On the Impossibility of Collaborative Research – and on the Usefulness of Researchers and Practitioners Irritating each Other[1]

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Abstract: In this paper we discuss on the basis of Luhmann's system theory the rigor-relevance-gap in management research and the proposal to overcome it with collaborative research. From a system theory perspective, social systems are self-referential or autopoietic which means that communication elements of one system such as, for example, science, cannot be authentically integrated into communication of other systems like business organizations. The self-referentiality of management research becomes obvious through an analysis of the current evaluation system. Social systems can only irritate – provoke – each other, i.e. alter conditions in such a way that respective other systems are forced to respond. As we argue in this paper, because of the differences between management science and practice and the closure of the science system researchers are not able to assess practical relevance of research within the system of science. On the basis of our analysis we show that, similar to other attempts to generate "applied science", "collaborative research" cannot succeed in producing research that is rigorous as well as relevant. Researchers and practitioners cannot collaboratively research they can only irritate each other. However, under certain conditions irritations or provocations turn out inspiring.

Keywords: applied research; collaborative research; evaluation of scientific work; rigour-relevance-gap; system theory

1. The Gap: From too Little to too much Science

During the Korean War, the Ford Foundation developed an interest in strengthening American management by improving management education. A series of papers and reports, culminating in the 1959 Gordon-Howell and the Pierson Report, described American business education as an assembly of trade schools lacking a strong scientific foundation (Gordon and Howell 1959; see also Pierson 1959). With donations totaling US $ 35 million, the Ford Foundation and the Carnegie Foundation, supported by AACSB and the Academy of Management, sponsored activities aiming at infusing science into management education and research (Cheit 1985; de Rond and Miller 2005; Goodrick 2002; Schlossman et al. 1987). The leading business schools hired faculty from such fields as industrial and social psychology, applied mathematics, statistics and economics whose scientific basis was beyond any doubt. The “hypotheticodeductive” (de Rond and Miller, 2005: 323) approach became standard (Harmon 2006; Hugstad 1983; Mintzberg 2004; Schlossman et al., 1987; Waldo 1955; Whitley 1988).

However, in the early 1980’s, only 25 years after this scientification initiative American business schools again came under heavy attack. This time overscientification was the problem (Aaronson 1992; Cheit, 1985; Hayer and Abernathy 1980b; Leavitt 1989; Muller et al. 1988; Rehder et al. 1991). Hayer and Abernathy (1980a) criticize the “sophisticated business curriculum” with its preference for “analytic detachment rather than insight that comes from ‚hands-on’ experience“, building upon wrong management ideas and models that do not respond to managerial needs. An AACSB sponsored report by Porter and McKibbin (1988) complains that the preparation of those who are going to teach management is narrow, overly-specialized, and does not provide them with the ability to relate to realistic management problem solving situations (Wren et al. 1994).

From that time on, the rigour-relevance-gap has become a prominent and perpetual topic within management science as a look into the yearly presidential addresses at Academy of Management meetings reveals. For example, in 1993, Donald Hambrick (1994: 13) bemoaned the “incestuous, closed loop” of management research and exclaimed “It is time for us to break out of our closed loop. It is time for us to matter”. In 2006, Thomas Cummings (2007: 356) deplored “[F]ew of us truly believe that practitioners really listen to us, and, if they do, they sure don't seem to be doing much with what they've heard". He conjured the "relevance ghost" that "continues to haunt us from … one presidential address to the next."

The rigour-relevance gap has been not only discussed in presidential addresses but also in special issues of journals (Beyer and Trice 1982; Hodgkinson et al. 2001; Rynes et al. 2001), articles outside these special issues (Beer 2001; Buckley et al. 1998; Daft and Lewin 1990; La Force and Novelli 1985; Lallé 2003), books (Campbell et al. 1982; Hakel et al. 1982; Murphy and Saal 1990; Van de Ven 2007) and edited books (Larwood and Gattiker 1999; Noll 1998).

In these discussions, collaborative research, i.e., research jointly performed by researchers and managers, is the most popular remedy for closing the gap between theory and practice (see, e.g., Anderson et al. 2001; Pettigrew 2001; Rynes and McNatt 1999; Van de Ven and Johnson 2006). In this article, we analyse whether collaborative research really is a successful remedy. We thereby proceed as follows: We first on the basis of Luhmann's system theory explain why science and practice (business organizations) as self-referential systems that cannot directly communicate with each other. According to Luhmann (1977; 1986), in the course of modernization, social systems like science, law, economy or religion developed into self-referential systems. Self-referentiality enabled these systems to dramatically increase their abilities for developing ever more effective mechanisms for dealing with juridical problems, for doing research with increasingly sophisticated theories and methods, for continuously boosting growth and efficiency of the economic system, for generating religious comfort without interfering with other society's functions, etc. However, a price had to be paid for this escalation of the performance of systems and this was an increasing encapsulation of these systems, an increasing self-referentiality or autopiesis. Consequences of this inability for the system of science are pointed out in an analysis of "applied research" that only rarely finds its way into top-tier journals indicating a trade-off between rigour and relevance. We then argue that genuine collaborative research is impossible and support this contention by critically examining reports on collaborative projects. We conclude that at the utmost the systems of science and practice can irritate each other. Conditions are discussed under which these irritations are likely to have positive effects for the participating systems.

2. Science and Practice as Self-referential Systems

2.1. Science

2.1.1. Communication in Science

Imagine a manager who incidentally gets hold of an issue of *Administrative Science Quarterly* or *Academy of Management Journal*. She discovers a title that catches her attention because somehow it relates to a problem that is a currently controversially discussed topic in her company: motivation through pay for performance in management. She skims through the article. Under the heading *Practical Implications* she finds some advice that appears helpful. But when she tries to read the whole article she meets with extensive difficulties. Actually, she cannot understand what she reads. References are made to theories which she never has heard of. A lot of references to other articles make her feel lost because she does not know how to access these many articles nor can she guess whether reading them would help her to understand the text. She cannot find that the article really deals with the problems her company is actually struggling with as she cannot identify a solution, neither in the hypotheses that are derived from a theoretical part nor in the results. The author has obviously applied complex methods to find out whether his hypotheses are correct but she would prefer an article in which a solution that has been successfully applied by a company – preferably a company similar to the one she is working in – is described in some detail. She wonders herself why researchers cannot discuss problems in a way she can understand. Why do they base their arguments on theories practitioners are not familiar with? Why do they refer to so many other publications which practitioners cannot know? Why do they apply so complicated scientific methods which nobody uses in practice? Why are practical implications formulated so vaguely?

The answer is a simple one: researchers do all that because they are supposed to. They just follow the rules of their community. If a researcher does not follow these rules the scientific community, in all likelihood, does not pay attention to his research output. Communication among researchers is about truth or, to use a somewhat less grandiose word, about adding new insights to scientific knowledge. Theories are the only legitimate basis to develop speculations on how to explain phenomena. For researchers hypotheses that are derived from theories are attempts to find out whether assumed causal relationships hold or do not hold – are true or false. Empirical analyses are tests for trueness. In order to formulate his hypotheses a researcher has to know which hypotheses other researchers who work on the same phenomenon (e.g., motivation through pay for performance) have tested with which results. He can achieve scientific progress by empirically supporting results of other researchers, thereby increasing the likelihood that these results are valid (true), by extending hypotheses that other researchers have formulated and, eventually, testing them and thus discovering contingencies under which extant results hold or do not hold or by delineating new hypotheses from new theories that he or others eventually test. Whatever this researcher does, he refers to other researchers' work what he has to acknowledge through citations. If he tests or retests hypotheses he has to apply methods that are accepted as legitimate within the scientific community. If he refines methods he has to show in which ways they are superior to the methods developed by other researchers he builds on. If he develops a new theory he is expected to discuss why and in which ways this new theory is more powerful than extant ones. Whatever researchers do, they work with and build on elements – theories, results, methods – that already exist within the scientific community. In short:

> "The publication [of a researcher] … is an element that rests upon other elements of the same type, i.e., other publications, and it refers to these other elements through citations

> [references to other authors'publications]. The function of a publication is to stimulate other publications that tie in with this publication and that in turn have to document this fact to their citations." (Stichweh 1994: 64, translation of all quotes from German publications by the authors)

In other words: the system of science is self-referential (Kieser and Leiner 2008; Luhmann 1998; Macdonald and Kam 2007; Seidl 2007). The incestuous loop castigated by Hambrick (1994) is a characteristic of normal science, natural science included. Only communication that is connectable to other communication within the system of science is legitimated as scientific communication.

Communication within the scientific system is not only self-referential it is also inaccessible – closed – for communication from other systems. For example, problems from practice cannot be simply imported into the system of science in order to generate scientific solutions for them. They have to be transformed (not translated!) into scientific communication. For example, a problem concerning pay for performance would have to get embedded within a theory, for example, agency theory and transformed into hypotheses, before it could be processed in the science system. Otherwise the members of the scientific system would not busy themselves with this problem in their role as researchers since they are supposed to deal with *scientific problems*. What will happen to a practice problem that is recasted within the system of science remains to be seen. In all likelihood the reaction will not result in "actionable knowledge" but rather in scientific knowledge. Communication from other systems – practitioners seeking advice on pay for performance or for other problems, the political system bringing out a budget for applied research or industry sponsoring an institute for collaborative research – may trigger reactions within the system of science but these reactions cannot be controlled in any way by the respective external systems. The system of science is open for inputs from other systems but if it responds it does so in its own ways.

The system of science's self-referentiality can result in separating research from teaching (Seidl, 2007): as researchers academics refer predominantly to researchers and their results, as teachers to other teachers, their textbooks, cases and overheads. This contention is supported when researchers complain that scientific results do no longer find their way into teaching (see, e.g., Czarniawska 2003), when research and teaching are seen as "different enterprises" (Barnett 1991; Hattie and Marsh 1996: 513) or when conflicts between teaching and research emerge (Gioia and Corley 2002; Harmon, 2006; Walstad and Allgood 2005). This observation ties in with the expectation of system theory concerning the presentation of scientific results to the public: „It is realistic to expect narrow limits of comprehensibility and the creation of a different kind of literature for popularization, didactical processing and lexical presentation [of scientific results]" (Luhmann, 1998: 624).

Business schools have developed strategies for concealing the gap. One strategy consists in employing two faculty teams – one for rigour and one for relevance (March and Sutton 1997; Zell 2001). Gioia and Corley (2002) even consider a third team specialized for teaching. Researchers cultivate an image of applicability by, e.g., publishing analyses with performance as a dependent variable (Kieser and Nicolai 2005; March and Sutton, 1997), adding an implications section to their articles, temporarily switching to the role of consultants, or pretending that practitioners contributed the formulation of their research problems. One could even suspect that propagating collaborative research is also such a strategy.

As we will see in the next section, evaluation processes within management science reinforce sel-referentiality.

2.1.2. The Social Construction of Rigour and Relevance in Evaluation Processes

The self-referentiality of the science system rests upon the principle that only scientists are entitled to evaluate science. This principle applies, e.g., to review processes for papers submitted to journals and to tenure and promotion decisions within universities. From the beginnings of modern science, the prestige of researchers within the scientific community has depended on their research output in form of texts and appreciation of this output by peers. However, before the 1950ies outside peer reviews were not common in medicine and science journals (a history of reviews in social science journals does not yet exist). The editors, sometimes assisted by colleagues whom they consulted if they did not feel too competent, alone decided on the acceptance of manuscripts for publication (Burnham 1990; Kronick 1990; Spier 2002; Zuckerman and Merton 1971). From the 1950s on, major journals gradually introduced peer evaluations as a regular procedure. After publication colleagues implicitly grant merit by referring in a more or less approving way to research results of colleagues and by extending or refining other researchers' results in their own research output. In this way, acceptance and appreciation of research results have always been the outcome of a discourse between researchers – a discourse through which rigour is socially constructed as researchers continuously specify demands for research quality. As a result of this discourse views on the appropriateness of theoretical and methodological approaches and criteria of research quality change over time. From the end of the 19th century on, a constantly growing community considered social science research that resembled natural science research as superior to other approaches, e.g., the historical approach (Camic and Xie 1994; Fourcade-Gourinchas 2001). The scientification process that started in the 1950ies boosted this trend. Conventions for the presentation of evidence were also subjected to discussions leading to tighter specifications for the acceptance of submitted papers. The review process became more formalized. Open reviewing was gradually replaced by blind and then by double-blind reviews and rules that governed the selection of reviewers and the succession of editors became more refined (Burnham, 1990; Kronick, 1990; Spier, 2002; Zuckerman and Merton, 1971). At some point of time, journals started to provide evaluation forms containing criteria extending to innovativeness, methodological power and quality of writing to reviewers.

The increasing number of journals in a field stimulated a need for ranking journals according to their importance. To certain extent, jounals' affiliation with associations or schools or editors' reputation of signalled importance. In 1955, Eugene Garfield suggested to systematically tally up the number of citations the articles of a journal received in a sample of journals to calculate an "impact factor" as an indication of journals' importance in a scientific field (Garfield 1955). This idea that revolutionized evaluation practices was based on the scientific community's well established practice to grant approval of research quality through citations (Zuckerman and Merton, 1971). Garfield founded the Institute for Scientific Information (ISI) which developed into an evaluation industry (Osterloh and Frey 2006) with hundreds of employees and a database of more than 550 million citations (Perkel 2005). Nowadays, citation indices play a crucial role in hiring, tenure, promotion, salary and grant decisions within science (Monastersky 2005). For many, citation indices seem to provide an objective measure of the impact not only of journals but also of individual researchers, schools and universities on the advancement of science.

The SSCI facilitated comparisons on the basis of seemingly unambiguous and easy to obtain performance measures and thereby drastically intensified competition between researchers, schools and journals. It also induced researchers (Bedeian 2006) and research institutions, notably journals, to develop strategies for increasing citations of their own publications, some of them ethically questionable (Monastersky, 2005).

A recent article by Judge et al. (2007; for a similar study with somewhat deviating results see Stremersch et al. 2007) can serve as an example to demonstrate how discourses can influence researchers' perceptions of research quality – rigour – and relevance and thus socially construct the appropriateness of research approaches. Judge et al. (2007) identify strategies for achieving success in an evaluation system in which citations achieved is a crucial performance indicator. They differentiate between universalistic predictors of citation frequencies like originality, methodological quality and clarity of presentation and particularistic predictors like an author's publication productivity, prestige or the author's affiliation. According to their concepts of science universalistic predictors should be given more weight than particularistic predictors. Arguing that the "peer review system serves as a quality screen that is more rigorous at higher-quality journals" (p. 494) Judge et al. (2007) add a measure for the impact of the journal in which the article appeared. Based on the assumption that reviews may be more often cited than individual empirical pieces the authors among a number of other control factors controlled for whether a paper was a review (e.g., meta-analyses). Publication in a journal with a high average citation rate turned out "the single best predictor of citation" (p. 500). However, articles with exploration research plots also had higher citations as well as the author's number of previously published top-tier articles and the prestige of author's affiliation, although a relatively weak one.

We find this study remarkable in several respects: basically, the authors analyse whether authors in selecting publications to base their own work on – or at least somehow recognize them by citation – apply the same criteria for rigour as leading journals cultivate them and whether these authors also share the conviction that the impact factor of a journal signals quality of the articles published in it. By transforming findings into implications Judge et al. (2007) contribute to the social construction and institutionalization of a research practice that takes citations for the most central criterion of quality (rigour, truth). Judge et al. (2007) do not explicitly question – though citing critical literature – the fiercely debated (see, e.g., Bedeian, 2006; Osterloh and Frey, 2006; Starbuck 2005) assumption that is central to their analysis that a journal's impact factor is strongly correlated with the quality of articles published in it. Thus this study legitimizes and reinforces researchers' orientation towards citations weighted with impact factors as an indicator of research quality, implicitly as an indicator of scientific progress (truth).

Relevance is mentioned only once. Judge et al. (2007) assume that meta-analyses which achieve very high citation rates provide "one of the best tools for sharing trustworthy information with practitioners" (2007: 503). Unfortunately, this argument is not substantiated with evidence that practitioners really find meta-analyses as trustworthy and actionable as researchers do. We contend that by excluding practitioners from the evaluation process and by emphasizing citations as expressions of collegial acknowledgement the evaluation practice analyzed by Judge et al. (2007) tends to widen the rigour-relevance-gap.

Peers are supposed not only to evaluate rigour of submitted papers but also relevance for practice. They apply this criterion by critically reading the practical implications section authors are expected to include in their papers. In other words: authors construct practical relevance and then reviewers assess whether this construction corresponds to their own construction of practice. Practical relevance is self-referentially constructed and evaluated within the science system. Experts in the construction of Potemkin villages evaluate Potemkin villages which authors have erected in their "implications for practice". The scientific community takes care that the communication on rigour is not connected – contaminated – with the practice system's communication.

We also surmise that publications like that of Judge et al. (2007) contribute to the already existing bias in management studies towards positivist research, as their predictors only apply to this kind of studies. As Harmon (2006: 239) aptly puts it: "competition requires a common currency to certify the winners and losers. The commoner the currency, the better". The referee-system amplifies the trend towards mainstream research: "when hard-pressed editors resort to editorial board members and regular authors to referee …, peer reviews can become a means of detecting deviance from a dominant view" (Macdonald and Kam, 2007: 647). Methodologically-centered evaluations of empirical studies appear more structured, more technical and, therefore, objective. Achieving consensus on them between reviewers seems easier (a false assumption as studies on the covariance of reviewers' judgments show (Cichetti 1991; Gans and Shepherd 1994; Miller 2006; Starbuck, 2005). It is relatively safe to state that a given problem is tackled with a higher degree of methodological sophistication in a certain paper than in earlier ones dealing with the same problem leaving open whether the results really constitute progress (Bedeian 1989; de Rond and Miller, 2005). As Luhmann (1998: 589) argues, empirical tests facilitate the flagging out of communication of empirically tested phenomena as “true” or “untrue” as “[t]he value symbols true and untrue are more easily available on the basis of methodological considerations”.

An evaluation system that tends to declare the mainstream’s logic-in-use (Kaplan 1964) as the appropriate one develops a lock-in effect since opposition is interpreted as fear of the consequences (Münch 2007: 172; Osterloh and Frey, 2006).

2.2. Practice

Imagine: a researcher has been given the chance to follow an executive meeting on the issue of pay for performance for managers. The meeting starts with a general discussion of pros and cons of such a system. The following questions are raised and debated: Have our main competitors already implemented such a system? Some participants are sure that they have done so. This prompts the question whether the own company will suffer disadvantages if it does not adopt this practice. Will shareholders, analysts and the press take the fact that the own company does not follow the trend as an indicator of comparatively lower management capabilities? Are our managers disappointed – and demotivated – that they cannot participate in such a system? Will some of the most qualified ones leave the company out of this reason? Would the implementation of such a system considerably increase the compensation budget? If yes, would the increase be compensated by higher performance to the effect that a reduction of the managerial workforce would be possible? Responding to this question, a finance executive strongly expresses her view that the introduction of such a system would lead to a sharp increase in management salaries, not compensated by higher performance and a possibly leaner management structure. A sales manager strongly opposes this view, mentioning that Jack Welch is a outspoken supporter of pay for performance on all levels. The discussion then focuses on the question whether consultancies with experience in pay for performance in the own industry exist and how much a project of this sort would cost. The meeting is concluded by agreeing to continue the discussion. Top management promises to present at the next meeting reports on management compensation practices in the industry and on consultancies with experience in this field and in the industry. The coincidentally present researcher is invited to report briefly (!) at the next meeting what management research has to contribute to this issue. At the next meeting, the researcher tells the executives that extant research results concerning the consequences of pay for performance in management are inconclusive. The skeptical finance executive takes this statement as an indication that systems of this sort are doubtful and should therefore be avoided ... .

What does this fictitious – but, hopefully, not unrealistic – story tell us? Organizations in the economic system are also self-referential (autopoietic) systems as they cannot directly communicate with their environment. In particular, since their members are not familiar with theories and methods, they cannot directly communicate with the science system. Nor can actors in the environment directly influence organizational decisions. Organizations have to decide what to observe in the environment, how to observe it, and which conclusions to draw from their observations. Thus Luhmann (1992b: 166, quoted after the translation by Seidl 2005: 43) defines organizations as “systems that consist of decisions and that themselves produce the decisions of which they consist through decisions of which they consist.” Decisions are the elements of organizations. Past decisions provide the information for ensuing decisions (Seidl 2005). Organizations absorb uncertainty through decision making.

It is crucial for organizations to identify contingencies produced by the organization’s structurally coupled environment and reduce the uncertainty that is linked to them. Thus, the decision not to pay any attention to results of management research on pay for performance makes sense for a company if the relationship between search effort and outcome seems dubious. The systems in the environment with which organizations are tightly coupled vary with type. Universities are most tightly coupled with the political system that deals with higher education, banks with the system of monetary policy, and so forth. Organizations permanently strive to reduce the complexity and uncertainty with which environmental systems confront them. “If all uncertainty were removed, the organization would cease to exist and no further decisions would be produced. The organization needs uncertainty for its autopoiesis” (Seidl, 2005: 43). Though organizations maintain close links with their respective environmental systems and align their structures with it, they are nevertheless self-referential systems that “have to cope with their own problems to construct a secure and expectable world of their own decisions – developing a self-constructed view of the world and a self constructed certainty about and confidence in the world” (Nassehi 2005: 187).

The environment influences decision making in organizations by framing their aims. An organization that operates in the economic system has to accept that profit and liquidity are crucial goals for its survival. Thus, communication in companies is coded along the criterion “reduces profit/increases profit”, as we can observe in our example.

Organizations interpret present states as outcomes of past decisions. They base decisions on assumptions about causality. *Causality is the organization’s generalized symbolic medium*. Since an organization is unable to operate in a world full of contingencies it has to keep its picture of the world – its assumptions concerning means and ends – simple (Cyert and March 1963) (which might explain managers’ aversion to scientific knowledge in which an increasing number of contingencies is connected to explanations and their preference for consulting knowledge that helps to keep problems simple and compatible to simple solution generators like the BCG portfolio analysis (Kieser 2002)).

An organization evaluates its decisions (if it officially evaluates them at all) after their implementation with the intention to legitimate them. These evaluations are always social constructions. As the evaluation is itself a decision (with a positive or negative result) it remains within the self-referential communication circle. If it were possible to identify optimal decisions, from a system theory point of view, there would be no reason for the existence of organizations as the structurally coupled systems could fulfill the respective tasks themselves by applying the appropriate calculus.

It is impossible for organizations to test the quality of ideas – including ideas inspired by management science – by basing decisions on them and observing and evaluating outcomes (Denrell 2003). Luhmann (1995) assumes, in accordance with other scholars of organizational evolution (see for example Aldrich 1999) that organizational evolution is only partially controllable. Basically, evolutionary processes are triggered by decisions in reaction to environmental stimuli (irritations). These decisions produce partially blind variations. Organizations react to irritating events – e.g., to restructuring concepts from management research, or to a consultancy, or to a competitor's price cut – by changing decision routines. The new decision routines are evaluated after their implementation as appropriate (stabilization) or as inappropriate (no stabilization). In the latter case, "decisions on decision routines" are resumed. Reforms – the plan to change a bundle of routines – are based on "rational" concepts. However, seen from a system theory perspective, reforms simply consist of communication. Once initiated, a reform communication triggers additional communication that connects itself to the reform. These processes may change the system in ways not intended by the original reform communication. A larger reform always leads to a result that deviates from the original blueprint formulated with some clarity. In this sense, variations are more or less blind. When a reform is finally declared completed its success cannot be assessed on the basis of objective indicators (Brunsson 2002). Success or failure of a reform is socially constructed through evaluation decisions. In other words: management research cannot treat organizations that consider research results in their decision making as units of a scientific experiment (March and Sutton, 1997).. In its knowledge production each system can consider information from the respective other system but then processes it in its own specific mode.

2.3. Why Contributions to Applied Science Are almost never Published in Top-tier Journals

Attempts of the system of science to influence practice systems necessitate compromises, as the example of applied science demonstrates. Researchers pursuing the production of applicable knowledge have to consider values, norms, and interests that prevail in organizations (Luhmann, 1998; Nicolai 2004). Furthermore, they have to use a language that practitioners understand. They also have to claim that knowledge offered for the solution of problems that are encountered in practice is unambiguous, i.e. does not contain many contingencies, since practitioners perceive knowledge in which a solution is dependent on many contingencies as highly impracticable. Researchers who develop applied science are also well advised to prevent the addressees of the knowledge, the practitioners, from looking behind the scene into the “workshop of science” (Luhmann, 1998: 650) since these would be scared by such an exposure to incommensurable theories, contradicting results, the complexity of methods and controversies about the appropriateness of methods for specific research problems. However, in science, suppressing knowledge about contingencies and contradicting theories and inconsistent results and operating with simplified research questions is perceived as a fallback behind the state of the art. Preventing practitioners from looking into the science workshop carries two risks for researchers trying to produce applicable knowledge: (1) It is difficult to link applicable knowledge with the actual scientific discourse, since, as the example of action research shows (almost no articles in top-tier journals) practice-oriented research has to compromise with regard to rigour. (2) Practitioners trying to apply applicable knowledge produced by researchers to specific problems usually discover that a lot of additional information is required to solve a given problem so that the “authority of science will be more and more questioned” (Luhmann, 1998: 641). This consequence is observable, e.g., with regard to research that treats performance as a dependent variable (Kieser and Nicolai, 2005; March and Sutton, 1997). Practitioners simply do not pay attention to the surge of publications in which so-called “success factors” are statistically filtered out.

Because practitioners' problems usually do not fall clearly into one discipline, applicable scientific knowledge is often needed for an interdisciplinary context. In many cases, such interdisciplinary research leads to a rather low level of theory development since the interdisciplinarity of extant scientific research is poorly synchronized with the interdisciplinarity of problems that pose themselves in practice. Action research as an applied approach provides an example for these difficulties being hardly ever represented in top-tier journals (Greenwood 2002; Gustavsen 2003).

Collaborative research struggles with these difficulties as well. Proponents of collaborative research point out that, since this approach is breaking new ground, it has to generate new knowledge, for example through action research or grounded theory (in other words: is not well connected to actual discourses). In this vein, Mohrman et al. (2008: 515) argue that

> „the application of teams in knowledge settings created a new context for teaming that was apparently rendering some or much of the knowledge of past practices of questionable use. We decided to use a grounded research methodology…".

In an attempt to signal connectivity to ongoing science discourses proponents of collaborative research emphasize that extant theoretical knowledge has been taken into consideration (e.g., Mohrman et al. 2008: 513) or should be taken into consideration (e.g., Adler and Beer, 2008), without making an effort of specifying which theories or methods had been used and in which ways they had contributed to collaborative projects. Likewise, proponents of collaborative research say next to nothing to the ways in which knowledge that is generated in collaborative projects might contribute to extant theoretical knowledge. As Kimberly (2007: 144) coins it: practice settings

> "often require pragmatic, time-driven, necessarily partial solutions to real client needs. They require compromise, often serious compromise, with the criteria of research excellence and integrity with which university-based researchers are inculcated during

> their training and that, I would argue, often require researchers to behave in ways that contradict what they teach their students about research design."

3. Collaborative Research – an Oxymoron?

If science as well as practice are autopoietic systems the impossibility of collaborative research follows from the fact that only people with a comprehensive training in theories and methods are capable of performing research that is considered as rigorous by peers and of collaboratively performing research, i.e., communicating research issues among each other. Practitioners do not possess the knowledge that is required for understanding and communicating research problems. Thus, the definition of collaborative research as a research that includes "the active involvement of managers and researchers in the framing of the research agenda, the selection and pursuit of methods, and the development of (implications for) actions" (Mohrman et al., 2008: 628) is misleading by implying that practitioners can become equal partners in the research process, i.e. have equal knowledge with respect to the formulation of research questions, selection and application of methods and interpretations of results.

Since the precondition of adequate training is as good as never met for the participating managers in collaborative management research projects we expect to find indications of a separation of the roles for researchers and practitioners in "collaborative" projects. In particular we assume (1) that motivations of practitioners and researchers to collaborate are different and, consequently, that they will engage in different activities and realize different rewards. Researchers will pursue research and practitioners will make decisions, both parties possibly profiting from the respective other party's activities. Practitioners predominantly expect "actionable knowledge", i.e. knowledge that can be used in decisions on the solution of acute and specific problems, researchers expect inspirations towards the production of scientific knowledge – publications. Since the barriers between the social systems of business organizations and science impede communication between them, we expect (2) that project structures are implemented and specific methods for facilitating and sustaining understanding across the groups of practitioners and researchers are applied. A further expectation is that (3) researchers in “collaborative projects” are willing to accept compromises with regard to rigour. These assumptions find support in the literature on collaborative research projects as we show in the following sections.

3.1. “Collaborative Research” Rests on Different Motivations and Produces different Results for Practitioners and Researchers

In descriptions of “collaborative research projects” some projects look like conventional executive training. The academics assist managers in acquiring new knowledge and receive some form of financial compensation in return. For example, Docherty and Shani (2008: 164) hold that “mechanisms to promote and support learning at different levels and across levels of an organization are the internal way of organizing collaborative scientific inquiry and the way of organizing, acting on, and developing the firm’s differentiated capabilities”. In other words, Docherty and Shani (2008) predominantly view collaboration as enabling managers through training to improve the performance of their organizations. Tushman et al. (2007: 348) argue that settings for executive education “forge collaborative research–practice relations“.

In a similar vein, it is not always possible to distinguish “collaborative projects” from consulting. Consulting usually entails some degree of participation of managers and employees of the client company in analysing and identifying problems and in realizing improvements of existing practices (see, e.g., Fosstenlökken et al. 2007; Kubr 2002; Morris 2000; Schein 1999). University professors from time to time shift to the roles of consultants (in which they behave differently than in their role as researchers or teachers). In a chapter on collaborative research, Werr and Greiner (2008: 94) point to “[i]nfluential researchers, such as Michael Porter, Michael Beer, and Susan Mohrman … [who] have set up their own research institutes in which they integrate research and practice, and which increasingly (and successfully) compete with large consulting organizations”. Perhaps these “research institutions” or at least their patterns of interaction with practitioners are not so different from normal consultancies. Adler and Beer (2008: 552) speak of the “scholar-consultant” as an important member of collaborative teams. One can assume that companies financially compensate scholar-consultants for their efforts. That Porter, Kaplan, Norton or Beer are linked to universities adds academic legitimacy to their service as consultants. At least from the perspective of the university “collaborative research” sounds better than “consulting”. Collaborative research projects that are not led by declared scholar-consultants take on a consulting flavour when achievements like “improvements in cost, quality, and productivity indices” are emphasized (see also Amabile et al. 2001; 2008: 56) without mentioning insights relevant for research that have been gained. However, consulting is not science (Kieser, 2002) and therefore, it seems justified when Kimberly (2007: 143) warns that “the quality of management research can be seriously compromised when researcher-manager interactions unfold under conditions of role confusion or role ambiguity, when it is not clear whether the faculty member is acting as a researcher (whose role is to discover new insights about the way the world works) or as a consultant (whose role is to provide advice to a client”. In “collaborative research projects” that do not obviously fall into the executive training or consulting category, financial contributions are not always absent. In the public sector, but not exclusively there, collaborative research is often subsidised by research grants (Knight and Pettigrew 2007; Newell et al. 2001).

When discussing conditions conducive to success of collaborative research, academics recommend creating “win-win-outcomes” (Werr and Greiner 2008: 102) for participants with different interests (Pasmore et al. 2008). The most often mentioned non-financial rewards for researchers are exposure to the “real world” (Werr and Greiner, 2008: 105), including “rich data”, and output in form of publications, presentations at conferences and dissertations

(Knight and Pettigrew, 2007; Knights et al. 2008). However, such a collaboratively achieved output is not a compensation for managers. “[I]t is simply not realistic to expect most managers to invest in joint publications” (Adler and Beer 2008: 552). Managers do not even read scientific management journal (Fry et al. 1985; Gopinath and Hoffman 1995; McKenzie et al. 2002); why should they be inclined to write for them? The implication is: managers are not interested in research and its results. They do not perceive themselves as co-researchers. To name them like this implies a “blurring of roles” (Knight and Pettigrew, 2007: 6).

The differences in motivation and interests may cause tension and conflicts. As McKelvey (2006: 825) aptly comments, ”[e]ngaged scholarship consists of pluralistic interests and conflict; there is the risk of decision by committees, power contests, and settling for the lowest common denominator”. Indeed, there are indications of conflicts. For example, Amabile et al. (2001: 427) report a “conflict over practitioner involvement that was never truly resolved”. The practitioners did not feel sufficiently involved “on an ongoing basis with the ‘real work’ of the study” (p. 426). Conflicts of this kind can be avoided by clearly separating roles and incentives for practitioners and researchers and involving practitioners in forum discussions in which the researchers present *their* intermediate results and discuss them with the practitioners. Moreover, in many recounts of collaborative projects the importance of understanding the differences between researchers and practitioners and of trust is emphasized (Amabile et al., 2001). However, understanding and trust are most needed when partners are engaged in *transactions* under the conditions of different interests and qualifications and uncertainty about the distribution of outcomes.

Thus we come to the conclusion that our analysis of descriptions of collaborative research support our contention of the impossibility of collaborative research.

3.2. Specific Structures and Methods for Facilitating and Sustaining Interactions across the Communities of Practitioners and Researchers

Since conflicts about roles and the distribution of outcomes are likely to develop a project structure with clear role definitions (Amabile et al., 2001) is required. The project leader is responsible for maintaining and adapting role definitions along the different project phases, especially through regular meetings. Also required are “communication instruments” or methods for facilitating interaction between managers and researchers who belong to different thought worlds. The proponents of collaborative research point to a number of methods that can be labelled communication instruments like appreciative inquiry (Tenkasi and Hay 2008: 56), developmental action inquiry (McGuire et al. 2008: 135), clinical research (Werr and Greiner, 2008: 106), action research, intervention research (David and Hatchuel 2008), coaching (Boyatzis et al. 2008), Dynamic Strategic Alignment (Olascoaga and Kur 2008), the Socio-technical Approach (Kolodny and Halpern 2008), Organizational Fitness Profiling (Beer and Eisenstat 2000), True-point Strategic Fitness Profiling (Adler and Beer, 2008) or narrative inquiry (Ospina and Dodge 2005). Methods of this kind are not to be found in books on research methods but are rather to subsume under intervention methods. Some methods are specifically designed for collaborative research some are copyrighted for specific institutions. However, as we have pointed out above, only broadly accepted research methods can produce broadly accepted research results, i.e. accepted for publication in top-tier journals.

For Mohrman et al. (2001: 360) “joint interpretation forums in which individuals can portray their own views of a situation, self-reflect, collectively reexamine, and come away with altered and enhanced interpretations and perspectives” are an essential feature of collaborative research. We find it remarkable that in this quote the participants do not collectively produce something. Rather they “come away with altered and enhanced interpretations and perspectives” – presumably each party for itself. The authors point out that “[b]eing involved in interpretation processes that take each other's viewpoints into account should facilitate the ability of each party to translate between, and at least partially integrate, their own and the other frameworks”. On the basis of our concept, taking perspectives is a process ridden with prerequisites. Understanding – attribution of a meaning – is not produced by the speaker but by the listener (Luhmann 1995). If a system (i.e., science or a business company) picks up texts from respective outsiders the meaning it attributes to them is determined by theist own logic, is its own product. Thus, for example, when practitioners explain a problem to researchers, the researchers have to process this communication on the basis of their own logic to be able to understand it, and vice versa. It is impossible to assess whether or to what extent the senders' and the receivers' interpretations overlap.

From this perspective encounters between practitioners and researchers, usually organized in form of projects, have to be interpreted as taking place in a "contact system", i.e., a temporary interaction system in which a discourse develops that is different from the discourses in the participants' primary systems (Luhmann 2005a; Mohe and Seidl 2007). These discourses of contact systems are also difficult to decipher for outsiders (including non-participating researchers) and this may be one of the reasons why members of the science system who have not been participating in the project are reluctant to accept outcomes as scientific publications. Also, the outcomes of these contact systems do not seem to guarantee change in the participating practitioners' organizations as this system also has difficulties absorbing the output from a contact system. Thus Mohrman et al. (2001: 370), proponents of collaborative research, are skeptical whether forums – contact systems – can bring about change even when their participants are in agreement and emphasize that additional actions in the organization are needed to implement results that have been projected in forums:

> "The creation of joint interpretive forums to interpret the data does not necessarily lead to ongoing self-design in an organization being informed by that knowledge. Taken at face value, it would seem that researchers must do more than work collaboratively with organization members to understand the research findings. Perhaps they must become part of an organization's self design activities if they wish to promote usefulness. Such participation would constitute adoption of a more traditional action research approach in which researchers are also change consultants (!) and are involved in internal organizational processes."

In other words: the system of the organization has to react to the output of the contact system – make decisions on whether and how to react. And, as Mohrman et al. (2001) also indicate, the group of researchers and the group of practitioners usually strive to maintain a distance from each other, even when cooperating in a contact system (Luhmann, 2005a). Each side is interested in preserving its own identity and logic, the members of each side reflect among themselves about the other group and discuss questions like the following ones: why do they not understand us, which arguments work best to convince the other group, why do they (in this case: the researchers) have to test this solution which other companies that need not know our (the participating company's) practices?

3.3. "Collaborative research" implies a trade-off between rigour and relevance

Proponents of "collaborative research" argue that this approach can produce results that take the dual hurdles of relevance and rigour (Hodgkinson et al., 2001; Pettigrew, 2001: 353; Van de Ven and Johnson, 2006) thus denying a pronounced conflict or trade-off between the two criteria of research quality. This assertion is extremely difficult to verify since available accounts of collaborative projects are written for researchers. The overwhelming majority of articles on the subject are texts on the epistemology of collaborative research. We do not know of any publications that contain jointly produced *research* output (Rasche 2007: 297 makes the same observation). Relevance comes into the picture through measures of practitioners' perceived usefulness that are designed by researchers in order to convince other researchers (see, e.g., Mohrman et al. 2001). The assertion that collaborative research is advancing relevance as well as rigour is contradicting our position developed above that applied research necessarily means less rigorous research and also the observation that the scientification of management science has resulted in a continuously widening rigour-relevance-gap. On closer inspection it becomes clear, however, that some authors who propagate collaborative research concede a certain trade-off, for example, when speaking of collaborative research as representing an "arbitrage strategy for surpassing the dual hurdles of relevance and rigour in the conduct of fundamental research of complex problems" (Van de Ven and Johnson, 2006: 815) or of "a broadening of the idea of rigour in the context of an applied social science" (Hodgkinson et al., 2001: 545). Unfortunately, these authors do not explain their idea of arbitrage or of broadening the criterion of rigour.

Shani et al. (2008: 540) argue that "truth [about] whether and why taking particular actions and influencing particular dynamics and parameters of the system lead to desired outcomes "is more likely to follow the application of rigorous scientific procedures for creating quasi-experimental studies, even if the studies occur in different systems over time." The authors thus imply that collaborative research in only one organization or in a few organizations is less rigourous than collaborative research extending over a large number of companies, thereby making advances to positivist research. In this vein, the editors of the *Handbook of Collaborative Management Research* distance themselves to some extent from action research: "It is not our intent to denigrate action research or other collaborations that do not adhere to these standards" (Mohrman et al., 2008: 628). The standard that is of special importance here is that "systematic knowledge production processes as researchers contribute to a broader body of knowledge that is accessible beyond the organization itself" (Mohrman et al., 2008: 628). In this way, the criteria of conventional positivist management research are continued though it is conceded that the state of the art in collaborative research has not yet achieved this methodological level. Analyses of single or few cases still prevail. Multi-organizational collaboration projects also seem difficult to establish: how can different organizational communities communicate with each other or are the researchers supposed to manufacture consent across organizations?

Another indication of a trade-off is the desire to "[d]evelop a journal devoted to collaborative management research" (Mohrman et al., 2008: 630). A journal dedicated to a specific field provides a niche that protects against competition from articles of other fields.

And, as Macdonald and Kam (2007: 648) point out, chances of new journals to reach the top tier are dim: „New journals are seen not as quality journals in waiting, but as a disgrace.“

Beer (2001: 60) holds that the ruling evaluation system prevents results from collaborative research to prevail over conventional management research. He complains that the positivistic interpretation of science has “led academics to value research designs in which the researcher is distanced from the subject being researched” and he urges business schools getting aware “how … incentives created by the promotion process discourage professional concern for creating knowledge that meets the test of implementability”. The question is, however, whether proponents of collaborative research accept that their approach “is *not compatible with the criteria of scientific explanation as established by positivist science”* (Susman and Evered 1978: 6001, our emphasis) which implies a minority position.

But how come that the engineering and medical sciences are less afflicted by a rigor-relevance gap? These sciences manage to some extent to incorporate applications of their results and evaluation of the effects of these applications into their system. Medical researchers can experiment with different therapies within the university clinic and engineers can test energy-saving engines on test-stands in their laboratories. In these cases the criterion true/false coincides with the criterion works/does not work. If test conditions work, the underlying theories and methods are considered true. Unfortunately, management researchers cannot take business organizations into a sick-bed or on a test stand. They have to test the effects of independent variables under the control of management on performance from outside the practice system – a highly problematic operation (Kieser and Nicolai, 2005; March and Sutton, 1997).

4. The Usefulness of Productive Irritations between Researchers and Practitioners

From the perspective of system theory, science's basic task is to generate descriptions and analyses of developments and phenomena distinct from the self-descriptions and self-analyses of the systems that are the objects of research. Scientific knowledge should enable *critical reflections* about current practices. If science loses its distance to its research objects, e.g., by collaborating with practitioners or by trying to produce directly applicable practical solutions, it would no longer be able to generate knowledge that is different in principle from the knowledge of competent consultants or practitioners and would no longer be able to fulfill its genuine function (Luhmann 2005b). In this vein, Kimberly (2007: 144) points to "tensions in the relationship between the two parties [practitioners and researchers], tensions that certainly can, if not openly and honestly acknowledged and discussed, lead to the sort of compromises that will diminish quality and that may, as boundaries shift, ultimately compromise researcher and institutional independence." Such a development would finally result in the disappearance of the respective science disciplines. It is worth remembering that, in its early days management science was in danger of losing its legitimacy *because* it lacked distance to practice or lacked rigour. However, the consequence of the insurmountable communication barriers between systems need not be that management researchers lean back and leave practitioners alone – or in the company of consultants who also maintain a specific communication system (Kieser, 2002; Kieser and Wellstein in print; Luhmann 1992a). We think that a fruitful exchange between management researchers and practitioners is possible as long as research is not the intended output.

A first precondition for such a fruitful knowledge exchange is that the systems of practice and science are capable of switching contexts. As we have argued above, a translation of scientific knowledge into practical knowledge is impossible (Seidl, 2007). Practitioners have to be able to understand scientific knowledge up to a certain extent and to re-interpret it in their specific context. Likewise, researchers have to be able to understand to a certain extent practitioners' knowledge. This mutual understanding can be fostered by

„bilingual facilitators“ (Bosch et al. 2001) who resemble „semiotic brokers“ (Tenkasi and Hay, 2008: 68). These facilitators are members of a company who are “familiar with the complex implicit pragmatic rules of the context in which they act” (Bosch et al., 2001: 209). They are thus capable of developing specific transfer (not translation) capabilities “that results from a parallel and intensive familiarity with two different pragmatic contexts” (Bosch et al., 2001: 209). This capability enables these translators to constructively apply utterances that are statements on facts or relationships in one context as metaphors that can take on specific meanings in an other context.

Facilitators should not only be able to speak the language of practice and science but also to transfer schemas between these contexts. They should be able to recognize and transmit *implications* of scientific analysis for practical problems (not implications for practice as constructed by researchers). They should be able to describe a practical situation in such a way that researchers are able to associate one or more relevant science concepts and can provide interpretations that practitioners might find inspiring. Such dual competence is, e.g., created when PhDs in management take jobs in companies but stay involved to some degree in science, or when researchers collaborate for some time in practice projects (Luhmann, 2005b). *Collaborative projects of the type we have in mind do not enable researchers to produce recipes for direct use nor practitioners to produce (more or less) practice-oriented theories.* Rather, bilingual facilitators can help to produce “productive irritations” as self-constructed re-interpretations of scientific knowledge. As Teubner (Teubner 2000: 408, our emphasis) coins it:

> „*Between the discourses, the continuation of meaning is impossible and at the same time necessary*. One discourse cannot but reconstruct the meaning of the other in its own terms and context and at the same time can make use of the meaning material of the other discourse as an external provocation to create internally something new.”

Ideas and concepts from theory can trigger productive processes of reconstruction. The interpretative openness (ambivalence) of theoretical constructs lends itself to such a productive irritation in the practice context. That concepts often take on different meanings in different practice contexts has been observed for management fashions (Benders and Bijsterveld 2000; Benders and van Veen 2001; Engwall et al. 2005; Kieser 1997; Seidl, 2007). In a similar vein, scientific concepts can trigger organizational discourses and thus, via "sensemaking" or "framing", contribute to uncertainty absorption in companies. Framing can be interpreted as a communication strategy „employed to bound and structure an otherwise equivocal phenomenon in more concrete and precise terms" (Tenkasi and Hay, 2008: 66). In contrast, "sensemaking" has to be seen as a "reciprocal dynamic where theory is used as a tool (sic) to make sense of practice, and practice to make sense of theory" (Tenkasi and Hay, 2008: 66).

5. Conclusions

Dozens of articles and books on collaborative research create the impression that management researchers are extending promising efforts towards closing the rigor-relevance-gap. We argued that collaborative *research* is a project doomed to failure. Perhaps the intensity of the discussion and euphonic slogans like "collaborative research", "engaged scholarship" or "evidence-based management" obscure the fact that the system of management research is continuing to perpetuate itself, including the gap and that all the ado about collaboration has not yet generated a single noteworthy truly collaboratively produced research output.

It is typical that all proponents of collaborative research plea for punctuated research interactions with practitioners. No one argues in favour of a permanent role of practitioners in management science (apart from adjunct teaching). A business school that would recruit practitioners *as researchers* would loose reputation. Likewise, a scientific journal recruiting practitioners as editors would run the risk of being perceived as a professional – and no longer as a scientific – journal.

Towards the end of our analysis we pointed out that under specific conditions it is possible that researchers and practitioners productively irritate each other. We assume that just this happens in many of the encounters between practitioners and researchers that are labelled collaborative research projects. We are convinced that the task of specifying the conditions under which management research could inspire practice would greatly benefit from giving up the myth of collaborative *research*.

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