Brown, 2007, p. 1047). This can include "more concrete examples, adopting more metaphors, and telling better stories" (Aldag, 1997, p. 15). Cascio (2008, p. 464) recommends writing reports in plain language and addressing directly the "how to's and when to's" in order to elaborate clearer practical implications. Some researchers suggest including "more comprehensible abstracts at the beginning of articles or, more radical, more attention being given to 'professional adaptation' sections in journals" (Starkey & Madan, 2001, p. 12) or advise business schools to "hire translators to help market the knowledge that exists in academic research" (Walsh et al., 2007, p. 151).

Other authors focus on measures to strengthen the "research-practice interface in the knowledge flow system [that] is established and maintained by several important linking agents", as Duncan (1972, p. 279) has put it. Executive education, for example, is seen as an "underleveraged vehicle" (Walsh et al., 2007, p. 132) for knowledge transfer (Tushman, O'Reilly, Fenollosa, Kleinbaum, & McGrath, 2007). The same applies to consulting (e.g. Duncan, 1972), (text) books (e.g. Dehler, 1998, p. 77), teaching (e.g. Hambrick, 1994, p. 14), or conferences (e.g. Shapiro et al., 2007, p. 263). Bansal et al. (2012, p. 90) pin their hopes on intermediary organizations that should serve as "boundary spanners". As they write: "With the growth and development of intermediary organizations […], we are confident that the relevance of management research will become more evident" (Bansal et al., 2012, p. 90). The Network for Business Sustainability (Bansal et al., 2012) and the Duke University's Behavioral Science & Policy Center (BSPC) are examples of these kinds of intermediary organization.

Authors discussing the transfer infrastructure attribute a particularly important role to so-called bridging media (Cohen, 2007). After reviewing the relevance problem, Ryan (1977, p. 29), for example, wrote:

> My final suggestion is the founding of a respectable, nonprofit publication designed to evaluate and summarize the best of the current research, and to report on the status of applications of theory and research. […] If this publication were jointly staffed by professors and business executives on loan from their organizations, supported by a small professional cadre, the result should be a respected source for both researchers and users.

In response to the "relevance gap", also the American Marketing Association's Task Force on the Development of Marketing Thought (1988, p. 21) proposed to start a new journal for disseminating academic marketing knowledge to practitioners. Some authors have also argued that greater importance should be attached to existing popularization media such as the *HBR*, *California Management Review*, *Long Range Planning*, or *Sloan Management Review* (Kelemen & Bansal, 2002; Rynes et al., 2007). More generally, proponents of this view call on academics to publish more regularly in practitioner-oriented journals or to use websites, Twitter, and blogs (e.g. Bansal et al., 2012, p. 87).

A further concern, according to proponents of the popularization view, is that popularization is believed not to help or even to harm reputation-building in academia (e.g. Kelemen & Bansal, 2002; Starkey & Madan, 2001). According to Cascio (2008, p. 462), "publishing in bridge or practitioner-oriented journals is viewed pejoratively as 'surrogate consulting'". The perceived lack of incentives for employing popularization measures or its potentially damaging effect on academic reputation is considered the reason for the persistence of the relevance problem (e.g. Wren, Halbesleben, & Buckley, 2007). Bettis (1991, p. 318), for example, notes: "I find it troubling that so-called 'practitioner' or 'trade' journals are not deemed worthy publication outlets at many schools." Thus, the status of popularization measures has to be enhanced; for example, by means of awards for scholarly contributions to the practice of management (e.g. Hambrick, 1994). Oviatt and Miller (1989, p. 310, with reference to Lynton, 1984) think that in order to narrow the relevance gap "greater emphasis and prestige might be given to executive education programs within business schools".

Critique in the existing literature. Given that the popularization view has been around for a very long time and that there have been many failed efforts to put its recommendations into action, it is surprising how little its core arguments have been debated in the literature. As surprisingly, little of the existing critique has been acknowledged in the writings of the proponents of the popularization view. Usually, external stakeholders, in particular the business media, share the popularization view. In one of the few explicit critiques available, Dehler (1998) countered the argument that an underdeveloped "transfer infrastructure" constituted the main reason for the relevance problem, by pointing out that there is already a broad range of popularization media in management. As he put it: "Hence, charges of inaccessibility by business leaders are curious, at best" (Dehler, 1998, p. 7).

Some critics of the popularization view also claim that bridging journals have had little success so far in terms of a broad "transfer" of scholarly management knowledge. Dunbar (1983) found that even in a leading bridging journal like the *HBR*, the "scientific basis for [...] general recommendations was not apparent". Schulz and Nicolai (in press) come to the same conclusion in their analysis of the *HBR*. The authors criticize the "idea of a unidirectional, linear knowledge transfer" (Schulz & Nicolai, in press) and doubt that enhancing the status of popularization media will be the key to solving the relevance problem:

> The problem seems to have deeper roots. The question is not why popularization is unpopular—as we have seen, it is not—but why an outlet like the *Harvard Business Review* acts in contradiction of its own editorial policy and does not fully exploit the resource "science". (Schulz & Nicolai, in press)

As Shapiro et al. (2007, p. 249) argued, attempts to transfer scientific results into management practice might get "lost in translation" but also "before translation": "Focusing only on methods to improve the translation of academic research to management practice, for example, will not close the gap if such research is not aligned with the interests of practicing managers" (p. 261). Scholarly journals might not offer the kind of relevance that practitioners are looking for. For example, managers might expect directly applicable success factors, while academic researchers find it difficult to deliver such knowledge without sacrificing rigor (March & Sutton, 1997; Schulz & Nicolai, in press).

On a more general note, critics have also challenged the implicit assumption of proponents of the popularization view that it is possible to isolate pieces of knowledge from the network of academic research by focusing on "how to's and when to's" (e.g. Starkey & Madan, 2001, p. 12). They argue that research findings are embedded in their scientific context, which needs to be taken into account for these findings to be understood properly. An early critical account from the area of human relations is Mann and Likert's statement:

> In most fields of science it is not necessary for administrators or executives to have a comprehensive understanding of the research in order to utilize the results. [...] But in the field of human relations, effective use of the research findings cannot be obtained merely by an executive issuing an order. (Mann & Likert, 1952, p. 15)

Several authors also question whether management research in general is able to produce instrumental knowledge (e.g. Bartunek & Rynes, 2010; Beyer, 1997; Nicolai & Seidl, 2010; Vermeulen, 2007; Whitley, 1995). For that reason, calls for the formulation of instrumental implications may be a "dubious venture" (Bartunek & Rynes, 2010, p. 109). March (2006, p. 85), for example, stated: "No academic has the experience to know the context of a management problem well enough to give specific advice about a specific situation."

Institutional View

Proponents of the institutional view are mainly early contributors to the relevance debate or science-external observers such as accreditation agencies, policy-makers, business media, or managers. In contrast to the popularization view, they are concerned with the practical relevance of research institutions—particularly business schools—as a whole rather than with the relevance of management research in particular. Typically, this literature does not focus on scholarly research but speaks about "the business school" or "the business professor" in general (e.g. Behrman & Levin, 1984; Bennis & O'Toole, 2005; Porter & McKibbin, 1988). Accordingly, the relevance issue is treated as a problem of the business schools' strategic planning, leadership, human resource development, quality management, stakeholder management, incentive structures, and so on. The contributions to this stream of thought are rarely published in scholarly journals but mainly in popular management journals like *HBR*, the business press, and reports. However, from time to time arguments from the institutional view are also taken up in the academic relevance debate.

Identified reasons for the lack of relevance. Proponents of the institutional view identify the roots of the relevance problem in two influential studies that appeared in 1959: the Gordon–Howell Report, published by the Ford Foundation (Gordon & Howell, 1959a), and the Pierson report, published by the Carnegie Foundation (Pierson, 1959a). Both reports criticized the weak scientific foundation of business schools and the experience-based, vocational character of management curricula. These studies had a strong impact on the business school landscape and prompted efforts to infuse science into Western management education. In fact, proponents of the institutional view argue that business schools overreacted to these reports and sacrificed relevance for rigor. The most influential publication that can be attributed to the institutional view is the Porter–McKibbin report (1988), which was commissioned by the American Assembly of Collegiate Schools of Business (AACSB). This report aimed to correct some of these developments, criticizing management education for being too narrow and overly specialized. The institutional view is still present in the more recent relevance literature. Bennis and O'Toole (2005, p. 96), for example, wrote: "During the past several decades, many leading B schools have quietly adopted an inappropriate—and ultimately self-defeating—model of academic excellence."

According to the institutional view (see also Mintzberg, 2004), one specific reason for the diminishing practical relevance of research may lie in changes in the recruiting policies of business schools:

> In less than two decades, the dominant members in business school faculties were young people, most of whom had never succeeded in business but who could tinker both mathematically and behaviorally with significant problems and who often deluded themselves and others into believing that they have actually found solutions. (Behrman & Levin, 1984, p. 141)

There are concerns "that today's management professors have minimal practical experience and may not possess the ability to integrate problem finding and problem solving across business functions through an understanding of real business world problems" (Buckley et al., 1998, p. 36). In the institutional view, business schools are often compared with law schools or medical schools:

> We cannot imagine a professor of surgery who has never seen a patient, or a piano teacher who doesn't play the instrument, and yet today's business schools are packed with intelligent, highly skilled faculty with little or no managerial experience. (Bennis & O'Toole, 2005, p. 102)

A lack of collaboration or a sense of "disconnect" (Buckley et al., 1998, p. 319) between business schools and the industry is a related criticism. "Business schools, and their faculty, do not interact enough with the business community" (Porter & McKibbin, 1988, p. 308). At the same time, the business schools' organizational structures are seen as unsuited for changing this situation: "The traditional reverence for academic and creative freedom usually ensures that business professors have enormous latitude in deciding what they teach, how they teach, what they research, and, generally, how they spend their time" (Oviatt & Miller, 1989, p. 305).

Furthermore, proponents of the institutional view argue that the "scientization" of business schools has led to the development of functional specializations that are at odds with the "real-life problems" of practitioners. Since practical problems usually cut across different areas, specialization is regarded as a further barrier to relevance: "business faculties tend to be pigeonholed in narrow disciplines, each of which claims to predominate in managerial activities but none of which prepares its faculty, or students, for well-integrated management programs" (Behrman & Levin, 1984, p. 141). In a similar vein, Porter and McKibbin (1988, p. 307) found that there "is a lack of meaningful integration across functional areas" and especially younger faculty members "are too narrowly educated in a functional specialty" (1988, p. 308).

Proponents of the institutional view tend to focus on MBAs as the main "output" of business schools rather than research. There is an implicit underlying assumption that the traditional, peer-review-based academic research system cannot be practically relevant anyway. This is evident, for example, in Bennis and O'Toole's view that by "allowing the scientific model [i.e., the focus on research activities] to drive out all others, business schools are institutionalizing their own irrelevance" (Bennis & O'Toole, 2005, p. 100). On the basis of this premise, incentives for research contradict practically oriented activities such as executive education, consulting, or publishing in practitioner journals. Thus, many proponents of this view see the reason for the lack of practical relevance in the inappropriate reward systems of business schools (Oviatt & Miller, 1989). As Bennis and O'Toole (2005, p. 101) wrote: "Just look at the hiring and tenure process. Deans may say they want practitioner-oriented research, but their schools reward scientific research designed to please academics."

Suggested solutions. Most recommendations of the institutional view aim to intensify the exchange between business schools and the practice community. As Buckley et al. (1998, p. 36) pointed out: "The solution appears to be quite simple: We need to work more closely with our constituencies—those firms and organizations that use the knowledge we generate and the graduates we produce." Grayson gives management scholars the following advice:

> Get out of the monasteries, whether these are universities or staff departments. [...] Learn to live with and in the real world of managers. (Greyson, 1973, p. 48)

Others recommend "faculty internships in business" (Hilgert, 1972, p. 62; Oviatt & Miller, 1989, p. 310), "opportunities for faculty to use sabbaticals, summer breaks, [...] 'scholars-in-industry' programs, or other periods of released time to work within an organization" (Buckley et al., 1998, p. 36). This should work both ways; that is, managers should do internships in business schools (Hilgert, 1972, p. 62). Moreover, "joint industry/academic problem-solving teams" (Buckley et al., 1998, p. 37) should be introduced and Ph.D. students should be required to gain practical experience (Behrman & Levin, 1984, p. 142; Porter & McKibbin, 1988, p. 327).

Concerning the career paths of management students, the "professional schools" (like medical schools) should serve as a model (Porter & McKibbin, 1988, p. 342). Grayson (1973, p. 48) argued that "universities should bring in real managers and involve them directly in problem-solving and joint-learning sessions. Doctors expect to return to medical school as part of their normal development; so should managers." To achieve this, the business schools that pay the professors' salaries should make greater use of their possibility to change incentives systems:

> Based on the med school model, business schools in large research universities would be given the same leeway as other professional schools to set their own standards of performance, for example, counting practical publications oriented toward managers (for example serious books and *Harvard Business Review* articles) as heavily for promotion and tenure as purely scientific and theoretical research. (O'Toole, 2011, p. 381)

Other measures for increasing the relevance of business schools are directed to teaching: "What is clear [...] is that the curriculum needs to reflect, in some way or another, a greater level of cross-functional integration than is currently the case in order to match the multifunctional nature of business problems" (Porter & McKibbin, 1988, p. 323). Reforms should not be restricted to adding new courses. "The entire MBA curriculum must be infused with multidisciplinary, practical, and ethical questions and analyses reflecting the complex challenges business leaders face" (Bennis & O'Toole, 2005, p. 206).

Porter and McKibbin (1988, p. 310) found that many business schools do not "practice what they teach":

> Perhaps the most disturbing finding was the general absence of concern for, or even expressions of awareness of, looming changes in the environment in which business schools will be operating in the next 10 to 15 years. We believe, however, that there are a number of increasing expectations and societal trend which business schools will need to take in to account *if* they are to avoid aimless drift and possible eventual irrelevance. (Porter & McKibbin, 1988, p. 311)

Therefore, they concluded, business schools "must plan strategically" (Porter & McKibbin, 1988, p. 341) and have to become, with the help of continuous quality assessments, more adaptable to environmental change.

Critique in the existing literature. Critics of the institutional view argue that today's business schools have already adopted too many of the recommendations of that view. Augier and March (2007) pointed out that in the 1980s and 1990s, the revolution in North American management education that came after World War II was followed by a "counter-revolution" that was supported by the business press and by elements of the business school community, which "led business schools to seek greater immediate relevance by increasing the emphasis on experiential knowledge in their research and in their teaching, and by decreasing the emphasis on academic knowledge" (Augier & March, 2007, p. 134).

Zell (2001, 2005) studied the different pressures for relevance from "students as customers", funding bodies, or the business press and media rankings. Considering that these developments tend to devalue research, she posed the question: "Has the pendulum swung too far?" (Zell, 2001, p. 324). Business school rankings in particular put pressure on management education for more relevance at the cost of rigorous research. According to DeAngelo et al. (2005, p. 15), however, "this is not a good reason to re-institute the obviously failed strategy of the 1950s!"

Schulz and Nicolai (in press) challenge the institutional argument about the perceived lack of incentives that could prompt researchers to conduct practically relevant research. They argue that the Porter–McKibbin report and similar analyses have had a significant impact on management education, such that there are already significant pressures on business schools to prove their practical relevance. "Today, it is not the scientific status of these disciplines, but rather their social usefulness that is the critical factor in their legitimacy", as they note (Schulz & Nicolai, in press).

Moreover, there are doubts as to whether a lack of university–industry linkages is still at the center of the problem. Even Porter and McKibbin (1988, p. 308) admitted that this was "only partially true" since many business school deans worried that faculty members spent *too much* time on their business contacts. According to DeAngelo et al. (2005, p. 8), this is even more true today:

> Faculty face […] large pecuniary temptations to substitute excessive amounts of highly remunerative outside consulting for their university research, teaching, and service obligations. And so business schools typically constrain faculty consulting, rather than encourage it, as Bennis and O'Toole advise.

As Nicolai and Seidl (2010, p. 1259) pointed out, if the main issue were the university–industry linkages, there should be no relevance problem at all since

> many professors of business studies offer consulting services to companies; business schools offer executive education programmes; companies

> cooperate with business professors at various different levels; students with a particular interest in management practice earn degrees in business schools and afterwards join industry.

Many teaching-related recommendations of the institutional view are already applied in practice, which has also caused criticism. This applies in particular to the re-integration of practitioners into management education. Since the "pendulum has now moved [...] toward greater inclusion of real-world experience [...], many business schools are hiring business practitioners to teach courses", as Clinebell and Clinebell (2008, p. 99) observed. Even though proponents of the institutional view stress that these measures are not meant to "lead us back to a vocational, or trade school, model" (Clinebell & Clinebell, 2008, p. 107), which Gordon, Howell, and Pearson rejected over 50 years ago, critics (e.g. DeAngelo et al., 2005) argue that this is exactly what is happening. For example, referring to the so-called New Economy boom, when "eBusiness" courses entered the curricula, DeAngelo et al. (2005, p. 9) commented:

> To present their schools as "always moving, always doing something", deans pressure faculty to make curriculum changes that appear interesting and fashionable given the current business environment. Continual wholesale programmatic shifts toward detailed treatment of trendy topics is destructive in the long run.

Critical voices complaining about the practical irrelevance of management studies did not become silent in the period following the Porter – McKibbin report. Instead they became dramatically louder. Against this backdrop, Nicolai and Seidl (2010, p. 1260) criticize the institutional view for missing the core of the relevance problem.

> Business professors might act as business consultants, teach executive courses, write popular management books, or supervise practice-oriented dissertations. However, this does not prove that there are meaningful links between such activities and the network of scientific publications [...]. The question of a meaningful connection between the results of scientific research and management practice is at the centre of the debate on relevance.

Action Research

Action research is an early, if not the first, systematic social science approach that takes the relevance issue into its focus. Action research is "traditionally defined as an approach to research that is based on a collaborative problem-solving relationship between researchers and clients" (Coghlan, 2011, p. 54). It aims "to contribute both to the practical concern of people in an immediate problematic situation and to goals of social science by joint collaboration within a mutually acceptable ethical framework" (Rapoport, 1970, p. 499). The foundations of action research lie in the work of Lewin and other social scientists working in the period after World War II (Coghlan, 2011, p. 55; Reason, 2006, p. 187). Unlike most other streams of thought in the relevance debate, action research contains explicit epistemological reflections—for example, it refers to Dewey's (1933) philosophical position of pragmatism (e.g. Greenwood & Levin, 1998). While earlier contributions were more narrowly focused on organization studies and related areas, more recently action researchers have taken a broader perspective and transferred the approach to other management areas, such as accounting (Mitchell, 2002). Some even claim that action research is needed to transform higher education in general (Levin & Greenwood, 2008).

In this section, we focus particularly on "classic" action research in the post-Lewin tradition, which developed mainly in organization studies (Coghlan, 2011, p. 56). Although action research also claims metaphorically to "bridge [...] the gap between theory and practice [and have] an intimate relationship with collaborative management research" (Coghlan, 2011, p. 55), the links to the broader relevance debate in management are generally weak. However, given that some recent contributors of the relevance debate seem to re-invent ideas that originated in this stream of thought, action researchers might reply to them: "Been there, done that" (McKelvey, 2006, p. 825).

Identified reasons for the lack of relevance. In the most frequently cited article of action research (Coghlan, 2011, p. 62), Susman and Evered (1978, p. 582) address the problem of relevance directly:

> 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 increasingly less useful for solving the practical problems that members of organizations face.

According to the advocates of action research, the orientation of management studies toward the traditional or "positivist" model of the natural sciences is the main reason for the lack of relevance: "What appears at first to be a crisis of relevancy or usefulness of organizational science is, we feel, really a crisis of epistemology" (Susman & Evered, 1978, p. 582). For example, action researchers reject the assumption that the world can be reconstructed into laws independently of the human interpretations of these laws. This applies in particular to organizations as human-made artifacts: "In this sense they [i.e., organizations] do not exist independently of human beings, like the planets, just waiting for an Isaac Newton of organizational theory to discover an equivalent of the laws of planetary motion" (Susman & Evered, 1978, p. 584). Thus, practical problem-solving cannot be based on general laws derived from an average of similar organizations. Rather, it depends on the unique history of a particular organization (Susman & Evered, 1978, p. 585).

Moreover, the disciplinary differentiation of traditional, positivistic research is considered unsuited for addressing "real-world" problems: "The world does not deliver social problems in neat disciplinary packages, despite the pathetic insistence of most academic social scientists in defending their academic turfs against all other forms of knowledge" (Greenwood, 2002, p. 127). In this view, discipline-oriented "positivist science" necessarily leads to a loss of relevance. Some action researchers even assume that this model implies a general aversion to relevant knowledge: "Perversely, most orthodox researchers believe that useful work is, by definition, scientifically trivial" (Greenwood & Levin, 1998, p. 239).

Some authors such as Beer (2001) refer to ideas from action research arguing that the relevance problem is an implementation problem. Since management research tends to challenge the status quo, the implementability of research findings and their acceptance by the managers cannot be taken for granted (Churchman, 1964). However, as Beer (2001, p. 59) pointed out, "[f]ew management scholars specify the conditions and processes that managers might use to implement their theories, concepts, and methods. Fewer still consider issues of implementation when choosing their research method." Thus, the findings of traditional management research might not be implemented even if they are in principle useful, because they do not take into account organizational defensive routines and an organization's resistance to change (Argyris, Putnam, & McLain-Smith, 1985).

Suggested solutions. According to many proponents of action research, the relevance problem in management studies can be solved by adopting an action research approach, which in essence aims to dissolve the difference between science and practice. "Instead of viewing relevance and rigor as a dilemma, both are positioned as primary and interwoven criteria for quality research" (Lüscher & Lewis, 2008, p. 223, with reference to Eden & Huxham, 1996). In this connection, action researchers refer to two of Lewin's much-quoted statements: "Nothing is as practical as a good theory" and "The best way to understand something is to try to change it" (cited in Greenwood & Levin, 1998, p. 241). Practical problems are defined in such a way that researchers can deal with them. Then, action researchers go to organizations to solve the problems of practitioners in collaboration with them (Levin & Greenwood, 2001; Reason, 2006). Subsequently, the implications for management science are worked out and published (Elden & Chisholm, 1993; Foster, 1972). Thus, action research is context-bound and addresses "real-life problems":

> Reading other researchers' work as a way of identifying new research questions, the standard practice, is partly supplanted in action research by a more direct process of researching what social stakeholders understand to be pressing problems. (Levin & Greenwood, 2001, p. 105)

Such problems cut across different disciplines. Solving them requires "research expertise from any and all academic and research locations relevant to the problem at hand—engineering, basic science, ethics, education, etc." (Greenwood, 2002, p. 127).

Action researchers believe that "published research is read more by producers of research than by practitioners" (Susman & Evered, 1978, p. 582) and thus do not consider the academic article the primary medium through which research becomes relevant. Instead, they treat the individual action research project that is carried out by ad hoc or permanent face-to-face groups within the organization as the most relevant channel. Susman and Evered (1978) proposed a cyclical process of action research, separated into diagnosing, planning action, taking action, and specifying learning. The last phase aims to identify general findings. The validity of the co-generated knowledge is thereby measured according to the criterion of workability (Levin & Greenwood, 2001, p. 105). Knowledge is considered valid if the actions that arise from it solve an organization's specific problems. "What works" in a given project is not defined by controlled and replicable data but by the fact that the problem is solved to the "satisfaction of all involved" (Levin & Greenwood, 2001, p. 105). This process helps to develop "actionable knowledge" and research findings that are implementable:

> Interventions, including the initial entry into the organization and conversations with management about their problems, allow the action researcher to observe the response (acceptance or resistance) of individuals, groups, and organizations to the intervention, revealing truth not observable in normal science. (Beer, 2011, p. 157)

To sum up, action research claims to be "a corrective to the deficiencies of positivist science by being future-oriented, collaborative, agnostic, and situational, implying system development and so generating theory grounded in action" (Coghlan, 2011, p. 62). Specific approaches within this stream of thought vary widely. Early contributions followed more of a functionalist, problem-driven approach, while more recent contributions are influenced by social constructivism focusing on sensemaking processes within organizations (Lüscher & Lewis, 2008, p. 224).

Critique in the existing literature. The critical debate on the extent to which action research is able to solve the relevance problem is quite diverse, addressing different aspects of the suggested solution. On a more general level, Kieser and Leiner (2009) argue that the core assumption of action researchers about the possibility of reconciling rigor and relevance is questionable and that neither the historical development of action research as a research program nor the scientific quality of its research results lends support to this view. Rapoport (1970, p. 505) suspected that it might be a "cliché" that there is nothing as practical as a good theory: "In fact the two taskmasters [. . .] (the client system and the scientific community) tend easily to become separated". McKelvey (2006) highlights the problems that are associated with the workability criterion. Like other forms of collaborative research, such a consensus-oriented process "consists of pluralistic interests and conflict; there is the risk of decision by committees, power contests, and settling for the lowest common denominator" (McKelvey, 2006, p. 825). As some of the descriptive relevance literature shows, whether organizations adopt specific practices or not does not depend so much on whether these rest on correct assumptions but rather on whether they fit into given social structures (e.g. Beck & Bonß, 1991). Argyris (2003, p. 425) pointed out that the workability criterion might even conflict with the truth orientation of scientific communication, which creates a dilemma for action researchers: "Some would go as far as saying that it might not be necessary or useful for such researchers to be concerned about truth. Indeed, they might find that being helpful requires ignoring truth." This dilemma makes it difficult to identify general findings that transcend the single case (Salipante & Aram, 2003, p. 192), so it remains unclear how local problem-solving can generate theory (Chi Vo et al., 2012, p. 382). Self-critical reviews that bemoan the poor quality of action research echo this problem (e.g. Greenwood, 2002).

In their turn, action researchers bemoan the marginal role that their work plays in the broader management debate (e.g. Greenwood, 2002; Gustavsen, 2003). Results from this kind of research only rarely find their way into prestigious journals (an exception is the study by Lüscher & Lewis, 2008) and are predominantly published in specialized outlets for action research such as *Action Research* or *Concepts and Transformation* (Kieser & Leiner, 2009, p. 524). Some authors attribute the minor influence of action research to a possible reviewer bias against the interdisciplinary, "non-positivistic" (i.e. value-driven, qualitative, local context-oriented, non-replicable, etc.) nature of this type of research (e.g. Greenwood, 2002). However, this argument does not explain why programmatic calls for more action research and related approaches repeatedly get published in leading management journals (e.g. Coghlan, 2011; Susman & Evered, 1978) while the products of action research do not. This shows that this type of research may also be marginal simply because "the potential of action research for generating robust actionable knowledge has not been yet realized" (Coghlan, 2011, p. 53).

Mode 2 and Related Approaches

The Mode 2 literature is a recent stream in the relevance debate in management studies. It emerged around 2000 and has grown dramatically ever since, such that some even say that it has become "fashionable" (Grey, 2001, p. 28). Mode 2 has strong affinities with action research in that it is concerned with interactive, problem-oriented knowledge production (Huff, 2000, p. 288), but it is a distinctive body of literature with different roots and only few cross-references. Historically, the concept of Mode 2 goes back to the work of Gibbons et al. (1994), from where it was transferred to management studies. Gibbons and his colleagues propagate Mode 2 as a "new epistemology" for the whole science system. It implies a "socially distributed" knowledge production and a "de-differentiation" between science and society (Gibbons et al., 1994; Nowotny, Scott, & Gibbons, 2001). In science studies and related areas, Mode 2 led to controversial debates (e.g. Rip, 2000; Weingart, 1997). This did not limit its strong impact on science policy-makers in the U.S. and Europe, who, "in their drive to ensure research relevance" (Swan, Bresnen, Robertson, Newell, & Dopson, 2010, p. 1311), widely adopted this approach (Hessels & Van Lente, 2008). This resulted in policy interventions and research funding strategies that have created pressures for institutional changes in line with the Mode 2 logic (Swan et al., 2010, p. 1314).

Identified reasons for the lack of relevance. Unlike proponents of the popularization view, the proponents of Mode 2 frame the relevance issue not only as a transfer problem but also as a knowledge-production problem. According to them, the orientation of business schools toward "Mode 1" research lies at the heart of the relevance problem. Mode 1 knowledge production is the traditional pursuit of scientific truth. As Mode 1 research aims at "knowledge for knowledge's sake" (Huff, 2000, p. 288), it is almost by definition of limited practical relevance. The university is the dominant producer of discipline-oriented Mode 1 knowledge. Mode 1 is not restricted to the "positivist" model of the natural sciences that action researchers criticize, but encompasses the peer-review system of modern science, including its focus on carefully validated knowledge according to self-developed quality standards. "The emphasis is on knowledge production certified by publication in a very small number of elite journals" (Huff, 2000, p. 288). In line with this characterization, post-modern research published in peer-reviewed scholarly journals is also considered Mode 1 research (Huff, 2000, p. 289).

According to the advocates of Mode 2, one reason for the lack of practical relevance of Mode 1 research is that it tends to deal with the "wrong" problems. As Gibbons et al. write: "in Mode 1 problems are set and solved in a context governed by the largely academic interest of a specific community" (1994, p. 3). Mode 1 researchers "using a disciplinary staff base patrolled by elite academic gate keepers called professors" (Tranfield, 2002, p. 380) do not solve practical problems but invent their own problems or try to fill holes in the existing theory (Huff & Huff, 2001, p. S53). The advocates of Mode 2 share with other authors of the relevance debate (e.g. Galbraith, 1980; Hambrick, 1994, p. 13; Van de Ven, 2000; Vermeulen, 2005, p. 979) and with many practicing managers (Roux et al., 2006, p. 24; Shapiro et al., 2007) the view that traditional management research is of little practical use because it does not address "real-world problems" (Kelemen & Bansal, 2002, p. 106).

Like action researchers, the proponents of Mode 2 criticize the alleged mono-disciplinary nature of traditional research. As practical solutions "cannot be reduced to disciplinary parts" (MacLean et al., 2002, p. 191), the mono-disciplinary orientation and the strict demarcation between scientific and lay knowledge in Mode 1 are a further barrier to relevance. Starkey and Madan (2001, p. 5) concluded: "Arguably, the Mode 1 approach to research and knowledge production is no longer sustainable."

Suggested solutions. According to the advocates of Mode 2, the relevance problem in management studies can be solved by adopting this mode as a "radically different style of knowledge production" (Huff, 2000, p. 288) that, according to them, is already rapidly spreading across academia (Gibbons et al., 1994). In Mode 2, knowledge is developed in the context of application and directly addresses the needs of practitioners (Nowotny et al., 2001); that is, it "is rooted in the tasks at hand" (Huff, 2000, p. 291), such that the generation of theoretical knowledge and its application occurs in the same process. In contrast to traditional research, the research process is a collaborative endeavor. Apart from academics from different disciplines, it includes various practitioners such as policy-makers and consultants (Tranfield, 2002, p. 381). Their social relationships within the research process are heterarchical. This implies that "distinctions between public and private knowledge production have become blurred" (Starkey & Madan, 2001, p. 10).

Mode 2 supports—or is perhaps synonymous with (Pascal et al., 2013, p. 264)—the calls for "engaged scholarship" (e.g. Van de Ven, 2007, 2011; Van de Ven & Johnson, 2006) and "collaborative research" (e.g. Mohrman et al., 2008). As academics and practitioners produce distinct but equally valuable knowledge, collaborating with practitioners is one "of the most widely endorsed suggestions" (Van de Ven & Johnson, 2006, p. 811) in the relevance debate. Research collaborations that incorporate diverse team members are expected to foster creativity, novelty, and more significant advances in knowledge (Van de Ven, 2011, p. 404; Van de Ven & Johnson, 2006, p. 811). The idea is that when laypersons "speak back" to scientists they "contextualize science by attempting to make it 'work' and resonate with their lived experience" (Corburn, 2005, p. 68), thereby producing "socially robust knowledge" (Gibbons et al., 1994). Amabile et al. (2001, p. 430) made some recommendations for increasing the chances of success in such collaborative teams. For example, "carefully select academics and practitioners for diverse complementary skills and backgrounds", clarify "commitments, roles, responsibilities and expectations at the outset", find "ways for the academics and practitioners to get to know and trust each other as people", "occasionally examine the effectiveness of the team's functioning", and ensure "that academics' and practitioners' institutions will be supportive or at least tolerant of their involvement". Van de Ven (2011) expects that researchers who abandon the "traditional approach of going it alone" in favor of collaborative research will have better career prospects: "As a result, research reports based on engaged scholarship should win out in competitive reviews for research funding, publications in journals, presentations at professional conferences, and professional training and development programs over those based on unengaged or disengaged research" (Van de Ven, 2011, p. 404).

The changes in quality control are another important aspect of Mode 2. The peer-review system of Mode 1 is supplemented by further, science-external criteria. As Gibbons et al. (1994, p. 8) explained:

> In disciplinary science, peer review operates to channel individuals to work on problems judged to be central to the advance of the discipline. […] In Mode 2 additional criteria are added through the context of application which now incorporates a diverse range of intellectual interests as well as other social, economic or political ones.

One of these new forms of quality control is the integration of practitioners into the peer-review process.

In general, the recommendations of the proponents of Mode 2 in management are relatively abstract. Burgoyne and James (2006, p. 313) proposed some "best practice principles for Mode 2 research", such as making "dissemination an integral part of the research process" or deciding "how the literature and the existing knowledge base will shape research", but did not specify or illustrate how Mode 2 as a "new epistemology" creates a novel form of knowledge. Considering that the original description of Mode 2 by Gibbons et al. (1994) is very abstract, it is not surprising that some authors who applied this concept to management reiterate well-known arguments, particularly from action research, under the Mode 2 label. MacLean et al. (2002), for example, admited the similarity to action research and could only vaguely describe the differences between the two approaches. Action research, they argued, "does not necessarily require diversity in terms of either the disciplines, participants or organizational location(s) involved" and pointed to the "more subjective and interpersonally creative nature of the outputs" of Mode 2 research (MacLean et al., 2002, p. 202). Chi Vo et al. (2012, p. 382) also observed parallels between Mode 2 and action research, while Coghlan (2011, p. 63) seems to equate these two forms of knowledge production. Levin and Greenwood (2008, p. 221) noted that "[action] researchers have been practicing Mode 2 knowledge production since the first AR-based [i.e. Action-Research based] experiments took place in the 1940s and 1950s".

Critique in the existing literature. In science studies and related fields, the Mode 2 concept has been subject to (sometimes severe) criticism. According to Shinn (2002, p. 604), for example, "[i]nstead of theory or data, the New Production of Knowledge—both book and concept—seems tinged with political commitment". Some authors (e.g. Godin, 1998; Weingart, 1997) find it hard to work out whether Mode 2 is an empirical description of how the sciences have changed or a normative concept of how science should be. Godin (1998) criticized the association of traditional research with disciplinary research in contrast to the new interdisciplinary research propagated by the advocates of Mode 2. Some critics (Weingart, 1997) also doubt that the (in principle indefinite) number of additional criteria for quality will lead to "better science". Instead, they argue that "knowledge is applied strategically along the line of existing conflicts of interest" (Weingart, 1997, p. 603). Weingart characterized Mode 2 itself as an example of uncertain scientific quality and strategic research:

> [T]he most conspicuous characteristic of the new mode of science is its susceptibility to fads and fashions. With the information explosion, and the exponential growth and the "medialization" of science, attention becomes an ever rarer commodity. "Hit-and-run" analyses are an ever greater temptation, especially where society and politics are involved. (Weingart, 1997, p. 592)

The relevance debate in management reflects some of these problems. Grey (2001, p. 28) has objected to the view that Mode 2 is something new for universities:

> The problem with this claim is that it is inattentive to the historical nature of universities, which have always been exhibited to both—if we must use these terms—M1 K [Mode 1 knowledge] and M2 K [Mode 2 knowledge] attributes.

Similarly, Hodgkinson and Starkey (2011, p. 360) admited: that perhaps "we were too infatuated with the idea of Mode 2 and allowed the argument to run too far in this direction. [. . .] The strong anti-Mode 1 argument was overstated." Others who do not want to discard the rigor of Mode 1 research have proposed "Mode 1.5" or "Mode 3" approaches (Huff, 2000; Huff & Huff, 2001), further increasing the ambiguity of the concept.

To the extent that Mode 2 has some parallels to action research, it also suffers from similar problems. Both approaches "remain unclear about what general knowledge can result from efforts to generate theory" (Chi Vo et al., 2012, p. 382). Thus, it is not surprising that Kieser and Leiner (2009, p. 524) did not succeed in identifying any substantive studies in which Mode 2 knowledge has been applied in order to resolve management problems. Bartunek (2011b, p. 556) also had difficulties in finding such studies: "There has been much more discussion of Mode 2 than illustrations of it in academic journals that I can find." Only MacLean et al. (2002), Starkey and Madan (2001), and to some extent also Burgoyne and James (2006) name some management projects in which Mode 2 had been applied, albeit "not very convincingly" according to Kieser and Leiner (2009, p. 524). These studies promote the Mode 2 approach and describe the different steps pursued within a so-called Mode 2 project or enumerate various collaborations between universities and the industry. However, they do not reveal the products of such research (van Aken, 2005, p. 20) and, hence, what specific insights have been developed and how these findings have contributed to the ongoing research debate.

According to Kieser and Leiner (2011, p. 10), the same is true for collaborative research in general: "Empirical evidence demonstrating that collaboratively produced research output, that is, research output coproduced by practitioners who are truly outsiders to the academic system is systematically characterized by high degrees of rigor and relevance is still outstanding." The observations of Beech et al. (2010) contradict the view that collaboration will lead to the "de-differentiation" between academic knowledge and the knowledge of practitioners: "Despite espoused intentions to co-produce knowledge (Rynes et al., 2007), attempts to do so involve both parties unintentionally operating in ways that the dialogue literature suggests will fail. The implicit theories in their talk and text make engaged dialogue less likely" (Beech et al., 2010, p. 1362). Nevertheless, such collaborations may be productive for both sides: "Knowledge is not transferred from academic to practitioner or vice versa, rather it is developed in the joint dialogue and applied, through further work, in the home-worlds of the two groups" (Beech et al., 2010, p. 1364). In that respect, this is what Kieser and Leiner (2011, p. 11) recommend: "Collaborate with practitioners: But beware of collaborative research." At the same time, the authors note that collaborative research, engaged scholarship, or Mode 2 have a high symbolic value for academics as "they signal that they are aware of the lack of relevance in their research but possess the necessary process-knowledge to effectively cope with this problem" (Kieser & Leiner, 2011, p. 23).

Bartunek (2011b, p. 556) believes that Mode 2 did not live up to the high expectations it created. As she writes, "it appears that while Mode 2 has stimulated thinking, it has not bridged academia and practice as much as had been hoped for". Instead of showing "radically new" forms of knowledge production, it is criticized for adding only very few theoretical or empirical insights to the relevance debate. Shinn (2002, p. 610) has pointed out: "The New Production of Knowledge is not a research school, since it does not articulate a research programme." Kieser and Leiner (2011, p. 5) noted, though, that Mode 2 changed the expectations of science-external constituencies:

> Mode 2 is not a new epistemology but rather a description—a description with normative implications—of the ways in which social movements mobilize the public and politicians to redefine the conditions under which—not the methods with which—researchers have to carry out research.

Design Science

"Design science" emerged in the mid-2000s and is another relatively prominent stream in the programmatic relevance debate. Among its proponents are many authors who had been involved in discussions about Mode 2 and saw design science as a "particularly promising" (Hodgkinson & Starkey, 2011, p. 358) approach to management research (e.g. van Aken, 2004, 2005; Denyer et al., 2008; Hodgkinson & Starkey, 2011; Huff, 2000; Jelinek, Romme, & Boland, 2008; Romme, 2003; Romme & Endenburg, 2006; Starkey, Hatchuel, & Tempest, 2009). According to van Aken's (2004, p. 224; emphasis in original) definition, the "mission of a design science is to develop knowledge for the design and realization of artefacts, i.e. to solve *construction problems*, or to be used in the improvement of the performance of existing entities, i.e. to solve *improvement problems*". In contrast to Mode 2, which, as van Aken (2005, p. 20) put it, tends to "focus on the research process and less on the knowledge produced by this process", design science addresses the relevance problem by elaborating on the features of solution-oriented knowledge. In this sense, design science can be seen as an extension of, rather than an alternative to, Mode 2. Some authors, such as Pascal et al. (2013, p. 265), even hold that "'design science' is an ideal-typical form of mode 2 knowledge production". At the heart of the design science approach is Simon's (1988) distinction between natural science and the sciences of the artificial. Referring to Simon, the advocates of design science argue that since organizations are social constructions, the study of management should be understood as a science of the artificial, like architecture or engineering (van Aken, 2005; Romme, 2003), and that this holds the key to the relevance problem. Like action researchers design researchers sporadically refer to pragmatism as an epistemological foundation (e.g. Romme, 2003, p. 558). However, design science claims also to be "fundamentally different [from action research] in its future-oriented focus on solution finding" (Romme, 2003, p. 564).

Identified reasons for the lack of relevance. The proponents of design research identify similar reasons for the lack of relevance as those of other streams of thought. Jelinek et al. (2008, p. 320), for example, refer to "past theory-driven descriptive and explanatory approaches that leave out messy reality", "pragmatically irrelevant business school curricula", and "evidence-free management practices". In particular, the advocates of design science assume that the "scientization of our field has greatly diminished the academic respectability of prescriptive knowledge" (van Aken, 2005, p. 22). Romme (2003, p. 558) suggested that the sciences and humanities, not the application-oriented disciplines, became the "main role models" of management research. Consequently, applicable knowledge "is regarded as rather un-academic" (Romme, 2003, p. 22) and "academic interest in prescription has largely disappeared" (Romme, 2003, p. 33). In this view, business schools lost their practical relevance by giving up their ambitions to develop prescriptions and focusing instead on "description-driven research programmes" (van Aken, 2004, p. 222), much like the natural sciences. Romme also argued that the descriptive and analytical science approach "to a large extent continues to prevail in organization studies, in part because most Ph.D. training programs tend to focus on it" (2003, p. 570). This, he claimed, prompted those members of business schools who were interested in practical knowledge to leave the universities:

> In other words, design in the technical as well as managerial and social domains moved from professional schools to a growing number of sites in the economy where it was viewed as more respectable and where it could expect larger direct economic rewards. (Romme, 2003, p. 562)

As a consequence, design inquiry is seen to be largely left to "organization development professionals and management consultants" (Romme, 2003, p. 569) with the result that the existing design knowledge is fragmented and non-rigorous. Moreover, in this stream of literature, academic knowledge is described as outdated. As Jelinek et al. (2008, p. 318) wrote,

> much of our contemporary theory of organizations rests on research carried out a half-century ago or more, in a less pervasively organizational time. This would be less critical if we were not surrounded by so very many examples of failed organizations.

Even if contemporary management research had to offer design knowledge, its fragmented character would be a further barrier to relevance, according to such authors. In this respect, the advocates of design science agree with proponents of evidence-based management (see also Denyer et al., 2008):

> Possibly, instead of being indifferent, managers are confused and defensive about what they do and the results reported around them, simply overwhelmed by a plethora of conflicting advice. Perhaps they

> pragmatically decide to ignore it all, including what others may see as hard evidence? (Jelinek et al., 2008, p. 318)

Suggested solutions. The advocates of design science stipulate that, in order to solve the relevance problem, management researchers should change the self-concept of their discipline. Instead of taking the natural sciences as a role model, they should take engineering and aim to produce application-oriented knowledge. van Aken (2004, p. 230) feels "that the still pressing relevance problem can be much mitigated by complementing such description-driven research with prescription-driven research".

Based on pragmatism, the design approach suggests that we ask "'Will it work?' rather than, 'Is it valid or true?'" (Romme, 2003, p. 558). Thereby, in order to achieve practically relevant recommendations for organizational design, it is not enough to derive recommendations from the study of existing solutions; it is necessary to create phenomena or artifacts and then evaluate them (van Aken, 2004; Holmström, Ketokivi, & Hameri, 2009). The aim is to create design knowledge, defined as knowledge that can be used in designing new solutions to problems.

The proponents of design science furthermore stress that design knowledge generated in that way is not meant for the layperson (nor for the scholar engaged in explanatory research) but for professionals such as an engineer or an "organizational designer" (van Aken, 2004). The knowledge repertoire of a professional contains object knowledge (that is, knowledge on the settings and properties of the artifacts or interventions to be designed), realization knowledge (that is, knowledge on manufacturing technologies), and a design repertoire consisting of explicit process knowledge (that is, knowledge on how to tackle the actual design process) (van Aken, 2004, 2005). The elements of this knowledge are "technological rules" that are defined as chunks "of general knowledge, linking an intervention or artefact with a desired outcome or performance in a certain field of application" (van Aken, 2004, p. 228). Like therapies in medicine, these rules should be constructed according to the following scheme: "if you want to achieve Y in situation Z, then perform action X" or "if you want to achieve Y in situation Z, then perform something like action X" (van Aken, 2005, p. 23; see also van Aken, 2004; Argyris et al., 1985; Romme, 2003, p. 568).

Design is not meant to substitute traditional scientific knowledge but to complement it (Jelinek et al., 2008; Romme, 2003). The "law-like relationships" (van Aken, 2004, p. 228) of scientific knowledge should help test designs systematically:

> For instance, one can design an aeroplane wing on the basis of tested, technological (black box) rules, but such wings can be designed much more efficiently on the basis of tested and grounded technological

rules, grounded on the laws and insights of aerodynamics and mechanics.

Critique in the existing literature. As a relatively new stream in the relevance debate, the design approach has not received much criticism so far. Pandza and Thorpe (2010) are among the few who criticize the design approach for misinterpreting Simon's legacy (1988, p. 173): "For Simon, everything that is not a natural science is a science of the artificial; he never actually divided social science into explanatory- and prescriptive-driven sciences!" Simon's notion of design addresses the socially constructed nature of society, not the possibility of "efficient societal engineering" (Pandza & Thorpe, 2010, p. 173). The same authors point to a basic difference between engineering design and management design that has so far been neglected by the proponents of management design: management designs encompass human beings, among them the designers. This means that "designers are part of the tested artefact" (Pandza & Thorpe, 2010, p. 179), which makes testing management designs a highly uncertain process. Pandza and Thorpe (2010) are therefore skeptical about the possibility of technological rules or other deterministic designs in management. In line with that, some proponents of design science recently suggested that we discard the term "technological rule" because it suggests a rather "mechanistic, precise" instruction and that we speak of "design proposition" instead (Denyer et al., 2008, p. 395).

Apart from this specific critique of design science, there is a more general and much older critique of using engineering as a model in management, a suggestion that has been traced back to the works of Frederick Taylor (Romme, 2003, p. 564). This idea has been repeatedly criticized for underestimating the fundamental differences between application-oriented social sciences and design science. "Analogies with […] engineering are sometimes drawn but they are rarely pursued with any zeal and sophistication", as Terry (1977, cited in Whitley, 1984a, p. 371) remarked.

Evidence-Based Management

Evidence-based management is the most recent stream of thought in this field and has witnessed an "explosive growth of interest" (Tourish, 2013, p. 174) in the last decade. The proponents of the argumentation that characterizes this stream of literature suggest that the concept of evidence-based medicine should be transferred to the area of management. "Evidence-based management means translating principles based on best evidence into organizational practices" (Rousseau, 2006, p. 256). Pfeffer and Sutton (2006a, 2006b, 2013) and Rousseau (2006) are prominent advocates of evidence-based management. There are, however, significant differences between these authors (Frank &

Kieser, 2013, p. 168). While the approach represented by Pfeffer and Sutton (2006a, 2006b, 2013) is mainly concerned with the "knowing–doing gap" (Pfeffer & Sutton, 1999) within organizations—that is, the fact that organizations do not use systematically all the knowledge that they possess—only Rousseau's approach (Rousseau, 2006, 2007) focuses on the "research–practice gap" (Rousseau, 2006, p. 256) and thus directly addresses the relevance of scholarly management research. The goal is to turn management studies into a discipline that is as relevant to those who practice management as medical science is to those who apply the respective findings (Rousseau, 2006, 2012; Rousseau et al., 2008).

Given the political acceptance of evidence-based approaches, Hodgkinson and Starkey (2011) expect that the argument for evidence-based management will gain even greater importance in the future. In their view, evidence-based management "is likely to become more central as public funding bodies are tasked to justify their investments and management research wrestles with the challenge of impact" (Hodgkinson & Starkey, 2011, p. 359).

Identified reasons for the lack of relevance. The causes of the lack of relevance that advocates of evidence-based management identify are similar to those identified in other streams of thought. In this body of literature, authors bemoan, for example, the fact that "academics prefer to ask their own questions (in their own time) [and] a general absence of healthy connections between organizational science and practice" (Rousseau, 2007, p. 1037). However, the advocates of evidence-based management also highlight some other important factors underlying the lack of practical relevance. In contrast to most contributors to the relevance debate, the proponents of evidence-based management do not shrink from blaming, at least partly, managers for the relevance problem, arguing that managers "rely largely on personal experience to the exclusion of more systematic knowledge [or] follow bad advice from business books or consultants based on weak evidence" (Rousseau, 2006, p. 257).

According to Rousseau and Boudreau (2011), current management education does not help overcome this problem and is thus responsible for the relevance gap. Education and research are seen as largely decoupled: for example, case studies "are used as a teaching method frequently without any grounding of their analysis in findings supported by management research" (Rousseau & Boudreau, 2011, p. 272). There are a number of reasons why this is the case. On the one hand, research-based content in management education is regarded as "fun squishing" (Rousseau & Boudreau, 2011, p. 272); on the other hand, "even for textbook writers, it is difficult to adequately synthesize the literature as long as experts in a particular area have not assembled systematic reviews. Little wonder [that] many management textbooks rely heavily upon popular trade books as sources" (Rousseau & McCarthy, 2007, p. 91). Moreover, shortcomings in management education create additional barriers to relevance. "The absence of a critical mass of evidence-based managers today translates into both poorer outcomes for organizations and into pressures to conform to more ad hoc approaches" (Rousseau & McCarthy, 2007, p. 99).

A further major reason for the relevance problem that the advocates of evidence-based management have identified is the peer-review system that values the production of new knowledge more than the synthesis of existing research. Since at "present the incentives overwhelmingly encourage scholars to diverge, [they] aren't particularly experienced in consensus building" (Rousseau, 2007, p. 1040). This makes it "just really difficult to figure out what the body of evidence says, even on many well-researched topics" (Rousseau, 2007, p. 1040). Thus, in this view a main barrier to relevance is that managers are unable to cope with the complex and fragmented body of scholarly knowledge.

Suggested solutions. Management, as proponents of this stream (e.g. Rousseau, 2006) envisage it, should follow the example of evidence-based medicine, in order to overcome the relevance problem. Management decisions should be grounded in the "best available scientific evidence" (Rousseau, 2006, p. 256). Since this approach "fundamentally is something performed by practitioners, not scholars" (Briner et al., 2009, p. 19), some solutions of the evidence-based approach aim at educating managers. The core principles of evidence-based knowledge creation should be applied to education programs and managers should be encouraged to "embrace scientific evidence" (Charlier, Brown, & Rynes, 2011, p. 223). For example, management educators should teach the principles of cause–effect relationships, the use of evidence-based aids to decision-making (e.g. checklists or flowcharts) and of the techniques that managers can apply to update their knowledge after they graduate (Rousseau & McCarthy, 2007).

Following the example of medicine, organizations should foster a "culture of evidence-based decision making" that relies on "information-sharing" and on systems that include "checklists, protocols, or standing orders" (Rousseau, 2006, pp. 259–260), all of which support decision-making. Broader institutional support from dedicated networks or centers is necessary to "bring together researchers, educators, and practitioners to identify critical practice questions, ascertain what scientific research tells us about each question, and then deliver this information in ways that support this use" (Rousseau, 2007, p. 1038).

The proponents of evidence-based management do not focus on the relevance of individual studies but on the accumulated body of knowledge on a specific topic. "Evidence implies that there is more than one source, one method, one best study, or one approach leading to an empirical relationship" (Frese, Bausch, Schmidt, Rauch, & Kabst, 2012, p. 48). Systematic reviews are "a key tool in developing the evidence base" (Tranfield, Denyer, & Smart, 2003,

p. 209) and, therefore, a precondition for practically relevant knowledge: "Evidence synthesis is a critical first step in priming the pump so that accumulated knowledge is made available for interpretation and use" (Rousseau et al., 2008, p. 507). The challenge is "to develop and implement a readily accessible (online with user-friendly language, illustrations, pictures, and interactive capabilities), up-to-date source of the best available evidence suitably formulated for its particular end users (managers, consultants, trainers, educators, students, researchers, etc.)" (Rousseau, 2007, p. 1038). To reach this goal, "methodologies for systematically integrating research evidence and developing and capitalizing its application potential" (Denyer et al., 2008, p. 408) are needed. These can be meta-analyses, narrative synthesis, a meta-narrative approach, or income – outcome analyses (Denyer et al., 2008).

Critique of the existing literature. Various authors have raised serious doubts as to whether the concept of evidence-based medicine can be fruitfully transferred to the field of management studies. One main concern relates to the type of evidence that is available in management studies. These critics argue that management research cannot generate evidence of comparable quality to that on which evidence-based medicine relies (Barends, ten Have, & Huisman, 2012; Hewison, 2004; Hodgkinson, 2011; Reay, Whitney, & Kohn, 2009; Tourish, 2013). More specifically, they point out that it is almost impossible to employ the "gold standard" (Timmermans & Berg, 2003) of clinical studies—the randomized controlled trial—to test various remedies for management problems. As Axelsson (1998, p. 13) observes, "empirical evidence on which management can be based will have a different character compared with the evidence of medical research. Randomized controlled trials are not possible in the same way in management research."

In line with this observation, Reay et al. (2009, p. 211) reported that in their review of studies that subscribe to the concept of evidence-based management, they could "not find any articles [. . .] that could be classified as generating Level 1 evidence", defined as evidence based on randomized controlled trials. Randomized controlled trials require that the treatment groups and the control groups are identical except for the treatment. If these groups differ with regard to other factors, it is not possible to draw clear-cut conclusions about the treatment. In medicine, setting up randomized controlled trials often involves using a unidimensional and fairly reliable outcome measure, such as death rate, to test a specific treatment, such as the application of a drug. Of course, the individuals in the treatment and in the control groups are never entirely identical with regard to characteristics that could influence the drug's effect. Nevertheless, according to Pawson (2002, p. 168), in medicine heterogeneity does not cause problems of the same magnitude as in management, "where interventions work through reasoning subjects rather than blinded patients".

Commenting on the quality of evidence that is usually available in management studies, Tourish (2013, p. 175) has pointed out that research results typically leave a lot of leeway as to what empirical studies are considered relevant to a particular management problem and how the results are interpreted. To illustrate this argument, he gave the example of two surveys on the motivational effects of pay-for-performance: using evidence gathered through one of these surveys, Pfeffer (1998) came to the conclusion that pay-for-performance does not motivate individuals to improve their job performance, whereas Rynes, Gerhart, and Parks (2005), who used a different survey, came to the opposite conclusion.

The plurality of paradigms that characterizes management studies is a further point that critics of evidence-based management emphasize. Learmonth and Harding (2006, p. 248) argued that the "claim that clinical practice should be evidence-based only makes sense given the existence of a more or less unified paradigm", while, according to Burrell (1996, p. 394), "the normal state of organizational science is pluralistic". These critics argue that it is unrealistic to expect that a large number of legitimate but competing theoretical perspectives will converge sufficiently to allow practitioners to use the existing evidence as a basis for their decisions. Against this background, Learmonth (2008, p. 286) has interpreted the rise of evidence-based management as a "form of resistance" to the multitude of different approaches (particularly interpretive approaches) that characterize management research.

Evidence-based management has also been criticized for neglecting the political dimension of management. For example, Hodgkinson (2012, p. 404) has pointed out that

> evidence-based management is an inherently political project, which risks creating an illusion of rationality, a multilayered façade masking underlying fundamental differences of interpretation, purpose, and power among the various stakeholders situated on both sides of the academic-practitioner/policy divide.

In that respect, Hodgkinson disagrees with Briner and Rousseau (2011, p. 19), who maintain that evidence-based management offers managers "the possibility of making clearer distinctions among politics, values, interests, and other forms of information such as research evidence" and thus promotes rational decision-making. According to Hodgkinson (2012, p. 410), "it is highly unlikely that organizational decision makers are going to willingly embrace practices that render their differing vested interests explicit". In a similar vein, Arndt and Bigelow (2007) have argued that people will try to influence how a decision problem is framed, as this determines whose expertise will be required and hence who will have a say in the decision-making process. With regard to academic actors, Hodgkinson (2012) agrees with Morrell (2008,

p. 616) that evidence-based management is a political project driven forward by

> a constellation of specialists, research institutes, funding bodies and political organizations with a common world view, who are powerful because of their shared belief that a particular form of knowledge production is applicable to social problems.

Another, more general point that critics of evidence-based management make concerns the fundamental differences between management studies and the natural sciences with respect to causality and evidence. As Tourish (2013, p. 183) wrote, organization "ultimately, emerges as a phenomenon that is co-produced and co-reproduced (within certain limits) by the discursive interactions between organizational actors". According to this view, organizational processes have to be treated as chains of human activities that are mediated by artifacts such as routines for performance evaluation, computer-supported sales forecasts, or cost-accounting procedures. Through analysis, it is possible to narrow down the causes of organizational problems in such chains of activities. However, as Tourish argues, other than in medicine, organizational malfunctions cannot be interpreted as pathological conditions that can be healed through specific types of therapy. In organizations, causality is not determined by natural laws. Thus, in order to solve organizational problems, it is necessary to change behaviors and routines. Based on such an interpretation of causality, Tourish (2013) suggests that the concept of evidence-based management should be replaced with the notion of "evidence oriented organizing"; by that he means that problems should be identified and solved through discourse rather than tackled with empirically supported "remedies". He suggests that it is possible to gain insights into the causes of specific problems and find ways of solving them by acknowledging

> that all organizational actors have a stake in determining what are important research topics, rather than just managers, [therefore,] research on these topics has to encompass a plurality of voices and accord them due weight in accounts of organizational phenomena, as well as in identifying courses of action that may seem to be suggested by research investigations. (Tourish, 2013, p. 183)

Finally, Reay et al. (2009, p. 13) have criticized the advocates of evidence-based management for maintaining that decisions should be based on evidence without examining first whether adopting the principle of evidence-based management improves management practice. As they pointed out, none of the works they had reviewed considered the question of whether there was adequate evidence that evidence-based management would improve organizational performance, "which is a startling finding since this question is of utmost importance" (Reay et al., 2009, p. 13). In fact, Reay et al. (2009,

p. 13) came to a paradoxical conclusion: if managers take evidence-based management seriously and apply the principle of "'evidence before action' (a golden rule in the medical research literature)", then they should not adopt evidence-based management.

Miscellaneous Contributions

In addition to the six streams of literature described earlier, there are also individual contributions to the relevance debate that cannot be easily categorized. These contributions have approached the relevance issue from a number of different perspectives that we will briefly describe in the following.

Research and practice treated as different "worlds". Some works on the relevance debate argue that researchers and managers live in separate "worlds", whose differences are not sufficiently acknowledged. According to Ferguson (1966, p. 3), management research has "contributed so little to the practices of management [because of] the lack of sustained understanding and involvement of managers and scientists in each other's worlds, which are so different in outlook, values, and practices". This argument has parallels in the more recent debate on the differences between the "cultures" (Choudhury, 1986, p. 23) or "frames of reference" (Shrivastava & Mitroff, 1984, p. 19) of managers and those of researchers. As Churchman, (1964, p. 38) put it, "we scientists must understand the world of the manager: not a piece of it, but the whole world".

Wrong method or theories. Occasionally, the cause of the relevance problem is seen to lie in the methodologies used in management studies. Interestingly, some authors call for more qualitative methods (e.g. Bettis, 1991; Numagami, 1998; Wolf & Rosenberg, 2012), while others for expanding the use of quantitative data and more sophisticated statistical methods (e.g. Donaldson, 1992). Some contributors to the relevance debate argue that it is the dominant theories or paradigms in management studies that constitute a barrier to relevance. Donaldson, for example, suspects that we might be "damned by our own theories" (Donaldson, 2002, p. 96) and suggests that the assumptions on which the theories of management studies rest conflict with the aim of improving managerial practices. Drawing especially on the works of Heidegger (1996) and Bourdieu (1988, 1990), several scholars (Sandberg & Tsoukas, 2011; Splitter & Seidl, 2011, in press; Weick, 2001) have argued that the relevance gap results partly from the adoption of traditional scientific approaches that analytically decompose the world in a way that is at odds with the world as practitioners experience it. This prevents researchers from comprehending management practice because it is disconnected from the "meaningful totality into which practitioners are immersed" (Sandberg &

Tsoukas, 2011, p. 341). These scholars argue that, in order to increase the relevance of management theory, it is crucial for researchers to adopt frameworks based on "practical rationality" rather than on the more traditional "scientific rationality" (Sandberg & Tsoukas, 2011) and to "shift from entities as the point of departure to entwinement" (Sandberg & Tsoukas, 2011, p. 340).

The peer-review system is conservative and slow. Several authors perceive the existing peer-review system as the underlying reason for the relevance problem, arguing that this system tends to reinforce conventional modes of research and prevent necessary changes in research strategies. According to Daft and Lewin (1990, p. 1), "normal science" has become a "straitjacket". Dunbar and Bresser (2014, p. 141) elaborate on this explanation:

> At the moment, the academic publication decision making process is largely opaque, inaccessible, and hidden from view. [. . .] Since at least some reviewers exercise the power of their role to dictate to authors what they must do to get published, authors may fear reviewers and be reluctant to offer new ideas that may be seen as challenges.

The authors consequently call for greater transparency in the review process, which would encourage more innovative and relevant research (Dunbar & Bresser, 2014). A further problem with the peer-review system is that it is too time-consuming. As McKelvey (2006, p. 826) writes, practitioners "need immediate help; they can't wait for scientists' lengthy conception-to-publication time cycle". According to this view, the slow process of scientific reasoning makes it difficult to grasp the rapidly changing phenomena of contemporary organizations (Thomas & Tymon, 1982). To remedy the problem, the review-based system should become faster so that researchers can respond more rapidly to the current needs of practitioners. Expanding on that suggestion, Corley and Gioia (2011) argue that, additionally, journals should encourage more foresight in research. They claim that management research is a "closed industry" (Corley & Gioia, 2011, p. 20) that so far has failed to anticipate societal and organizational concerns. Top-tier management journals, they argue, should require of their authors to show "prescience", which "involves anticipating and influencing the type of managerial knowledge needed to deal with coming societal and organizational concerns" (Corley & Gioia, 2011, p. 23).

Pressures for immediate relevance. Some authors also argue that the very pressures for relevance create irrelevance (e.g. Daft & Buenger, 1990; Kieser & Nicolai, 2005; March & Sutton, 1997). These authors are critical of the fact that relevant research is equated with the empirical analysis of the impact of interventions on performance (e.g. Schendel, 1995), an approach that resonates with practitioners' ideas of "success factors" (Kieser & Nicolai, 2005). Hence, in the interests of achieving relevance, some scholarly journals reward studies that use performance as a dependent variable (Kieser & Nicolai, 2005; March & Sutton, 1997). At the same time, it is doubtful whether these kinds of studies are suited to solving the relevance problem given their resemblance to an "ill-considered medieval hunt for the philosopher's stone" (Ghemawat, 1991, p. 11). While the "questionable status of studies in which organizational performance appears as a dependent variable is not a secret" (March & Sutton, 1997, p. 702), journals nevertheless value this type of research. In this way the best intentions to increase relevance can produce the opposite effect. As Daft and Buenger (1990, p. 96) contend, "the attempt to provide useful information to managers and to use performance as an outcome variable actually prevents the field of strategic management from providing useful information to managers".

Critical management studies. CMS scholars have a very particular perspective on the issue of relevance. They criticize the mainstream views in the relevance debate for their narrow notion of practical relevance, which focuses almost exclusively on managers. They call for a broader concept of social usefulness that encompasses the plurality of organizational stakeholders:

> It is high time to raise the sights of business schools beyond a myopic notion of "relevance" fixated upon a narrow range of topics and perspectives that are considered important to (existing or aspirant) executives, or at least pose no threat to their worldview, to business school benefactors or to the students who aspire to become tomorrow's business leaders. (Willmott, 2012, p. 603)

While the argument for a broader concept of relevance is shared by other contributors to the relevance debate (e.g. Hodgkinson & Starkey, 2011), the CMS debate on relevance is distinctive in that it questions whether management research should be at all "engaged", in the sense of being practically relevant. Fournier and Grey are concerned that "engaged" management research risks being colonized by the practicing managers and hence losing its critical stance. They point to an "irreconcilable tension" (Fournier & Grey, 2000, p. 27) between, on the one hand, "championing the cause of the oppressed at the risk of further contributing to their domination by having our critique appropriated and translated into 'performative knowledge'" (Fournier & Grey, 2000, pp. 26–27) and, on the other hand, to "keep our critique to ourselves and simply relish in the aesthetic pleasure that writing critically may provide us with (or suffer in silence at our inability to make a difference)" (Fournier & Grey, 2000, p. 27). However, this so-called anti-performative stance is not undisputed among the proponents of CMS. Spicer, Alvesson, and Kärreman (2009, p. 537), for example, countered it by explicitly propagating a view of CMS as "a profoundly performative project", and Wickert and Schaefer (2015, p. 107) criticize the lack of impact that CMS has had on practice, which as they argue, "is partly because CMS research often focuses on criticizing antagonistically, rather than engaging with managers".

Shortcomings of the Programmatic Relevance Debate

As our review indicates and various scholars have suggested (Augier & March, 2011; Bartunek & Rynes, 2014; Corley & Gioia, 2011; Wensley, 2007), the literature that reflects the programmatic relevance debate is fragmented and repetitive, often dominated by ad hoc statements, largely devoid of empirical support and strongly influenced by normative expectations. Although there are some critical voices, there is little exchange of arguments in the debate as a whole and this trend has hampered cumulative progress. In the following, we highlight some of the general shortcomings of the programmatic debate, which relate to some degree to all streams of thought. These concern (1) the unclear relationship between rigor and relevance, (2) the lack of explanations for the failure of suggested solutions, (3) the lack of exemplars, and (4) the problematic use of engineering and medicine as models for management studies.

Unclear relationship between rigor and relevance. The overwhelming majority of authors in the relevance debate argue that rigor and relevance should become aligned (e.g. Gulati, 2007). In order to stress this point, some use programmatic labels, such as "twin imperatives" (Hodgkinson, Herriot, & Anderson, 2001), a "dual approach to knowledge production" (Tranfield & Starkey, 1998, p. 349), or "Pasteur's quadrant" (Tushman et al., 2007, citing Stokes, 1997). This demand is not new. Throughout the history of management research, there has been a general consensus that rigor and relevance should be combined. Even the reports of Carnegie (Pierson, 1959a) and Ford (Gordon & Howell, 1959a), which are often misrepresented as calling for rigor *instead* of relevance, aimed to combine these two dimensions. While both reports emphasized the need for more rigorous scholarly research and a greater degree of academic respectability, they did not intend to devalue relevance. Gordon and Howell (1959b, p. 116), for example, worried also about practical relevance which is evident in their observation that the "gap between what society needs and what the business schools are offering has grown wide enough for all to see". Rigorous scholarly research was regarded as the key to closing this gap. By shifting attention from "best practices" and rules of thumb to rigor, this new approach was meant to help develop broader analytical competences and thus enable future managers to cope with increasingly complex business problems (Gordon & Howell, 1959b, p. 116; similarly Pierson, 1959b, p. 118). Rigorous research was meant to provide the "fundamental analytical tools" (Bach, 1958, p. 351) needed to solve the problems that plagued the economy following World War II: "At that time most business academics believed these problems were largely technical and that the application of rigorous scientific methods would lead towards their solution" (Khurana & Spender, 2012, p. 621).

It seems that the focus on how the relationship between management research and practice *should* be has resulted in some ambiguities in the perception of how this relationship actually *is*. On the one hand, most authors explicitly state that rigor and relevance *can* be combined, even if this requires particular effort (Nicolai, 2004). For example, Bartunek and Rynes conclude their review of the relevance literature by pointing out that the majority "of the special issues and special forums have emphasized the compatibility of academia and practice" (Bartunek & Rynes, 2014, p. 1190). On the other hand, authors who write on relevance often use metaphors like "pendulum swings", which seem to imply a trade-off between the two dimensions. Simon compared the attempt to combine rigor and relevance to an attempt to mix oil with water: "It is easy to describe the intended product, less easy to produce it. And the task is not finished when the goal has been achieved. Left to themselves, the oil and water will separate again" (Simon, 1976, p. 338). Frequently, a rhetoric of balance or integration is used to marginalize the forces that might drive the two dimensions apart. "The 'balance' and 'integration' problems were reduced or 'solved' by [being declared] potentially nonexistent" (Augier & March, 2007, p. 131). Consequently, little is known about these forces and some important questions on the relevance debate remain open: for example, does the focus on "real-life problems" or on the "workability criterion" create sufficient common ground for practitioners and researchers on which the two dimensions can be aligned? In order to assess any proposed solutions to the relevance problem, questions such as this need to be answered first. This has yet to be accomplished. One of the few most recent attempts to do so is Bartunek and Rynes's analysis of possible "academic–practitioner tensions as significant phenomena whose exploration can suggest important knowledge" (Bartunek & Rynes, 2014, p. 1182).

Lack of explanations for the failure of suggested solutions. The programmatic literature often reiterates existing suggestions on how to overcome the relevance problem but rarely discusses why these suggestions have not resulted in the envisaged outcomes. The editorial policies of highly regarded management journals, which require authors to address both academics and practitioners (Bartunek & Rynes, 2010, p. 102; Nicolai & Seidl, 2010) and to provide "implications for practice" (Kieser & Leiner, 2009, p. 522), are an example of the many ineffective attempts to combine rigor and relevance. *Organization Science*, for instance, was founded in order to "play a role in enhancing research relevance" (Daft & Lewin, 1990, p. 2), "encourage the joining of theory to practice" (Daft & Lewin, 1990, p. 9), and "seek to affect practice" (Daft & Lewin, 1990, p. 3). Eighteen years after that journal was launched, the editors found that it rarely reached practitioners and were forced to admit that "direct practical relevance was a naïve aspiration for OS" (Daft & Lewin, 2008, p. 181). These experiences, however, did not lead to any in-depth analysis of why previous efforts to address both practitioners and academics had failed. Instead, the reaction of many journals was generally a strategy of "more of the same". For example, many journals started pressing authors even more than before to derive explicit implications for practice. According to Bartunek and Rynes (2010), the number of articles that contained a section on the implications of the presented research for practice increased dramatically from 32% in the 1990s to 58% in the 2000s. At the same time, there is no evidence that this change lessened the problem of relevance. Indeed, some authors claim that the gulf between theory and practice is actually widening (Hodgkinson et al., 2001; Rynes et al., 2001).

Another point is that the many repeated calls for more bridging media did not take the failures of earlier efforts into consideration. For example, the *Academy of Management Executive*, a journal founded very much in the spirit of popularization ("our vanguard effort to bridge theory and practice", cited in Hambrick, 1994, p. 13), was forced to reconsider its editorial policies and was then re-launched as the *Academy of Management Perspectives* because in spite of "considerable effort and ingenuity, the goal of reaching an audience of practicing managers [had] been difficult for AME to achieve" (Academy of Management Perspectives, 2009). However, the at best mixed results of previous endeavors to simplify academic research and to translate it for and thus transfer it to practice were hardly discussed. Instead, new "transfer journals" that invoked the old bridging metaphor began to be launched (Bartunek, 2011b, p. 557).

In our review of the literature, we too found hardly any analysis of the outcomes of the proposed measures for increasing relevance. Nevertheless, for any progress to be made, it seems necessary that the relevance debate examines critically why previous attempts to solve the relevance problem have had such limited success: simply dealing with the relevance problem in a repetitive and ritualistic fashion means that there is little prospect of change.

Lack of exemplars. Given that most of the authors of the programmatic literature acknowledge the difficulties that aligning rigor and relevance entails but see no fundamental problems in the endeavor, it is somewhat surprising that they rarely provide examples of cases where management research has been successfully applied that could serve as models for further research. The very few examples that the programmatic literature does provide—such as the AACSB (2008) report on the impact of research—appear inadequate: first, many of these examples (e.g. Bansal et al., 2012, p. 75; Huff & Huff, 2001, p. S50; Van de Ven & Johnson, 2006, p. 811) do not deal with knowledge per se. As discussed in the sections on the institutional view and Mode 2, many authors refer merely to university–industry partnerships, collaborative consulting projects, or professors engaged in consulting. The role of scholarly research in these partnerships and projects is not revealed. Second, the examples that explicitly refer to knowledge often do not take into account the chief characteristic of contemporary published science; that is, the peer-reviewed journal article. For example, many authors (e.g. van Aken, 2005, p. 21) refer to old classics such as the works of Fayol, Hertzberg, or McGregor, which, despite their merits, cannot be regarded as rigorous research. Others (e.g. Starkey & Madan, 2001, p. S12) refer to sections of the popular management literature such as Peters and Waterman's *In search of excellence* (1982), which are not based on rigorous research, as one of the authors himself readily admited (Webber & Peters, 2001). Third, in the very few explicit references to scholarly knowledge, the immediate practical applications are often not derived from that knowledge but from other (non-scientific) sources of knowledge. Nicolai (2004) calls this phenomenon "applied science fiction". Michael Porter's early work is an example of this. At first glance, Porter's book (1980) on competitive strategy seems to represent a successful translation of the "structure–conduct–performance" paradigm of industrial economics into practical implications for strategic management. However, on closer inspection it becomes clear that the book's practical implications for strategy are not derived from economic reasoning (Nicolai, 2004). As Langlois (2000, p. 7) has shown, they come "from the eclectic outer core [...] rather than from anything flowing directly from the logic of neoclassical industrial organization". Similarly, the concept of core competence (Prahalad & Hamel, 1990) is often falsely (see Barney & Arikan, 2001, p. 176) presented as an example of the popularization of the resource-based view (e.g. Starkey & Madan, 2001, p. S13). Fourth, the examples that do refer to scholarly knowledge that has affected the practical world are largely based on self-fulfilling prophecies such as the Black–Scholes formula (see the subsection on the performativity of theory). To our knowledge, there are no sufficiently documented cases of straightforward practical application of management knowledge derived directly from research.

Engineering and medicine as problematic models. The programmatic relevance debate relies heavily on engineering and medicine as—often idealized—models from which management research is expected to learn. This applies especially to design science (which primarily draws on engineering) and evidence-based management (which primarily draws on medicine). Treating engineering and medicine as models has a long tradition in management studies and pervades the whole debate on programmatic relevance (e.g. Bennis & O'Toole, 2005; Grayson, 1973; Haire, 1964). Thompson (1956), for example, noted in the editorial of the inaugural issue of *Administrative Science Quarterly*: "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, p. 103). Similarly, the Pierson Report (1959a) regarded the "more fully developed professions" (Pierson, 1959a, p. 27) of medicine and engineering as primary models, although it was noted that the elements of these professions should not be applied "mechanically" (Pierson, 1959a, p. 32) to business education.

Like many authors in management studies, authors in the fields of medicine and engineering emphasize that their work is applied research (Whitley, 1988). In these two disciplines, scientific progress is primarily measured in terms of the ability of research to improve the efficacy of solutions to technical or health problems. The usefulness of research is tested on the basis of the usefulness of its outputs, such as new therapies, new surgical technologies, new energy-saving engines, new design principles for more robust structures, new software, and so on. Given that medicine and engineering are generally perceived as successful applied sciences, it seems natural that management researchers try to learn from the practices of these disciplines. However, the authors who draw parallels with medicine and engineering rarely consider the fundamental differences resulting from the fact that these disciplines are based on the natural sciences, while management studies has its roots in the social sciences. With regard to the issue of practical relevance, these differences are of particular importance, as we will elaborate in the following.

One fundamental question is whether management research can derive generalizations that take the form of laws such as those on which engineering and medicine rely. Whitley (1988, p. 64; see also Tsoukas, 1994) argued that the highly contextual nature of management limits the possibilities of discovering generalized rules: "attempts to establish a general 'science of managing' which would generate knowledge of highly general relations between limited and standard properties of separate, standard objects are doomed to failure since 'managing' is not a standardized activity". For example, management problems cannot be categorized in the same way as medical diseases (Learmonth, 2006, 2008; Learmonth & Harding, 2006; Tourish, 2013).

Another point is that theories and best practices in medicine and engineering usually change because knowledge progresses, not because of changes in the human body or in nature. For example, how the human body works is assumed to be relatively stable even if new research findings revolutionize the theories about it. Similarly, in order to derive valid law-like generalizations in management studies, the knowledge and beliefs of social agents would have to remain more or less stable and managers would have to behave as rule followers with little room for choice. However, the idea of relatively stable beliefs is particularly problematic in the context of social science that is strongly oriented to application and applicability and aims to change social behavior (see also the discussion on double hermeneutics later in the text). Mintzberg and Quinn (1996, p. xii) illustrate this point: "Imagine an engineering student's hand shooting up in physics class: 'Listen, prof, it's fine to tell us how the atom does work. But what we really want to know is how the atom should work.'"

It should also be noted that in the social sciences there is a certain tension between generalizations that are true and generalizations that are useful. This is particularly evident in economics-based theories of management. On the one hand, many of these theories apply Muth's theory of rational expectations, according to which managers act *as if* they were familiar with the current state-of-the-art scholarly knowledge, which includes, of course, the economics-based management theory itself (Muth, 1961). This suggests that these management theories cannot make a difference to the practitioner without violating their own assumptions. On the other hand, theories that acknowledge the possibility of changes in behavior tend to invalidate their own law-like generalizations. This applies, for example, to the "theories" that claim to have discovered a relationship between a positive market share and return on investment (ROI) (Wensley, 1982). Indeed, many companies promptly proceeded to base their strategies on the discovered "law". However, as a growing number of companies competed for a market share, the cost of gaining a greater market share increased. This lowered the ROI, and the original relationship between market share and ROI disappeared. Commenting on "regularities" of this type in management studies, Numagami (1998, p. 8) observed that at some point "the regularity will disappear, partly because other managers may change their state of knowledge through learning as they learn by observing the strategic change of the knowledgeable managers". Therefore, in contrast to medicine and engineering, it may not be feasible for management studies to discover, let alone rely on, relatively invariant laws (Numagami, 1998; Tsoukas, 1994). Finally, the methods used by medicine and engineering are hardly transferrable to the area of management. Instruments and laboratories for testing designs for management systems are not available. There is nothing comparable to wind tunnels, flight-test methods, or strength-testing machines to test alternative designs for management systems (Pandza & Thorpe, 2010).

In sum, given the fundamental differences between management studies on the one hand and engineering and medicine on the other, it is highly questionable whether using these two disciplines as models in order to increase the relevance of management research is a fruitful endeavor. Moreover, although medicine and engineering have served as models for decades, management studies has not come any closer to these disciplines with respect to the relationship between theory and practice. Far from advancing the relevance debate, the explicit or implicit comparison to these two disciplines has obscured important aspects of the relevance issue. Considering that management studies is a social science, it would seem more likely that management researchers may learn more about practical relevance from other application-oriented social sciences.

The Descriptive Relevance Literature

Apart from the programmatic relevance literature, there is a large body of research that has examined the relevance of management research in a descriptive fashion. Rather than focusing on the development of suggestions for dealing with the issue of relevance, these works are primarily concerned with the description or assessment of the interplay between management research and its external stakeholders. We can distinguish seven streams of research that seem highly relevant but have received only little attention in the programmatic relevance debate. In the following, we will briefly review each of these different streams, describing their respective theoretical and empirical approaches and highlighting their core insights (Table 2).

Forms and Meaning of Practical Relevance

One important stream of research has focused on the concept of "practical relevance" per se, examining the different meanings and forms associated with the term. As many researchers have highlighted (Augier & March, 2007; Nicolai & Seidl, 2010; Pearce & Huang, 2012; Staw, 1995), the term "relevance" is not very well defined. Staw (1995, p. 85), for example, points out that we need to distinguish between "relevance" in terms of "practical relevance"—that is, "meaning to outside publics"—and "relevance" in terms of contributing to the "understanding of how organizations function", independently of whether this pertains to other researchers or external audiences. While Staw himself is more interested in the second meaning, the research on relevance is typically concerned with the former. Apart from that, the more narrow term "practical relevance" is not well defined either. As Augier and March (2007, p. 138) write: "The definition of [practical] relevance is ambiguous, its measurement imprecise, and its meaning complex."

The most important impulse for the exploration of the different forms of practical relevance came from public policy research (Knorr, 1977; Pelz, 1978; Rich, 1977), which suggested that we distinguish between instrumental, conceptual, and symbolic uses of academic research. Beyer and Trice (1982) and Astley and Zammuto (1992) introduced this distinction into management studies. They speak of *instrumental use* when "the findings of a research study directly influence managerial action" (Astley & Zammuto, 1992, p. 452)—that is, when managers act "on research results in specific, direct ways" (Beyer & Trice, 1982, p. 598)—of *conceptual use* "when ideas, concepts, or scientific research results influence how a practitioner conceptualizes a problem without specific, direct usage such as when a theory influences the way a

Table 2 Descriptive Literature on Practical Relevance

Stream of research	Main insights	Theoretical background	Empirical methods	Conceptual studies (examples)	Empirical studies (examples)
Forms and meaning of relevance	• There are many different forms of relevance • Management research is more likely to be of conceptual than instrumental relevance	Diverse	Journal content analysis; literature review	Astley and Zammuto (1992), Bartunek (2011a), Beyer (1997)	Bartunek and Rynes (2010), Beyer and Trice (1982), Nicolai and Seidl (2010), Pearce and Huang (2012)
Performativity of theory	• In contrast to natural sciences, theories in management studies often function as self-fulfilling or self-defeating prophecies • Aspects other than scientific rigor determine whether theories are adopted by practitioners • Management theories can also have detrimental effects on management practice	Sociology of science (Callon, Giddens, Merton)	Case study	Cabantous and Gond (2011), Ferraro, Pfeffer, and Sutton (2005a, 2005b), Ghoshal (2005)	MacKenzie and Millo (2003)

Table 2 Continued

Stream of research	Main insights	Theoretical background	Empirical methods	Conceptual studies (examples)	Empirical studies (examples)
Organizational adoption/ adaptation	• The utilization of academic concepts is not a simple transfer process but a complex organizational process • The utilization of academic knowledge ranges from partial to complete and from rhetorical to substantive utilization • The utilization of academic concepts is associated with a change in their meaning	Organization theory, systems theory, theory of organizational change	Case study, literature review, survey, experiment	Seidl (2007)	Beyer and Trice (1982), Jarzabkowski and Wilson (2006), Nicolai and Dautwiz (2010)
Role of research in the general management discourse	• The scholarly management discourse is more affected by popular management discourse than vice versa	Institutional theory, theory of fashion, diffusion theory	Bibliometric analysis, case studies, content analysis of journals, surveys	Abrahamson (1996), Kieser (1997)	Abrahamson and Eisenman (2001), Abrahamson and Fairchild (1999, 2001), Barley, Meyer, and Gash (1988), Benders and Bijsterveld (2000), Gopinath and Hoffman (1995a, 1995b), Kelemen and Bansal (2002), Marcus, Goodman, and Grazman (1995), Rynes, Colbert, and Brown (2002), Rynes et al. (2007), Schulz and Nicolai (in press)

Table 2 Continued

Stream of research	Main insights	Theoretical background	Empirical methods	Conceptual studies (examples)	Empirical studies (examples)
	• Popularization media are not used as means of communicating research results • The dissemination of management knowledge is largely dominated by fashion cycles • The degree to which management knowledge is diffused largely depends on rhetoric and is not tantamount to improvements in practice				
Logics of management science and practice	• Management science and practice operate on the basis of different logics	New systems theory, practice theory	–	Astley and Zammuto (1992), Kieser (2002), Kieser and Leiner (2009), Kieser and Nicolai (2005), Nicolai (2004), Rasche and Behnam (2009), Seidl (2007, 2009)	–

Table 2 Continued

Stream of research	Main insights	Theoretical background	Empirical methods	Conceptual studies (examples)	Empirical studies (examples)
	• The traditional view of research utilization as the direct and linear transfer of research output to management practice is inappropriate				
Characteristics of an applied academic discipline	• The practical orientation of management research requires particular distance from practitioners and particular reflexivity in the research process • The relevance debate is largely an expression of a struggle for control over business school education rather than an authentic concern for relevance • The practical orientation of management studies has opened it to influences from outside the academic domain reducing its coherence and resulting in "fragmented adhocracy"	Sociology of science; theory of professions	Historical analysis	Whitley (1984b, 1995)	Augier and March (2011), Wensley (2007), Whitley (1984a), Zell (2001)

Table 2 Continued

Stream of research	Main insights	Theoretical background	Empirical methods	Conceptual studies (examples)	Empirical studies (examples)
	• There are significant barriers to developing management into a research-based profession				
Science-based advice	• The political arena is an important domain outside science that is (potentially) affected by management research • Science-based advice is inevitably political in nature • The design of the advice-giving process determines the impact on decision-making • Science-based advice cannot and is not expected to resolve the problems that politicians (i.e. practitioners in the political arena) face	Sociology of science; political science	Case studies	Douglas (2009), Jasanoff (1994, 2004), Lengwiler (2008), Lentsch and Weingart (2011), Maasen and Weingart (2005)	Böschen, Kastenhofer, Rust, Soentgen, and Wehling (2010), Bogner and Menz (2010), Stilgoe (2005)

manager thinks about a problem" (Astley & Zammuto, 1992, p. 452) and of *symbolic use* when research results are used "to legitimate and sustain predetermined positions" (Beyer & Trice, 1982, p. 598).

Based on a review of the literature, Nicolai and Seidl (2010) developed this classification further, identifying different sub-types of relevance. They showed that instrumental relevance can be realized in the form of schemes that provide systematics for ordering decision situations, technologies, or recipes that guide the process of choosing a particular course of action, and forecasts that describe trends or predictions about future developments. Conceptual relevance, in turn, can be realized as linguistic constructs that offer concepts or metaphors, as description of contingencies that uncover new or alternative routes of action, and as descriptions of causal relationships that show that conventional wisdom is misleading or uncover hitherto unknown causal effects, including side effects (see also McGahan, 2007). Finally, symbolic relevance, or "legitimative relevance", as they termed it, can be realized in the form of credentializing; that is, offering means of legitimizing people or knowledge domains, and rhetoric devices that provide a symbolic language.

Building on the distinction between different forms of research utilization, various researchers have discussed and examined what forms of relevance are actually produced by management studies. Pointing to the different logics of management research and practice, Astley and Zammuto (1992) and Nicolai and Seidl (2010) argue that the output of research is unlikely to be of instrumental relevance. Instead, one can only expect conceptual and symbolic use. A similar view is also expressed by Beyer (1997, p. 17):

> Given the complexity and variety of organizational and management situations, it seems rather idealistic to expect that we can do research that is so generalizable that it fits all circumstances. Unless we tailor a research project to answer a specific need in a specific situation, we cannot expect instrumental use.

Analyzing articles published in leading management journals, Pearce and Huang (2012) argue that the percentage of papers offering either instrumentally or conceptually useful knowledge to managers has generally decreased over the last 50 years. Two other studies have focused particularly on the types of relevance offered in the "implications for practice" sections. In their study of 450 papers published in three top-tier North American journals, Nicolai and Seidl (2010) found that almost 50% of the implications for practitioners specified by the authors can be classified as "uncovering causal relationships" as one of three forms of conceptual relevance. About 40% of all "implications for practitioners" are offered in the form of technological rules or recipes as one of three forms of instrumental relevance. However, the authors showed that on closer inspection these recommendations were formulated very tentatively, with expressions such as "managers should consider

. . . ”. Often they were also tautological (e.g. “Managers should try to acquire customers that have the greatest potential, as long as the costs of acquiring such customers do not outweigh the benefits”) or only loosely related to the findings of the study itself. These findings concur with a study conducted by Bartunek and Rynes (2010), in which they analyzed 1738 empirical papers in five top-tier North American journals. They showed that 74% of authors used tentative language in the implications sections, offering recommendations that were mostly of a conceptual rather than instrumental nature. This is also consistent with earlier studies that have found conceptual use to be the most frequent form of use (Beyer & Trice, 1982, p. 600). Both studies come to the conclusion that offering conceptual forms of relevance seems more appropriate for research articles than instrumental forms. Bartunek and Rynes (2010, p. 110) concluded: “These considerations suggest that the goal of most IFP [implications for practice] sections in top-tier journals should probably be something other than instrumental use [. . .] and that a tentative tone is appropriate”.

Taken together, the works in this stream of research make important contributions to the relevance debate. First, they highlight the fact that practical relevance has many different dimensions, which has not been properly acknowledged in most contributions to the relevance debate. Bartunek (2011a, p. 392) noted:

> This recognition of different types of uses of scholarly research is important. Most discussions of the “relevance” of management research implicitly, if not explicitly, seem to assume that the instrumental use of research findings is identical to relevance and thus to despair when such work cannot be easily applied directly.

Second, these works have shown that different forms of relevance are associated with different challenges both to the researcher and to the practitioner. In sum, these works point to the fact that it is more realistic to expect conceptual rather than instrumental forms of relevance from academic research, particularly from research articles. As Nicolai and Seidl (2010, pp. 1277–1278) concluded: “The suggested focus on conceptual relevance implies a reorientation in the relevance debate, which so far has been dominated (implicitly or explicitly) by a model of instrumental relevance.”

The Performativity of Management Theories

A growing stream of research investigates how management theories that are meant to describe management practice come to change the very practice they describe. Scholars in this stream of research have been drawing particularly on Merton (1948) and Giddens (1984) to argue that, in contrast to the natural sciences, theories in social science in general and in management studies in particular have the potential to become “performative”, in the sense of changing their own object of research. This has also been referred to as an issue of “double hermeneutic”, which is defined as “mutual interpretive interplay between social science and those whose activities compose its subject matter [. . .]. The theories and findings of the social sciences cannot be kept wholly separate from the universe of meaning and action which they are about” (Giddens, 1984, p. xxxiii). As Merton (1948, p. 195) pointed out, this interplay can lead to self-fulfilling prophecies; that is, “in the beginning, a false definition of a situation evoking a behaviour which makes the originally false conception come true” or self-defeating prophecies; that is, an initially adequate description of a situation evoking a change in behavior and resulting in a deviation of the situation from the description.

In their widely cited study of the history of the Chicago Options Exchange, MacKenzie and Millo (2003) showed how option-pricing theory and the associated models were subsequently adopted by financial managers, resulting in trading behavior that resembled very much the behavior described by the theory. As the authors argue, “[option] pricing theory [. . .] succeeded empirically not because it discovered pre-existing price patterns but because markets changed in ways that made its assumptions more accurate and because the theory was used in arbitrage” (MacKenzie & Millo, 2003, p. 107). Similarly, Cabantous and Gond (2011, p. 547) have argued that rational-choice theory embedded in economics influenced the behavior of managers, such that “organizational actors collectively produce rational decisions and thus grant social reality to rational choice theory”. As other studies have shown, it is often also the underlying assumptions of a particular theoretical approach that are adopted by practitioners. Ghoshal and Moran (1996; see also Ghoshal, 2005), for example, argue that the unrealistic assumptions of transaction cost theory about human nature, such as the idea of innate opportunism, have been incorporated in the worldview of managers who adapt their actions to the theory, creating the very opportunistic behavior the theory predicts. Similarly, Ferraro et al. (2005a, p. 8) have provided an account of how economic theorizing more generally (and its assumptions about the self-interested nature of human beings) has influenced “how we think about ourselves and how we act” creating the very self-interested behavior that it describes.

Several authors have examined various conditions and mechanisms that allow theories to become self-fulfilling. Ferraro et al. (2005a) speak of three central mechanisms: (1) when the institutional designs and organizational arrangements reflect the theories of their designers (p. 9), (2) when theories come to be taken as describing normative ideals such that people “believe they ought to behave in [a particular] way or risk appearing foolish, gullible, or naive” (p. 14), (3) when theories or their assumptions get embedded into everyday language such that it “affects how individuals see and understand themselves and the world” (p. 9). The authors identify two conditions that increase the likelihood of this to happen: when the theoretical assumptions resonate with the particular cultural context and when there is accountability that "creates pressures on the actors to adopt legitimate behaviors, such as those normatively sanctioned by [the particular] theory" (Ferraro et al., 2005a, p. 17). Ferraro et al. (2005b) add to this further conditions, which, if considered seriously, point to the fact that this type of self-fulfilling dynamics are likely to be an exception rather than the norm. One condition is that the theory, or an aspect of it, has to be known to the actors. This condition is very restrictive, if we consider the empirical evidence showing that the results of management research are seldom known outside academic circles (see the section on the general management discourse later in the text). Another condition is that the aspects addressed in the theory have to be within the actors' decision scope and possibility. Only if one can act on the theory will the theory affect one's behavior. Building on the work of Ferraro et al. (2005a), Cabantous and Gond (2011) identify the mechanisms that, when combined, sustain the performativity of theories: (1) conventionalization, which refers to the embedding of theoretical assumptions into the thinking of managers (e.g. as part of a business school's education), (2) engineering, which refers to the embedding of theoretical assumptions into management tools and techniques, and (3) commodification, which refers to the availability of tools and services in which particular theoretical assumptions are embedded.

Taken together, the different studies in this stream of research point to several important issues regarding the relation between management studies and management practice: first, given the focus on double hermeneutics, this research generally highlights an important particularity of the social sciences and management studies in particular. In contrast to the natural sciences, social science theories can affect the social world and thereby change what they describe, thus becoming self-fulfilling or self-defeating prophecies. This obviously also raises more general questions about the ways in which theories can be assessed as true or false (Felin & Foss, 2009). Second, these studies point to the fact that aspects other than scientific rigor or scientific truth value determine whether theories are adopted by practitioners. The extent to which theoretical rigor has an influence on the adoption of a theory is still fiercely debated (Felin & Foss, 2009). Third, these studies show that "practical relevance"—that is, the impact of research output on management practice—is not necessarily positive. As Ghoshal (2005, p. 75) provocatively put it, management theories can "destroy good management practice". Fourth, all these points highlight the potential responsibilities of researchers for the effect of their theories on practice. As Ghoshal and Moran (1996, p. 39) write: "Social sciences carry a special responsibility because of the process of the double hermeneutic: Its theories affect the agents who are its subject matter."

Organizational Adoption and Adaptation of Academic Management Knowledge

A small stream of research, which started with a study by Beyer and Trice (1982), examines the way in which academic knowledge is adopted by organizations and adapted to a particular organizational context. As Beyer and Trice (1982, p. 595) pointed out: "Clearly, utilization is a complex behavioral process. Conceptual frameworks that fail to reflect this ignore some parts of the phenomenon." The authors argued that since the utilization of scientific knowledge by practitioners is associated with a change of the respective organization, we need to attend to the various organizational dynamics. According to the same authors, one consequence of this is that we should distinguish between the adoption and the implementation of scientific research results as two analytically distinct aspects of utilization that are associated with different behavioral and organizational dynamics.

Studies have shown that the utilization of research results depends on the degree to which they resonate with the assumptions of organizational members and conform to their interests (Beyer & Trice, 1982; Nicolai & Dautwiz, 2010). If this is not the case, research results are likely to be ignored or their implementation may be met with resistance. Another point existing studies make is that we need to distinguish between "rhetorical" and "substantive" utilization and combinations of the two (Nicolai & Dautwiz, 2010, pp. 883–884). There have also been calls to "discriminate between [the] partial and complete use" (Beyer & Trice, 1982, p. 616) of research output. As some researchers have argued, the adoption of research results is often limited to labels without any consideration of the content (Seidl, 2007). It has also been shown that the ambiguity of such labels increases the chances of adoption (Nicolai & Dautwiz, 2010). In line with that, Beyer and Trice speculate that a reason for the popularity of some theories is that they are "elastic enough to permit and encourage a wide variety of conceptual uses" (1982, p. 600).

Various studies examine how the content of the adopted concept is changed in the utilization process. Beyer and Trice (1982, p. 615), for example, point out that people "distort ideas derived from organizational research to pursue their own advantage and sometimes even to harm someone else". Furthermore, several authors have argued that the meaning of adopted research content inevitably changes as a result of becoming embedded in a new context (Jarzabkowski & Wilson, 2006; Rasche & Behnam, 2009; Seidl, 2007, 2009). Seidl (2007, p. 206) writes: "the transfer of a set of labels from one discourse to another is associated with a (mostly unnoticed) re-interpretation, i.e., with a change of its meaning". In the study that Nicolai and Dautwiz carried out in a large German corporation to examine the utilization of the core competence concept, a concept with arguably "rudimentary roots in the scientific discourse" (Nicolai & Dautwiz, 2010, p. 886), they found that the meaning attributed to the concept varied between different levels of usage. "On the general level of organizational usage we found [...] that the top managers simplified the core competence concept and decoupled it largely from its original meaning" (Nicolai & Dautwiz, 2010, p. 885). However, on his or her respective local level, "each manager defined his or her own set of core competences. This novel kind of 'contextual ambiguity' [originates] from the plurality of local contexts in which the concept is interpreted" (Nicolai & Dautwiz, 2010, p. 886).

Taken together, these studies point to several important issues regarding the relation between management studies and management practice: first, they highlight the fact that the utilization of research output is a complex organizational process that exhibits many characteristics that are known from studies on organizational change in general. This suggests that organization theories and theories of organizational change might fruitfully be mobilized in this kind of research. As Nicolai and Dautwiz (2010, p. 887) conclude: "Further research should bring organizational variables back in for a richer understanding of a management [concept's] intra-organizational evolution which cannot be marginalized to an adoption event." Second, these studies sensitize us to important differences in the utilization of research results, such as the substantive compared to the rhetorical and the partial compared to the complete utilization of research output. Finally, they point out that the utilization process is associated with the reinterpretation of the meaning of the concepts involved. Hence, what might initially appear as the adoption of identical concepts might just be an

> illusion based on the fact that organizations use the same labels, or sets of labels, for their own constructs. [...] In other words, different organizations might use the same labels [...], but the concrete practices behind the labels could be different. (Seidl, 2007, p. 206)

The Role of Research in the General Management Discourse

Another stream of literature has examined the impact of management research on the general management discourse. Management scholars are thereby examined as one of several groups of actors who "participate in the creation, elaboration, and marketing of new ideas and knowledge products" (Abrahamson & Fairchild, 2001, p. 148). In this stream of literature, researchers have examined the extent to which the publications of management scholars have contributed to the dissemination of research results into the practitioners' discourse. Most studies in this stream have been based on analyses of the works published in scholarly publications, practitioner publications, and semi-academic or so-called "bridging journals", which are journals "devoted to disseminating academic research to practitioners" (Gopinath & Hoffman, 1995a, p. 578).

A number of more general studies have shown that research results rarely get disseminated into the practitioner discourse. For example, in their survey of scholars and of top managers of large corporations in the U.S., Marcus et al. (1995) found that, with the exception of Michael Porter's work (1980), hardly any concepts developed in strategy research had been considered particularly important by practitioners. This is partly a result of the fact that scholarly journals and particularly bridging journals are seldom read by practitioners. As Gopinath and Hoffman (1995a, p. 589; see also Gopinath & Hoffman, 1995b) found in their survey, "CEOs simply do not read publications containing strategic management research with sufficient regularity to obtain a good sampling of the current research available." Similar results have been reported by Rynes et al. (2002). It has also been shown that many research findings that scholars themselves consider particularly important are barely covered in bridging journals. Rynes et al. (2007), for example, analyzed how research results in the area of human resource management were covered in key bridging journals (*Human Resource Management*, *HR Magazine*, and the *HBR*). They concluded that "bridge journals provide little coverage of some of the research findings deemed most important by HR researchers" (Rynes et al., 2007, p. 1004) and that when these findings are reported, the messages transmitted "are sometimes very different from the ones a reader would find in peer reviewed academic journals" (Rynes et al., 2007, p. 1004). Thus, in contrast to their own mission statements, these journals hardly ever "translate" or "transfer" important findings of research in human resources. Similar results are reported by Kelemen and Bansal (2002), who show that the way in which research results are described in practitioner-oriented journals differs considerably from the way in which they were described in the scholarly discourse. Dunbar (1983, p. 140) has shown that even in a leading bridging journal like the *HBR*, the "scientific basis for [. . .] general recommendations was not apparent".

These findings reflect those of studies on management fashions or the management knowledge industry more generally, which suggest that scholars are not particularly successful in introducing new ideas to the management knowledge market. Most popular management concepts stem from the domain of management practice or consulting (Abrahamson & Fairchild, 1999, 2001). Benders and Bijsterveld (2000, p. 62; see also Kieser, 1997, p. 70) point out that one reason for this is that the

> rhetorical tactics of fashion setters are at odds with academic criteria and methodological efforts to arrive at accurate descriptions of reality: fashion setters try to convince managers of the merits of their products, whereas academics are expected to give a balanced account of the pros and cons.

Suddaby and Greenwood (2001, p. 948) have argued that the large management consulting firms diminish the influence of management research further by creating "knowledge centres [that] mimic aspects of universities, exhibiting a dual commitment to teaching, research and dissemination of results". Moreover, through symbolic associations with prominent academics they raise "the legitimacy of each firm's knowledge product" (Suddaby & Greenwood, 2001, p. 949).

Furthermore, various studies have shown that there is typically a considerable time lag between practitioner publications and publications by scholars (Abrahamson, 1996; Abrahamson & Fairchild, 2001; Benders & Bijsterveld, 2000). For example, in their case study on the fashion life cycle of total quality management (TQM), Abrahamson and Fairchild (2001) showed that the scholarly publications on TQM came out a long time after the popular press had picked up the topic and also that it remained a topic in the scholarly discourse for a much longer time than in the popular press. Even though management scholars seem to have little influence on the development of management fashions per se, it has been argued that scholarly publications serve a symbolic function by providing legitimacy to the fashion discourse as a whole (Kieser, 1997).

While most studies have focused on the effect of the scholarly on the practitioner discourse, two studies have been concerned with the reverse influence. Barley et al. (1988) analyzed and compared the content of academic and practitioner publications on organizational culture over a decade. Their study suggests that the scholarly discourse is much more influenced by the practitioner discourse than the reverse. As they concluded, "the data suggest that conceptual and symbolic influence flowed in only one direction: from practitioners to academics" (Barley et al., 1988, p. 52). In another study, Schulz and Nicolai (in press) analyzed the role of bridging journals in the transfer of knowledge between the scholarly and the practitioner discourse. Based on a bibliometric analysis of the *HBR*, they found that rather than serving as a publication outlet for translating and transferring research findings to the practitioner domain, the journal served as a means of introducing practitioner knowledge into the scientific domain. "The citation pattern that emerges shows that the HBR transfers knowledge from the downstream [i.e., from the practitioner domain] to the upstream side [i.e., the scholarly domain]—and not vice-versa", as Schulz and Nicolai (in press) explain. According to the authors, this suggests that practitioners are less interested in the output of management research than academics are interested in the observations of practitioners.

Taken together, the different works in this stream of research make several contributions to the relevance debate: first, they highlight the fact that the relevance of research refers to the impact of research on management practice as well as on the popular management discourse. Second, they reveal the particular dynamics of the management knowledge market and the position that publications by scholars occupy in it. Acknowledging that this market is dominated by fashions leads to an appreciation of its cyclical nature, as well as of the fact that the dissemination of management concepts is largely dependent on rhetoric (fostered through the ambiguity of concepts) and that a high degree of dissemination is not tantamount to effecting changes, let alone improvements, in the practice. As Abrahamson and Eisenman (2001, p. 74) stress, studying the mechanisms of the management knowledge market

> would enable management scholars, who are interested not only in understanding the world of management but also in shaping it, to understand how they can influence the creating of a better management knowledge industry and play a more important role within it.

Third, these studies also lead to a different understanding of the influence between the scientific discourse and the practitioner discourse. In contrast to the widely held assumption of a unidirectional, linear process of knowledge transfer from management research to management practice, they point to a "reciprocal or even circular relationship" between these two discourses (Schulz & Nicolai, in press). Fourth, the various studies examining the content of bridging journals have important implications for the assessment of the popularization view described earlier. They show that existing popularization media act in contraction to their own mission statements and do not communicate scientific knowledge to practitioners. This contradicts the popular claim that the relevance problem is mainly a translation problem that could be solved by creating more diffusion channels. Rather, it supports the view that the relevance problem has to do with the content of academic research as such.

The Logics of Management Science and Management Practice

Drawing on different sociological perspectives, a number of primarily conceptual works have examined and compared the modes of operation of management science and of management practice, arguing that these two domains follow different logics. Drawing particularly on Wittgenstein's concept of language games (Wittgenstein, 2001), Astley and Zammuto (1992, p. 443) described management science and management practice as "interdependent, yet semiautonomous, domains, which engage in their own specialized forms of discourse". These two domains are characterized by different linguistic conventions or rules that determine the meaning of contributions to the respective domains. The scientific domain in particular offers specific scientific constructs that act as "interpretive frames of reference" (Astley & Zammuto, 1992, p. 445) for observing and communicating about the managerial world. As Astley (1985a, pp. 509–510) writes: "Just as organizational participants subjectively interpret events in order to experience everyday life as meaningful, so administrative scientists superimpose analytical frameworks on empirical observations to render knowledge meaningful." Thereby, the purpose of scholarly activity is not to produce "accurate" descriptions of the phenomena under study, but to develop abstract and general theories that allow meaning to be assigned to empirical observations (Astley, 1985a). As Astley and Zammuto (1992, p. 446) wrote: "Esoteric discourse itself becomes the essential product of science—the raison d'etre of largely self-sustaining scientific communities [...]. Through scientific discourse we articulate our relationship to the world and fulfill our intellectual potential." In contrast to the scientific realm, "the primary function of the managerial language game is to facilitate practical action. [...] Organizations are created and sustained as managers engage their surroundings through the use of linguistic codes and conventions that define appropriate patterns of social activity" (Astley & Zammuto, 1992, p. 449).

Drawing particularly on modern systems theory (Luhmann, 1995), several, predominantly German scholars (Kieser, 2002; Kieser & Leiner, 2009; Kieser & Nicolai, 2005; Nicolai, 2004; Nicolai & Seidl, 2010; Rasche & Behnam, 2009; Seidl, 2007, 2009) have taken Astley and Zamuto's characterization of management science and practice as determined by different logics a step further. They argue that management science and practice (or better: individual organizations) constitute self-reproducing networks of communication. Whether or not a particular communicative element belongs to the academic domain or to the practitioner's domain is determined by the network of communication and, hence, by the extent to which a particular communication is linked up with other communications in that particular network. If it is not linked to other scientific communications it is not part of science. In management studies, as in many other academic disciplines, the primary communication element is the scientific publication and integration into the network of scientific communication materializes in the form of cross-references between articles (Nicolai, 2004). This means that, in order to be considered scientific, publications have to base their argument on, and be framed in terms of, earlier scientific communications. This is necessary, so that later scientific publications will consider that publication scientific and base their own arguments on it (Seidl, 2007). This network of communications "operates on the basis of the generalized symbolic medium of truth" (Kieser & Leiner, 2009, p. 521). Accordingly, the "meaning of a communication within a scientific discourse is basically its particular truth or falsehood, to which further communications can refer in order to claim truth for themselves" (Seidl, 2007, p. 202). Thereby, the criteria for "truth" and "falsehood" are themselves determined by earlier scientific communications. Analogously to the scientific system, operations in the practical world will be considered part of a particular "system of practice" (Rasche & Behnam, 2009, p. 250) only if they become integrated into the respective network. In contrast to the scientific discourse, communications in the system of practice are based on different symbolically generalized communication mediums (Kieser & Leiner, 2009, p. 522). Whether a communication is theoretically or empirically supportable, and in this sense true or false, is not of core concern. Instead, functionality—that is, whether something works or does not work—is essential in communication between practitioners.

As a result of this conceptualization of management science and practice, scholars have come to acknowledge that many of the features of the scientific publication system that have been criticized in the relevance debate are essential to the very functioning of the scientific system. For example, while management science has often been criticized for being too abstract and removed from the concrete problems of practitioners (see our review of the programmatic literature mentioned earlier), Astley and Zammuto (1992, p. 445), based on their Wittgensteinian perspective, argued that this is a crucial aspect of science:

> Though generality diminishes the capacity of theoretical language to denote the specifics of concrete experience, it enhances its capacity to convey meaningful connotations—connotations that derive from the scientist's location of research within the generalizations of a theoretical tradition. The reduction of theoretical language to highly specific, empirically descriptive terminology would destroy this source of meaning.

Similarly, systems-theoretical studies (Nicolai, 2004; Seidl, 2009) have shown that the self-referentiality of science—that is, the fact that scientific statements are developed and assessed according to the criteria of "scientific truth" that are themselves the result of earlier scientific statements—is not a deficiency of science but the very mode by which science operates. However, they point out that, other than often misunderstood, characterizing the scientific system as self-referential does not mean that the scientific system is understood as closed off from the external world. It merely means that operations outside the scientific system cannot impact the generation of scientific statement directly but will be transformed according to the concepts and criteria that the scientific system holds in stock. Thus, rather than leading to solipsism, the self-referentiality of the social system is seen as a precondition for generating and processing scientific information about the external world.

Various scholars (Nicolai, 2004; Rasche & Behnam, 2009; Seidl, 2009) have stressed that the appreciation of the fundamentally self-referential logic of management science implies a more complex understanding of the relation between management science and practice. They argue that if we acknowledge that scientific communication is only meaningful in its particular scientific context, we can no longer speak of a "transfer" of scientific results to the practice domain (Seidl, 2009). However, they stress that this does not mean that management science cannot and does not have any impact on management practice. Yet, the particular ways in which research output affects management practice and the particular way in which it is understood are ultimately determined by the system of practice itself. The practice system adopting scientific results cannot but reconstruct the meaning of the scientific results according to their own logic. The result of this is a "productive misunderstanding" (Seidl, 2007, 2009). However, the impossibility of a direct transfer of scientific results to the practice domain tends to be obscured by constructing "façades of relevance" (Kieser & Nicolai, 2005), by telling stories of "applied science fiction" (Nicolai, 2004), and by operating "as if" (Rasche & Behnam, 2009) a direct transfer were possible.

Taken together, these works make several important contributions to the relevance debate: first, the appreciation of the self-referential logic of management studies calls into question attempts to "close" or "bridge" the gap between management research and management practice. As Rasche and Behnam observe: "The demand for stable and well worked-out bridges between science and practice obscures the self-referential logic that underlies both systems" (Rasche & Behnam, 2009, p. 249). The works in this stream stress that self-referentiality is an essential feature of the scientific system that cannot be changed—unless one turns to a non-scientific mode of communication. Second, these works lead to the rejection of the traditional conceptualization of research utilization as a direct and linear process of transferring research results into practice, leading to a more sophisticated and more complex understanding of the interaction between management science and practice that acknowledges the constitutive role of the adopting system in (re)constructing the meaning of research output.

Characteristics of Management Studies as an Applied Science

A number of scholars have examined the characteristics of management studies as an applied science. Their focus is on the structure of management research as such and how it is affected by expectations about the production of practically relevant results. Based on the sociology of science, these works examine management studies as a whole, in comparison to other disciplines. In one of the first studies of its kind, Whitley (1984a) discussed the consequences of the quest for practical relevance on the kind of knowledge management studies can and does produce. He argues that management studies as a "practically oriented" social science differs from "intellectually oriented" sciences in that it aims both to explain the status quo and to improve or transcend it. Accordingly, management research has to produce descriptions of management "in which existing arrangements can be conceived as needing improvement and some conception of what constitutes 'improvement'. It therefore has to transcend current beliefs and practices rather than reproduce them in formulating its problems and intellectual goals" (Whitley, 1984a,

p. 371). This has three important implications for management research: first, the aim to improve practice implies that researchers need a high degree of autonomy and distance from management practice. As Whitley (1984a, p. 373) writes: "Accepting managers' own definitions of problems and descriptions of social realities at their face value means that the conditions which led to these accounts being proffered cannot be studied and hence alterations suggested." Second, management researchers have to make explicit value judgments with regard to "what is seen as a problem and how knowledge about it is assessed" (Whitley, 1984a, p. 373), which inevitably affects what particular research output is created. Third, research directed at improving practice "needs to have some view about its own intervention in the everyday world [and to be] more reflexive and self-conscious than other forms of social science" (Whitley, 1984a, p. 373). Whitley points out that these issues, which challenge the scientific status of research output, have not been sufficiently reflected in management studies.

In addition to that, Whitley (1984a, p. 380) points out that as a social science, particularly as a social science focused on real-life issues, management studies cannot control the boundary conditions of the phenomena under investigation in such a way that "the mechanisms and their effects can be isolated". Consequently, management studies can only identify "tendencies" (Whitley, 1984a, p. 381) rather than "constant regularities" (Whitley, 1984a, p. 380). As Whitley (1984a, pp. 380–381) put it: "To search for universal laws [in management studies] is to search for chimera; rather efforts should be focused on identifying the real causal mechanisms which function as tendencies in the production of everyday phenomena."

In addition to that, several studies have examined how the particular institutional context has affected the organization of management research. Various scholars argue that the current state of management research to a large extent is a result of the historical development of business schools as the institutional context within which management research is being conducted (Augier & March, 2007; Wensley, 2007; Whitley, 1984b, 1995). On a more general level, these studies have shown how in management research changes in the emphasis on either rigor or relevance can be explained as a result of changes in the focus of management education, which has been traditionally plagued by tensions between the "claims of experiential knowledge" and "the claims of academic knowledge" (Augier & March, 2007, p. 144) or between "scholarly knowledge" and "folk wisdom" (Wensley, 2007, p. 30). Based on historical analyses, these studies show that initially business schools were primarily focused on experiential knowledge, but that the criticism they drew for their lack of scientific rigor at the end of the 1950s (Gordon & Howell, 1959a; Pierson, 1959a) led to the hiring and promotion of more scientifically trained researchers. In the 1980s and 1990s, this resulted in the aforementioned "counter-revolution" and criticism for their lack of relevance, which can also be linked to the emergence of business school rankings in the business press (Augier & March, 2007; Zell, 2001).

The history of shifts or "swings of the pendulum" (Zell, 2001, p. 338) between different emphases of business schools in general and management research in particular has been described as a "history of rhetoric" (Augier & March, 2007, p. 144), which can be interpreted as struggles for gaining control over business school education. As Augier and March wrote, "the debate over relevance [might as well be treated] as minor rhetorical frosting for a serious political contest over [the] control of business schools and management education" (Augier & March, 2007, p. 136).

In his study, Whitley (1984b) focused particularly on the internal structures of control within the organization of management research and its effect on research output. He argued that by claiming that their aim is to generate practically useful knowledge, management researchers have conceded some of their control over the organization of management research to parties outside the academic domain and have reduced possibilities for the internal coordination of research endeavors. Whitley (1984b, p. 337) has written in this respect:

> Regardless of the actual technical usefulness of research into management […], its openness to non-intellectuals and standards set by labour markets not controlled by knowledge producers has had a major effect on the sort of reputational organization which has developed.

In particular, this had three important consequences for the development of management research (Whitley, 1984b, p. 337): first, management research has little autonomy from other areas, be they management practice or other disciplinary fields; second, there is a low concentration of control over access to critical resources; and third, there is a plurality of different audiences awarding reputational feedback, including fellow researchers as well as various groups of practitioners. Because of these particular contextual factors, management studies has developed into a "fragmented adhocracy" (Whitley, 1984b, p. 332), constituting a congeries "of overlapping yet disconnected topics, results and pronouncements with little in common except a joint institutional base" (Whitley, 1984a, p. 387).

In another study, Whitley (1995) showed that in contrast to other practically oriented sciences, such as engineering or medicine, the particular contextual factors of management studies have compromised the chance of advancing the research-based professionalization of management practice. As Whitley (1995, p. 94) wrote,

> the highly contextual, interdependent and dynamic nature of many managerial problems, and high level of employer control over managerial tasks and role definition, severely limit the extent to which academic

> institutions are able to control the definition and assessment of managerial skills. [. . .] [E]mployers are most unlikely to rely on the academic definition and certification of managerial competencies, and formal research-based knowledge is rarely directly applicable to practical problems.

Taken together, these works make several important contributions to the relevance debate: first, they highlight the fact that the aim of generating practically relevant knowledge implies that researchers need to preserve their independence and critical distance from practitioners and their interpretations of the practical world. Interestingly, this conclusion seems to contradict various views in the programmatic literature, which suggest that the distance between researcher and practice is a reason for the relevance problem. Furthermore, they emphasize that because of their practice orientation applied researchers need to be particularly reflexive regarding their value judgments and the way that the research output affects management practice. Third, they highlight the rhetorical dimension of the relevance debate itself, which can be seen (at least in part) as a means in a struggle for control. Fourth, these studies show that the focus on producing practically relevant knowledge opens up management studies to external influences and reduces the possibilities for the internal coordination of research activities resulting in a "fragmented adhocracy" (Whitley, 1984b). Finally, these studies have shown that due to the particular contextual factors, the possibilities for developing a research-based profession of management are severely limited.

Science-Based Advice

The final stream of literature that we will briefly describe here is the interdisciplinary research on science-based advice. This stream differs from the others that we reviewed earlier in that it is concerned not only with management research but with the sciences more generally and in that the studies that represent it have been published in outlets that span a range of different disciplines. The focus of these studies is on advice-giving in the political arena, a process that is often highly formalized due to its political and public significance. Of particular interest are the consequences of different forms of interaction between those who give and those who receive scientific advice.

Maasen and Weingart (2005) have made a number of important observations: first, they have pointed out that the extensive availability of scientific advice does not necessarily reduce the uncertainty of decision-makers. Regarding issues such as financial crises or genetically engineered food, many scientific experts have been asked by politicians to investigate, discuss and assess the risks and benefits that are at stake. However, all the analyses, discussions, and assessments that have resulted from these investigations have not made these issues less controversial (see, e.g. Levidow, 1999). Dissent between scientific advisers about the same decision problem is quite common (see, e.g. Bogner & Menz, 2010). Advice from experts on critical issues, such as the environmental impact of new technologies, is often contradictory and for that reason the expertise of the consulted scientists may appear to be questionable (see, e.g. Böschen et al., 2010). Consequently, as Jasanoff (1994) has pointed out, political decision-makers may feel that they cannot rely on the authority of science in order to resolve complex issues that affect governmental policies. Second, Maasen and Weingart (2005) stressed that scientific advice-givers act politically. As Bijker, Bal, and Hendriks (2009, p. 143; see also Hilgartner, 2001) have shown, scientific consultants perform "backstage coordination work [such as] problem definition, committee formation, and writing conventions [through which] a scientific advisory body positions itself to its audiences" and adapt their reports "to the changing social circumstances and contexts" (Bijker et al., 2009, p. 143). Third, as various authors have shown, the demand for scientific advice among political decision-makers is growing, even though such advice is often contradictory (Bechmann, 2003; Heinrichs, 2005). This trend is attributed to the proliferation of expertise, which "reflects the ongoing specialization of knowledge that leaves virtually everybody to be layman in almost any realm of knowledge and expert in a very narrowly defined area of activity" (Halffman & Hoppe, 2005, p. 5). It is also attributed to the perception that objective scientific truth is hardly available. This allows actors affected by political decisions to mobilize scientific expertise that supports their interests and thus to force political decision-makers to "validate" the information that is in line with specific interests with information from "neutral" scientists. As some researchers have shown, the increasing demand for scientific advice in the political arena has created a market for advice consisting of think tanks, NGOs, independent research institutes, and commercial consulting companies—not all of which are committed to serious academic research (Lentsch & Weingart, 2011).

As Lentsch and Weingart (2011) have noted, it is widely acknowledged in the political arena that the scientific analysis of politically relevant problems is characterized by uncertainty, assumptions, and value loadings. While science cannot resolve all the questions regarding these problems, it is expected that they will be subjected to explicit and systematic analysis that will be then communicated to decision-makers. The criteria on which such analyses are based do not represent "scientific truth" but "serviceable truth: a state of knowledge that satisfies tests of scientific acceptability and supports reasoned decision making, but also assures those exposed to risk that their interests have not been sacrificed on the altar of an impossible scientific certainty" (Jasanoff, 1994, p. 250). Furthermore, the quality of scientific advice is measured in terms of its political robustness; that is, on whether it is acceptable and feasible "to implement recommendations based on [such advice and] normally implies that the knowledge and the preferences of those who can be considered stakeholders are taken into account" (Lentsch & Weingart, 2011, p. 8). Finally, scientific advice is assessed in terms of the extent to which it lends itself to the media discourse (Weingart, 1998).

Studies have shown that the influence that scientific and political stakeholders have on the advice and the ensuing decision depends on the way in which the process of advice giving is organized. Three different types of organizations—each corresponding to a different "design"—that provide advice can be distinguished in the literature: the first are collegial bodies in which the representatives of different scientific and political groups negotiate recommendations that will be made to political decision-makers. An example of such a body is the Royal Commission on Environmental Pollution in the UK (Owens, 2011), which "has enjoyed an unusual degree of legitimacy and trust" (Owens, 2011, p. 91). This success was made possible "by its constitution as a 'committee of experts', which means that its approach to any given subject involves a highly effective combination of specialist expertise, alternative disciplinary lenses and what amounts in every study to an intelligent lay perspective" (Owens, 2011, p. 91).

The second form corresponds to hierarchical, research-based organizations that offer advice but separate the decisions that concern the content of recommendations from the subsequent implementation of these decisions. An example is the Dutch institutional framework of social–economic policy preparation (Butter & Mosch, 2003, p. 363), which is characterized by the clear separation of different lines of accountability. In the first step of policy preparation, an autonomous agency, the Dutch Central Bureau of Statistics (CBS), carries out an independent collection of data. In the second step, the Netherlands Bureau for Economic Policy Analysis (CPB) analyzes the potential effects of current and future government policies on the economic development on the basis of a formalized econometric model. In the final step, policy goals are developed in the Social Economic Council (SER), whose members are representatives of labor unions, employer associations, and independent members who consist of professors, the head of the Dutch Central Bank, and the director of the CPB. The SER "works as a device to inform the government about the points of view of employee and employer organizations about social-economic questions. [...] The presence of economic and legal scientists make sure that the discussions are based on solid arguments" (Butter & Mosch, 2003, p. 370). As Butter and Mosch emphasize, the SER has an important function in "promoting trust between the various policy-makers by acting as a platform of discussions for social partners, government, central bank, CPB, and scientists" (Butter & Mosch, 2003, p. 369).

The third category of advice-giving organizations is "academies" relying on the expertise of highly distinguished scientists who are considered entitled to contribute to an issue of political relevance. For such “academies”, quality control is a question of process management:

> Good advice in this sense is not just the output of a project, but rather a matter of ensuring that all aspects of the process are targeted on delivering the ultimate goal: bringing scientific issues to bear on policy and thereby making an impact on the development of that policy. (Lentsch & Weingart, 2011, p. 14)

While the design of advice-giving organizations depends on the national political culture and the stage of maturity of the scientific field they represent, researchers have identified a number of general principles that increase the legitimacy of and trust in such organizations, such as maintaining distance and independence between advisers and advised and ensuring that the advice-giving and decision-making processes are transparent (Lentsch & Weingart, 2011, pp. 15–16).

Taken together, these works provide several important insights into how management research is utilized. First, while the relevance debate typically focuses on management practice, the discussion on advice-giving draws attention to the political arena as another domain outside science that is affected by the output of academic research. Second, it highlights the effects of institutionalized and highly structured forms of exchange between science and practice in the political field. Third, it brings to the fore the inevitably political nature of science-based advice-giving. Advice-giving cannot but be political. As several studies have shown (e.g. Bimber, 1996), actors draw on research results selectively, depending on whether they serve their interests. Similarly, scientists often act politically and the political acceptability of the suggestions they make is the core concern in the process of giving advice. Fourth, these studies indicate that the design of the advice-giving process determines the impact that advice-giving has on policy-making more strongly than the scientific content of the advice does. Finally, these studies support the idea that science-based advice does not resolve political issues. Interestingly, neither is it expected to do so. Quite often academic research generates contradictory findings, which means that decision-makers cannot draw an incontrovertible conclusion. The recipient of contradictory advice has to decide which advice to trust or whether to distrust academic research altogether.

Toward a Research Program on the Utilization of Management Research

Our systematic review of the relevance literature has revealed some fundamental problems in the current debate, all of which stem from the fact that in the programmatic literature the proposed solutions to the relevance problem—and thus the resulting debate—largely lack adequate theoretical and empirical foundations (Augier & March, 2011; Bartunek & Rynes, 2014; Corley & Gioia, 2011; Wensley, 2007). This lack has inevitably hampered progress. In addition to that, there has been little serious engagement with the various criticisms of the individual programmatic suggestions. This situation is surprising for two reasons. First, it clearly contradicts the standards of scientific inquiry that are applied elsewhere in management research; namely, that arguments need to be empirically and/or theoretically substantiated and to reflect the current state of a debate. Second, several works in the descriptive relevance literature provide fruitful theoretical and empirical insights, which, however, the programmatic relevance debate has largely ignored. Furthermore, many of the views expressed in the programmatic relevance debate seem to contradict some of the central tenets of the existing theories of organization and management (e.g. Hodgkinson, 2012).

Given these fundamental problems, it seems necessary that the current relevance debate should be reorientated in order to advance this important area of inquiry. In the following, we will argue that the debate should shift its focus from developing immediate solutions to developing a rigorous research program on the utilization of management research and the social dynamics between academic and experiential knowledge. Our suggestion that the relevance issue as such should become an explicit object of research is not entirely new, as many similar implicit or explicit calls have already been made in the existing literature (Beyer, 1982; Beyer & Trice, 1982; Gruber & Niles, 1975; Jarzabkowski, Mohrman, & Scherer, 2010). Nyilasy and Reid (2007, p. 440), for example, have written: "With a Copernican turn, we need to start using our own social scientific methods and observe the gap in an empirical-positive manner [. . .] and we need to launch research projects to investigate". However, the parameters of such a reorientation have not been clarified in sufficient depth, so these calls have not had much of an impact to date. In contrast, in other disciplines outside management studies, such as nursing studies (Estabrooks, 1999), education studies (Huberman, 1987, 1994), sociology (Beck & Bonß, 1991), and studies of science (Rich, 1991), a similar reorientation started a long time ago. The precedent that these fields have set could provide useful guidance to management studies.

The proposed reorientation of the relevance debate toward the utilization of research has several advantages: first, conceptualizing relevance in terms of "utilization" is much broader in scope than conceptualizing it in terms of "diffusion", "transfer", or similar terms. The concept of utilization does not restrict the ways and the context in which research is used, nor does it prescribe who can use it and what role its users play in the process. Furthermore, it allows for the possibility that the original knowledge is transformed or enriched as it becomes utilized. Finally, it does not restrict the kind of impact the utilization of knowledge has and the extent to which this impact is considered desirable by the various stakeholders. Second, because this concept is "open" and encompassing, it is possible to integrate the diverse streams of research that rest on different, narrower concepts. Finally and most importantly, the development of a rigorous scientific program on the utilization of scientific knowledge offers the opportunity to develop a sound understanding of the relation between management science and its external constituencies, which in turn can provide the basis for a more rigorous debate on the possibilities of increasing the relevance of management research. The proposed reorientation does not mean that the search for solutions to the relevance problem should be given up, but suggests that this search should be subsidiary to the theoretically and empirically driven exploration of the utilization process.

In the following, we describe four central aspects of developing a research program on the utilization of management research. These concern the necessity to identify and develop theoretical frameworks and concepts, initiate specific empirical research projects, identify suitable methodological approaches, and examine the impact of the relevance debate on management research and practice.

Developing Theoretical Frameworks for Research on the Utilization of Management Research

One problem in the current relevance debate is the lack of accepted theoretical frameworks that make it possible to grasp the complexities of utilizing management research. Large parts of the programmatic relevance literature rely (implicitly or explicitly) on models based on the problematic notion of a simple linear transfer of knowledge. These models cannot capture adequately the complexities of the utilization process. However, although they have been repeatedly criticized (Nicolai, 2004; Nicolai & Seidl, 2010; Whitley, 1984a, 1995), they still persist in this debate. Many decades ago, Cherns was among the first to point out "the problems that arise for the use of research from the false models which are frequently held of the process whereby research actually gets into use" (1972, p. 25; see also Cherns, 1968). The persistence of these models is surprising since there are theories of organization and management that are much more advanced. The existing body of theories of organization and management provides a rich pool of concepts that can be employed in research on the utilization of management research. In our review of the descriptive literature on relevance, we pointed out a number of concepts that could be exploited more systematically, such as the models of the management fashion market (Abrahamson & Fairchild, 1999, 2001) or the system-theoretical models of differentiating between management science and practice (Nicolai, 2004; Nicolai & Seidl, 2010; Rasche & Behnam, 2009). A broad research program on how management research can be utilized might also benefit from comprehensive theoretical models that allow for a more holistic view of the dimensions of the utilization process. Such models could help orient as well as integrate different research endeavors—as similar models have done in other disciplines (e.g. Huberman, 1987; Wingens, 1990).

While the present study does not allow us to expand on such frameworks, we would like to point out some of the features they ought to possess in order to capture the complexities of research on the utilization of management research. Some of these aspects have already been highlighted in the descriptive relevance literature reviewed earlier. The first and most critical aspect concerns the central concept of academic and other knowledge. The prospective frameworks will need to take into account that knowledge is defined by the particular context in which it is embedded (Brown & Duguid, 1991), which means that knowledge cannot be considered independently of its context. This, of course, renders the notion of "knowledge transfer" untenable. Thus, academic knowledge inevitably gets transformed in the utilization process. This point also highlights the fundamental differences between academic knowledge and practical or "experiential" knowledge; that is, knowledge embedded in a scientific context, as opposed to knowledge embedded in practical contexts (Augier & March, 2007, 2011; Van de Ven & Johnson, 2006). Recognizing that knowledge depends on its context also draws attention to the different modes of production and consumption of academic and of practical knowledge.

The second point we would like to make is that the scientific domain needs to be clearly conceptualized. Much of the relevance literature is based on a vague notion of science (Nicolai & Seidl, 2010) that centers on the activities of professors (Oviatt & Miller, 1989) or more generally on the activities that take place within academic institutions (Behrman & Levin, 1984). Although ambiguous concepts are neither unusual nor necessarily problematic in management research (Astley & Zammuto, 1992; Bacharach, 1989), in this case, ambiguity hampers efforts to understand how management research is utilized. Without a clear concept of science it is difficult to discuss the relation between science and practice and to distinguish between genuine scientific and non-scientific knowledge. As we highlighted earlier, in the programmatic relevance literature, most of the examples of successfully utilized management research do not even concern scientific knowledge.

Our third point is that any prospective theoretical frameworks need to take into account that the utilization of knowledge is shaped by its social and political context. Utilized knowledge becomes "edited", "translated" (Czarniawska & Sevón, 1996), or "re-interpreted" (Seidl, 2007), in the sense that it becomes embedded into existing knowledge structures (Sahlin & Wedlin, 2008). Furthermore, in contrast to what much of the programmatic relevance literature implicitly assumes, the utilization of knowledge is not politically neutral, but, as various authors have pointed out (e.g. Hodgkinson, 2012), molded by various interests. This makes it necessary to acknowledge "the 'bargained' nature of research knowledge, whose use is invariably 'strategic' in the social setting in which it is introduced" (Huberman, 1994, p. 18).

Our fourth and final point is that prospective theoretical frameworks need to acknowledge that there are many different ways in which management research can be utilized. Thus, they need to transcend the narrow focus on instrumental forms of relevance that characterizes the programmatic literature and offer conceptual tools for differentiating between the various ways in which knowledge can be utilized. Classifying these ways into instrumental, conceptual and symbolic forms of utilization is a good starting point (e.g. Astley & Zammuto, 1992; Beyer & Trice, 1982). A further step beyond this "rather primitive distinction" (Rich, 1991, p. 333) would involve devising more refined ways of differentiating between various forms of knowledge usage (for suggestions see Rich, 1991). Apart from providing a more comprehensive picture of how knowledge is utilized, a broader range of typologies might also promote the more systematic examination of the processes that are associated with different forms of utilization.

Launching Empirical Research Projects

Apart from developing more sophisticated ways of conceptualizing the process through which management research is utilized, it is also necessary to study empirically various aspects of this process (see Jarzabkowski et al., 2010, p. 1199). As we showed earlier, many works in the relevance literature lack empirical substantiation and are based at most on anecdotal evidence (Augier & March, 2011; Wensley, 2007). In contrast, the descriptive relevance literature has produced various empirical findings. Nevertheless, many important aspects of the utilization process are still poorly understood. Most importantly, the relevance debate is dominated by many unsubstantiated claims that have yet to be empirically confirmed or rejected. In addition to the aspects already covered by the various streams of the descriptive literature, we would like to highlight several important areas of empirical research.

First, it is necessary to test empirically the cases that have been hailed as examples of successfully utilized management research. As pointed out earlier, the programmatic literature frequently refers to individual cases that are supposed to prove to some extent the efficacy of the suggested solutions to the relevance problem. However, so far there have been no rigorous empirical studies on (1) whether management research has been indeed utilized in these cases, (2) whether these examples, if genuine, can be perceived as successful, and (3) what explains their alleged success. Second, it is necessary to study empirically different forms of research utilization within organizational and other non-scientific contexts. This will shed light on the different ways in which research can be utilized as well as on how these interrelate. Third, it is necessary to conduct longitudinal studies that trace the entire process of knowledge production and utilization. As various authors have highlighted (e.g. Beyer, 1997; Mesny & Mailhot, 2012; Weiss, 1980), this process can be lengthy and complex and is still poorly understood in its entirety.

Fourth, although several studies have usefully examined the impact of management research on the general management discourse, it is necessary to examine how the general management discourse in turn affects managerial practice. Fifth, it is necessary to examine empirically the conditions that facilitate research utilization within specific organizational contexts. Existing research has highlighted some potentially important factors, such as the "interpretive viability" (Benders & Bijsterfeld, 2000) of research results and their "resonance" (Seidl, 2007) within the organization (Hodgkinson, 2012). However, apart from the fact that these tentative findings require further empirical substantiation, they are far from providing a comprehensive view of all factors involved in the relevance problem. Finally, it is necessary to examine systematically what impact the suggestions of the programmatic relevance literature have on the utilization of management research. As already noted, many of the suggested solutions have failed to solve the relevance problem; for example, launching bridging journals (Schulz & Nicolai, in press) and including dedicated sections in research articles on the implications of the presented research for practitioners (Bartunek & Rynes, 2010) have not made a visible difference. This makes it necessary to examine in detail why these solutions have failed and to prevent the reiteration of the same ineffective solutions in the future.

Methodological Challenges and Developing Suitable Methodological Approaches

In order to study empirically how management research is utilized, it is necessary to identify and develop appropriate methodological approaches. However, this poses significant methodological challenges, considering that the findings of management research are unlikely to be utilized directly, without being further processed. Weiss (1980, p. 381) coined the term "knowledge creep" to capture the subtle and indirect ways in which scientific knowledge may come to influence practitioners. Mesny and Mailhot have argued that that relevance gap might be partly a "visibility gap" (Mesny & Mailhot, 2012, p. 198), because researchers typically focus only on the ways in which management research is visibly utilized and overlook the rest. In the same vein, Beyer (1997, p. 18) cautioned that if we only look for "direct traces of our research in managerial actions, we will not only be doomed to disappointment—we will have failed to understand the nature of research utilization". Thus, empirical research needs to tackle the longitudinal nature of the process through which knowledge is utilized, as well as the fact that this process is not linear and that knowledge is likely to change in the course of being utilized and thus become harder to trace.

Despite these challenges, empirical research in this area is not impossible. Mesny and Mailhot (2012, p. 198), for example, have pointed out that these challenges are "common to a large number of scientific fields, especially the social sciences" and suggested that researchers follow "the path of other fields in the social sciences which have found ways to document the dissemination of social innovations and make conceptual relevance more traceable" (Mesny & Mailhot, 2012, pp. 201–202). Apart from that, there are many works in the descriptive relevance literature reviewed here that have studied research utilization successfully and could serve as a basis for further research. These include various case studies (e.g. Abrahamson & Fairchild, 2001; MacKenzie & Millo, 2003; Nicolai & Dautwiz, 2010), bibliometric approaches (Schulz & Nicolai, in press), historical approaches (Augier & March, 2011; Wensley, 2007; Whitley, 1984a), and textual approaches (Barley et al., 1988; Beech et al., 2010; Benders & Bijsterveld, 2000; Nicolai & Seidl, 2010; Pearce & Huang, 2012; Rynes et al., 2007).

The Effect of the Relevance Debate on Management Research and Practice

A well-designed research program for examining how the knowledge generated through management studies should be utilized should also examine the impact of the relevance debate on the utilization of such knowledge. In other words, the relevance debate should be treated in itself as an empirical phenomenon that is likely to affect both research and practice. This would involve studying how research practices have changed in response to particular suggestions of the programmatic relevance literature: for example, Bartunek and Rynes (2010) examined how the relevance debate has affected the practice of including a section on the practical implications of the research presented in academic papers. In the same vein, it would be worth exploring why scientists adopt some programmatic suggestions but ignore others and why some of these suggestions are adopted substantively while others just symbolically. Finally, it is necessary to examine how the relevance debate affects the likelihood of practitioners adopting the knowledge that management research generates, the ways in which they may utilize it, and how they perceive management research in general. Although the inconclusiveness of the overall debate might create ambivalence among practitioners toward the practical utility of management research, at the same time it might make them more aware of the various ways in which management research can be utilized in practice.

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