

Collaborate With Practitioners: But Beware of Collaborative Research

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Abstract

Collaborative research is seen as a promising approach for bridging the rigor-relevance gap. In this essay, the authors criticize that the proponents of this approach tend to downplay communication difficulties between practitioners and researchers. The authors apply psycholinguistic concepts of layperson–expert communication and system theory to demonstrate that it is extremely difficult to integrate knowledge that has been generated in the different contexts of science and practice. The authors argue that collaborative approaches like action research, consulting, or executive training are more effective than collaborative research in bridging the rigor-relevance gap. A critical review of reports on collaborative research projects discloses some evidence in support of their assumptions. We conclude by encouraging researchers and practitioners to inspire each other through different forms of collaboration. However, we argue that attempts to turn managers into coresearchers with the special responsibility for making sure that rigor is complemented by relevance are overvalued in the academic discourse.

Keywords

action research, collaborative research, expert-layperson communication, rigor-relevance gap, system theory

Introduction: Does Collaborative Research Help to Bridge the Rigor-Relevance Gap?

Taking medical schools as a model, the founders of business schools intended to create an academic institution, which would create scientific knowledge for the benefit of the profession—the profession of management (Khurana, 2007). The newly created schools did not live up to these expectations. In the first five decades of their existence, they predominantly codified and taught business practices without trying to evaluate or to improve practices on the basis of theory (Augier, March, & Sullivan, 2005). Simon (1957) characterized American business education at these times as “a wasteland of vocationalism that needed to be transformed into science-based professionalism, as medicine and engineering had been transformed a generation or two earlier” (p. 138). At the end of the 1950s, Gordon and Howell (1959) and Pierson (1959), in reports sponsored by the Carnegie Foundation and the Ford Foundation, respectively, criticized business schools for being not useful for practice because of a lack of scientific content. These reports triggered initiatives aiming at the “scientification” of American business schools (Augier et al., 2005; Goodrick, 2002; Schlossman, Sedlak, & Wechsler, 1987). These efforts were successful; business schools became scientific. They created theories, methods, and results based on conceptual or empirical analyses, which they published in scientific journals also created

by them. Unfortunately, practitioners found this research output not very useful for solving their problems. Ironically, now too much, and not too little, scientification at the cost of relevance for practice seems to be the problem (Bennis & O’Toole, 2005; Cheit, 1985). Already in the 1980s, only 20 years after the reports from the foundations, Hayes and Abernathy (1980) criticize the “sophisticated business curriculum” with its preference for “analytic detachment rather than insight that comes from, hands-on experience,” building on wrong management ideas and models that do not respond to managerial needs. An Association to Advance Collegiate Schools of Business–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, Buckley, & Michaelsen, 1994).

From that time on, the rigor-relevance gap has become a prominent and perpetual issue within management science as a look into numerous scholarly discourses (see, for example, Fincham & Clark, 2009; Rynes, 2007), special issues of

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journals (see, for example, Beyer & Trice, 1982; Rynes, Bartunek, & Daft, 2001), or the yearly presidential addresses at Academy of Management meetings reveals. For example, in 2006, Thomas Cummings (2007) 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” (p. 356). He conjured the “relevance ghost” that “continues to haunt us from . . . one presidential address to the next.”

Management researchers are accused of practicing their science in detachment from the real world of managerial practice, to infer their research problems from the scientific discourse instead of practice (Bennis & O’Toole, 2005; Daft & Lewin, 2008; Mintzberg, 2004; Pfeffer & Fong, 2002). Management researchers produce research predominantly in response to other researchers’ research—they persist in “an incestuous closed loop”, as Hambrick (1994, p. 13) diagnoses. To overcome the “double hurdle of scholarly quality and relevance” (Pettigrew, 2001, p. 61), management scholars should rather communicate with practitioners when figuring out research problems, think of practitioners as prospective recipients when conducting research, and take efforts to convince practitioners of the usefulness of their research. Instead, they continue to strive to impress colleagues rather than practitioners.

Suggestions for bridging the gap follow right from this analysis: Management researchers should break out of the loop and expose themselves to the real life of management practice. Knowledge of the real world would enable them to come up with research questions whose pursuit would generate results with relevance for science *and* practice (Amabile et al., 2001; Cohen, 2007; Latham, 2007; Rynes et al., 2001). However, communication between management researchers and practitioners is extremely difficult. Members of these two communities not only speak different languages but they also, in their respective professional work, follow different logics (Astley & Zammuto, 1992; Bartunek, 2007). Thus, as Shapiro, Kirkman, and Courtney (2007) point out, dialogues between researchers and practitioners not only get “lost in translation”—the interlocutors do not understand each other because they speak different languages—but also “before translation”—they base their arguments on different logics.

In spite of these difficulties, collaborative research, more than other forms of contact with practitioners, is seen as ensuring alignment of researchers’ and practitioners’ interests in management research. Collaboration with practitioners, as Van de Ven and Johnson (2006) hold not only “enhances the relevance of research for practice but also contributes significantly to advancing research knowledge” (p. 802). Collaborative research “may serve. . . to produce a type of scholarship that bridges the gap between theory and practice” (Ospina & Dodge, 2005, p. 420). For the editors of the *Handbook of Collaborative Management Research* “the

only effective way to rapidly close the knowledge-relevancy gap is through closer collaboration between the academics and management communities” (Mohrman, Pasmore, Shani, Stymne, & Adler, 2008, p. 626). In spite of all these expectations connected with collaborative research, neither a clear definition of this concept nor agreement on the appropriate modes of implementing it can be found in the literature. The editors of the *Handbook of Collaborative Management Research* state that “[d]ifferent degrees of collaboration are possible” (Pasmore, Stymne, Shani, Mohrman, & Adler, 2008, p. 13) but hasten to add that “[c]ertainly there is some threshold below which collaboration is no longer genuine” (Pasmore et al., 2008; without specifying this threshold). Nor does the concept “impose the requirement of an equal partnership” (p. 12), although “a more equal partnership would be ideal” (Pasmore et al., 2008). These authors see “the joint pursuit of answers to questions of mutual interest through dialogue, experimentation, the review of knowledge, or other means” (Pasmore et al., 2008) as an essential ingredient. Knowledge generated should be “actionable” as well as “scientifically relevant” (Pasmore et al., 2008). Heterogeneous approaches are subsumed under the label “collaborative research.” Articles in the *Handbook* encompass, among other approaches, industry-academic networks (Knights, Alferoff, Starkey, & Tiratsoo, 2008), consulting by “researcher-consultants” (Werr & Greiner, 2008, p. 107), different forms of action research (Bradbury, 2008), PhD programs for practitioners (Adler & Beer, 2008), and executive education (Mirvis, 2008). The editors try to classify these approaches into three categories (Mohrman et al., 2008): (a) “the assembling of networks of practitioners and academics to share perspectives about and collaboratively investigate a particular realm of concern” (Mohrman et al., 2008), (b) “the convening of stakeholders to address a particular system’s need for development through setting up a highly participative inquiry process” (Mohrman et al., 2008) and (c) “collaborative research in which the academic researchers and the practitioners set out to research a problem where their interests intersect, and where the shared purpose is to create knowledge of new organizational/managerial approaches” (pp. 616–617; in an article that appeared in an earlier edited book on collaborative research, Shani, David, & Willson, 2004 distinguish eight approaches). Although approaches within the first of these classifications are not necessarily aiming at a specific output—each side may take home its own idiosyncratic insights—approaches within the other two categories are aiming at the joint production of a shared output. However, in one case (Category 2), the output is a solution to a *practical problem*—a system’s need for development—whereas in the other one a contribution to a *research problem* is expected. In the first case, the collaboration predominantly takes place on practitioners’ turf, in the second one, on the researchers’ turf. It is exactly this

difference that interests us: (a) *Is it realistic to expect that practitioners in collaborative projects aiming at a research output are able to make valuable contributions toward this end?* and (b) *Is it realistic to expect that management researchers in collaborative projects aiming at the solution of practical problems are able to make valuable contributions toward this end?*

We proceed as follows: We first discuss, on the basis of psychological-linguistic studies, problems of lay–expert communication because collaborating practitioners and researchers are, depending on the issue at hand, laypersons or experts to each other. Then we look into communication problems between experts and laypersons in social movements where laypersons as activists and scientists often work closely together. This excursion to other fields of inquiry is necessary because studies on collaborative projects in management say very little on communication problems between the different groups of participants. We then analyze, on the basis of system theory, the extent to which it is possible to organize collaboration between the members of two systems with different logics, such as science and practice. On the basis of this analysis, we arrive at the conclusion that in management research a trade-off between rigor and relevance is unavoidable. We continue by discussing why the scientific community regards forms of collaboration that predominantly aim at solving practical problems like action research or consulting through academics as less attractive. The insights gained in this analysis lead us to a critical review of reports on collaborative research projects in the literature. We conclude by emphasizing that management researchers should engage in collaboration with practitioners but not in collaborative *research*.

Academics and Practitioners Are Experts and Laypersons to Each Other. Can They Communicate Effectively?

Researchers' and practitioners' knowledge bases are different. In communication concerning theory and methodology, the academics are the experts and the practitioners the laypersons. In communication about processes in practice, it is the other way around.

The partners in lay–expert communication cannot evaluate upcoming issues independently from their specific cognitive contexts. A partner assigns meaning to a knowledge element introduced by the other partner by embedding it into his or her respective cognitive frame of reference. The cognitive frame of reference consists of stable elements like prior knowledge, attitudes, convictions, and stereotypes and of dynamic elements like the actual perceptions, situational information, and the course the communication takes.

Communication between experts and laypersons can be called successful if the individual cognitive frames of reference of the partners are brought into congruence to such a degree that their intersection—the *common ground*—is sufficiently large to reach the goal of the communication, for example, an *informed decision* (Bromme, Jucks, & Rambow, 2004; Clark, 2003). In a communication episode, the partners' subjective frames of reference are confronted with each other. One person transmits information that makes sense from his perspective, and the partner then decodes it in a way that makes sense on the basis of his or her frame of reference. If the knowledge elements about which the partners communicate are complex and if knowledge discrepancy is large, the initial common ground is small and in need of enlargement through cooperation information sent to him or her (Clark & Schaefer, 1989). For example, if collaborating practitioners are not familiar with relevant theories or methods, the researchers have to introduce them so that they can discuss alternative choices with the researchers. Likewise, the practitioners have to make the researchers familiar with the problems that they are struggling with in their specific environments.

Communication between the partners increases the common ground and can also lead to its restructuring. Ultimately, it is the joint conviction that a sufficient degree of understanding between the partners has been reached that determines the necessary extent and intensity of the common ground. However, as we will see later, such a joint conviction can be treacherous.

The effectiveness of lay–expert communication to a large extent depends on the ability of the partners to assess what the respective other partner already knows about the issue at hand. This ability rests on processes of perspective changes: the partners have to coordinate their communication inputs with the respective other partner's frame of reference. The absorption of the partner's knowledge into one's own knowledge context is a highly complex and time-consuming process as Flavell (1985) argues:

[T]he fact that you thoroughly understand calculus constitutes an obstacle to your continuously keeping in mind my ignorance of it while trying to explain it to me; you may momentarily realize how hard it is for me but that realization may quietly slip away once you get immersed in your explanation.

Defining vocabularies, applying metaphors and story lines or negotiating joint inputs into artifacts are important activities in this process (Bromme, 2000; Jeffrey, 2003). Management researchers have difficulties imagining that terms like “absorptive capacity,” “mimetic processes,” or “hypothesis” trigger no or completely divergent associations with practitioners.

In their communication, experts have to “unpack” their knowledge from the routinization and contextualization they are used to. Abstract concepts that structure the thinking of partners and predetermine their approaches for solving problems have to be broken down into less abstract concepts. Precisely, those characteristics of one’s knowledge, thinking, and perception that proved advantageous in the past become problematic in the communication with laypersons.

The knowledge of people who are laypersons in a given field is organized in the form of lay theories (Furnham, 1988) that tend to be highly stable. New information is frequently embedded into a defective knowledge structure instead of giving reason for changing the existing structure (Chinn & Brewer, 1998). Lay theories also induce laypersons to ask experts the wrong questions because they are not aware of their knowledge deficiencies. Because lay theories are seldom completely wrong, the experts have difficulties recognizing questions as deficient. The more complex the knowledge of the expert, the more difficult it is to develop a common ground.

Social dimension can aggravate the communication difficulties: For example, laypersons are afraid of appearing stupid or ridiculous and stop asking experts. When experts manage to transfer complex concepts into simple looking and easy to understand concepts, they risk losing their status as experts. Moreover, simplification can be misleading. Time pressure increases all these difficulties. *These barriers in layperson–expert communication make effective communication in collaborative research on research issues extremely unlikely.*

Expert–Layperson Communication in “Participatory Research”

Collaborative projects encounter communication problems. For example, Amabile et al. (2001) report that in a collaborative project conflicts “arose in part from the lack of a common core of research knowledge” (p. 426). Mohrman, Gibson, and Mohrman (2001) argue that “different communities subjectively generate and consume knowledge in terms of their own ‘thought-worlds’” (p. 359). Our analysis in the last section suggests that such communication difficulties can only be overcome to a certain extent. Because there are no studies of collaborative projects in management that present more detailed data on the communication problems, we examine studies from other fields, which are more informative on this point.

Communication problems between laypersons and scientists are often observable in social movements. For example, activists collaborate with scientists in projects for improving environmental health (Brown et al., 2003, 2004; Corburn, 2005), reducing risks of genetic engineering (Kerr, Cunningham-Burley, & Amos, 1998), fighting hazardous

ingredients in personal care products (Houlihan, Brody, & Schwan, 2002), or preventing a risky dam project (McCormick, 2006). This type of collaborative research is also called “street science” (Corburn, 2005) or “citizen science” (Irwin, 1995). It is typically initiated when social movements challenge scientists’ findings on the basis of contradicting local—contextual—data or when movements suggest preventive, precautionary, and contingent actions that are not yet supported by science. The case of 2,4,5-T (a pesticide) is well known (Irwin, 1995). A scientific committee in the United Kingdom concluded that 2,4,5-T was safe to use, subject to the condition that appropriate precautions were taken. However, farm workers argued that because these precautions could not be realized in the day-to-day settings, it was not safe. The experiential knowledge of the farm workers to which the scientists were largely blind represents contextual knowledge that should have been considered in the decision of the committee. The conclusion is that scientists and laypersons with contextual knowledge have to collaborate to generate appropriate decisions on regulations. A successful collaboration of this sort concerns treatment of AIDS (Epstein, 1996): Participation of activists in clinical trials resulted in a redefinition of research problems and in a change of health policy. When laypersons “speak back” to scientists, it is argued, 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, 1999).

Collins and Evans (2002) interpret collaboration between scientists and local experts (with contextual knowledge) as “interaction of two communities of experts, one without certificates” (p. 270). They hold that collaboration, besides “contributory expertise” requires “interactional expertise,” that is, expertise on how to communicate effectively with the respective other community. If only one side is equipped with this sort of expertise, the other party “should be presented by someone with enough interactional expertise to make sure the combination is done with integrity” (Collins & Evans, 2002, p. 256). As Epstein (1995) explains, the AIDS treatment activists “needed to undergo a metamorphosis to become a new species of expert that could speak credibly in the language of the researchers” (p. 417). They acquired this ability by enrolling “a number of statisticians, ethicists, researchers, and governmental officials behind their program” (Epstein, 1995, p. 421). They also acquired interactional expertise by seizing “on pre-existing lines of cleavage within the biomedical mainstream.”

Obviously, participation in projects of this kind always has a political side to it: Activists prefer to work with “sympathetic scientists” (Brown et al., 2003) and activists make themselves knowledgeable with the goal of challenging scientific knowledge that justifies policies they are battling against (McCormick, 2006). In some cases, activists succeed

in convincing scientists to pursue new research questions and to use different data, partly furnished by the movement and thus, create new knowledge that politicians cannot neglect (Epstein, 1996; McCormick, 2006, 2007).

Collaborations of this kind can significantly alter the knowledge that scientists and activists work with. For example, McCormick, Brown, and Zavestoski (2003) found that “[a]ctivists described drastic changes in their expectations what science could prove in terms of environmental causation, their perception of the length of time necessary to conduct research, and the processes involved” (p. 569). However, these activists did not really take on the role of researchers, as one activist commented: “Activists have a seat at the table, not because we want to become scientists, but because we need to push along some of the work that should have been done long ago” (McCormick et al., 2003, p. 571). Activists who collaborate with scientists do not necessarily feel committed to the logic of science. They, for example, do not wait for definite scientific support of their activities (Brown et al., 2004). Collaborating scientists also can perceive role conflicts, as one of them commented

Being affiliated with advocacy work . . . runs counter to the classic scientific process where the scientist approaches data in an impartial way . . . and takes whatever comes out of the data in [a] very balanced unbiased way. (Brown et al., 2004, p. 59)

Kerr, Cunningham-Burley, and Tutton (2007) question the extent to which movements seem to bridge the lay–scientist divide. Studying various dialogues about science, technology, and medicine they found that “lay positions did not tend to challenge expert positions, because participants often presented them as an adjunct, rather than an alternative to expert dominance of discussion and decision-making” (Kerr et al., 2007, p. 407). Considering changes in the “new genetics,” Kerr (2003) concludes that “it would . . . be naïve to assume that present . . . relationships between professionals, publics and genetic are fundamentally different from those of the past” (p. 220). In particular, *the authority of scientists in having the last word in disputes about scientific results is not questioned* (Irwin, 2006).

Mode 2 research is an approach that is said to produce knowledge in the context of application and to achieve transdisciplinarity, organizational diversity, social accountability as well as reflexivity and practicality of solutions as a quality control (MacLean, MacIntosh, & Grant, 2002; Gibbons, 2000). Proponents of Mode 2 research develop their arguments to a great extent on the basis of observations of participatory projects, as their approving reference to cases like the activists’ involvement in AIDS treatment development shows:

The involvement of activists in AIDS research is an example of how the active participation—in this case

of a highly educated group who quickly acquired sufficient medical, biological and statistically relevant knowledge—reshaped the statistical reasoning underlying clinical trials and produces significant changes in how these trials were conducted. (Nowotny, Scott, & Gibbons, 2001, p. 96—that activists enrolled scientists in their team is not mentioned)

Other examples that the proponents of Mode 2 research refer to also demonstrate that contextualization—a key feature—concerns the formulation and reformulation of research questions, changing priorities of funding policies, and the transfer of scientific knowledge to particular contexts but leaves the core research process in the hands of scientists (see also Weingart, 1997). Therefore, we conclude that 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.

Is Collaboration Between Systems With Different Logics Possible?

Linguists hold that experts and laypersons as well as experts of different fields encounter massive difficulties when trying to develop a common ground needed for collaboration. Our analysis of scholars and laypersons collaborating in social movements contributed corroborating results. Laypersons can influence processes of science—sometimes with remarkable consequences—however, they cannot competently carry them out. In this section, we apply Luhmann’s (1995) system theory to gain additional insights into the problems of collaborative research. In contrast to psycholinguistics, Luhmann did not study communication between actual people but sequences of communicative events, which are held together by certain rules and expectation structures (Blühdorn, 2000; Seidl, 2005, 2009). System theory holds that effective *direct* communication between different social systems is not possible.

Thus, communication on research is usually not able to be absorbed into the communication of other systems. As Linda Johanson (2007), Managing ASQ Editor, advises researchers: “The concept of a contribution [to research] is meaningful only in terms of a particular audience of readers and what they already know from previous work” (p. 291). In a similar vein, Grey (2001) argues that in universities “[m]uch knowledge will, quite rightly, only be used by other academics during the process of refining, testing, criticizing and discarding” (p. S29). *If practitioners are not trained as researchers and are not familiar with theories, methods, and the state of the art in the field, research publications do not make sense for them.* These texts not only use

a specific language and refer to specific theories, methods, and previous findings, they also follow a specific logic that is different from the logic on which practitioners base their communication (Kieser & Leiner, 2009; Nicolai, 2004; Rasche & Behnam, 2009; Seidl, 2007). In management research, this logic became institutionalized as a consequence of the process of scientification triggered by the foundations' reports.

Drawing on Merton (1973), Luhmann (1998) conceptualizes the logic of the system of science as self-referential: only scientists are entitled to evaluate and to advance science, and they always have to use theories and methods that are approved as scientific within the scientific community. Contributions of laypersons are only adopted into the scientific discourse if processed by scientists, for example, in their capacity as coresearchers, interviewers, or editors. According to system theory, the meaning of a specific communication is not produced by the sender but by the receiver (Luhmann, 1995). As a consequence, the same words can take on different meanings in different systems. For example, an article published in a scientific management journal will probably receive different interpretations when it is read in a university seminar or a project group consisting of managers. For the managers, the text only makes sense if it can be linked to their specific context. The logic of the system in which the interpretation takes place determines the interpretation.

Scientific communication always follows the code true/false. In a typical scientific article, for example, an author argues that although he or she holds essential parts of a theory and most findings building on that theory for true, some specific findings are, according to his or her analysis, deficient (false), which motivated him or her to replace these false findings with true ones that he or she generated by applying a modified model, a more precise (a "truer") method, or a more appropriate sample. When scientists' communication applies another code, for example, the code useful/not useful for practice, it is typically flagged as communication that builds on science and not as a scientific contribution—for example, as an "implications for practice"—section, a practitioner-oriented article, or a management book.

Doing research is not the business of business organizations. Business organizations are systems for making decisions (Seidl, 2005). Moving from one decision opportunity to the next one, managers generate and communicate decision alternatives. Communication aims to recruit support for certain decision alternatives. The code of communication is "increasing/decreasing organizational success" whereby, depending on the decision, success may be assessed along different criteria. For outsiders, the communication around decision processes is as difficult to decipher as communication between scientists. A lot of background information is explicitly or implicitly referred to the context of the problem,

the outcomes of former decisions that are made responsible for the current problems, former decisions that had been directed at solving the respective problems but had been not or only partially successful, the power structure among the relevant actors, the current situation the organization is in, reflected in performance measures and indices, organizational routines connected to the problem, values contained in the organizational culture, and so on.

According to Luhmann (1992b), business organizations are also self-referential systems as their environments, which include other organizations, have no *direct* influence on their internal operations (Hernes & Bakken, 2003). Their communication is coded along the criterion that contributes/does not contribute to organizational success. Of course, the system of an organization can deal with scientific communication but only if this has been recoded into the system's specific communication mode ("What implications does this research have for our decisions?"). Social systems are operationally closed but structurally open, which means that they can respond to events in their environment. However, the environment is never "given" or obvious. Organizations have to decide what to observe in the environment, how to observe it, and which inferences to draw from their observations. In this way, they also have to decide what to observe in the field of management science, how to observe it, and what to do with the acquired information.

Organizations base decisions on perceived outcomes of prior decisions and assumptions about causality. Because they are unable to operate in a world full of contingencies, they have to keep their picture of the world—their assumptions concerning causalities—simple (Cyert & March, 1963; another cause of the inability to adopt scientific communication with its increasing number of contingencies).

An organization evaluates its decisions after their implementation. Because organizational data are always incomplete, never "objective," and interpretations of decision makers are self-protecting (March, 1994), these evaluations are social constructions. As the evaluation is itself a decision (a decision that a decision was appropriate/not appropriate), it remains within the self-referential communication circle. Therefore, it is impossible for organizations to test the quality of ideas—including ideas imported from management science—by basing decisions on them and observing and evaluating outcomes (Denrell, 2003).

In terms of system theory, collaborative research projects have to be seen as *separate systems* (in analogy to projects in which consultants collaborate with members of the client organization, which are analyzed on the basis of system theory by Mohe & Seidl, 2011). Luhmann (2005a) denotes such systems "contact systems" as they are linked with two other systems, in our case, with the system of science and the system of practice (p. 360). In contrast to its parent systems, the contact system is a temporary system. Moreover, also in

contrast to its parent systems, because their communication is not addressed to a social system but only to the members of the project team, contact systems have to be conceptualized as *interaction systems* consisting of specific persons (not of connected communications). The participants produce communications in a reflective response to the issues brought up by the parent systems. Thus, the communication taking place in a contact system belongs to neither of the “parent systems.” The contact system develops its own logic. However, like the parent systems, the contact system is operationally closed, which means that no direct transfer of meanings is possible between the three systems.

The meaning of “their” communications is ultimately determined by the respective communication system. . . . Each individual communication can only be understood in the context of the system in which it takes place; if it were transferred into a different system, it would constitute a *different* communication. (Mohe & Seidl, 2011, p. 11-12, italics in the original)

Each system entirely determines itself whether there will be a response to message received from another system and how it will look. If the same concepts are discussed in the three systems, each system will attribute its specific interpretation to them. Attempts to interpret a concept along the lines of another system can prompt “refined illusions” (Luhmann, 2005a) or “productive misunderstandings”. In Teubner’s (2000) words

One discourse [i.e. social system] 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 [i.e. social system] as an external provocation to create internally something new. (p. 408)

Of course, the meanings assigned by the three systems to labels, which stand for concepts, need not be completely controversial. Nevertheless, the contact system relates to the problems of the “parent systems” on the basis of its own logic. Moreover, the parent systems will reinterpret the solutions worked out in the contact system also on the basis of their own logics. A perfect consistency of interpretations between the three systems would be pure coincidence.

It is instructive to compare Luhmann’s (1998) concept of operational closure with findings from a collaborative project studied by Mohrman et al. (2001). The researchers (Mohrman et al., 2001) argue that “different communities subjectively generate and consume knowledge in terms of their own ‘thought-worlds’ (unique interpretative repertoires), interpretive conventions, and specific social processes” and that, therefore, to have an impact on practice

academic research “will need to be reconfigured to fit an organization’s meaning system and context” (Mohrman et al., 2001, p. 359). They hold “that perspective taking is required if knowledge from the organizational science community is to be considered alongside and integrated with the knowledge of the practitioner community to determine effective action” (Mohrman et al., 2001). They find that “joint interpretive forums . . . increase the probability that perspective taking will occur” as well as “collective cognitive processes that can set the stage for subsequent action planning” (Mohrman et al., 2001, p. 360).

Although Mohrman et al. (2001) agree with Luhmann (1998) that different social systems apply their own logics—live in different thought worlds when evaluating and generating knowledge—they disagree on the possibility of aligning interpretations. Mohrman et al. (2001) assume that teaming practitioners and management researchers in “joint interpretive forums” brings about perspective taking on both sides, thus enabling the participants from the two camps to understand each other’s interpretations and to arrive at a common interpretation of the usefulness of certain design changes. According to Luhmann, achieving such an extent of cognitive conformity is impossible. Our analysis of the difficulties of communication between experts and laypersons supports Luhmann’s view. In a certain way, the findings of Mohrman et al. (2001) seem to fit Luhmann’s perspective as they could not support their hypotheses relating to the “joint interpretive forum”:

[O]nly 2 of the 12 intervariable correlations between joint interpretive forums and perspective taking were significant. Perhaps the forums that were set up for joint interpretation did not enable the deep reflection and exchange that may be necessary for perspective taking. These joint interpretive forums may have influenced perceived usefulness by creating familiarity and trust rather than by influencing the interpretive schemata of the various parties. (p. 369)

Why Not Action Research or Other More Effective Forms of Collaboration?

Collaborative research “involves practitioners on academics’ terms, in the conduct of research activities in which we are far more expert than they” (Bartunek, 2007, p. 1328). In contrast, action research is not initiated to generate research that might eventually become helpful for solving problems in practice. Instead, the solution of problems in practice has priority. Producing scientific insights is a side effect, though an important one. Knowledge exchanges between the parties involved predominantly follow the logic of practice, not that

of science. The practitioners' interpretations of their actual problems provide the grounds for collaborative knowledge production. The researchers have to make every effort to understand the practitioners. They have to reconstruct the contingencies of the problem from the practitioners' perspective. The practitioners decide whether the scholars' inputs make sense with regard to their problem definition. Of course, discussions with the researchers can lead to revisions in the problem definition but ultimately the organizational members have to implement the collaboratively worked out solution. The scholars' reflections on the process and its outcomes, which are discussed with the practitioners, essentially reframe the outcome in terms of research. It is the scholars' task to present the solution in such a way that other practitioners feel encouraged to experiment with similar solutions in their specific contexts.

However, this emphasis on the production of actionable knowledge has its price: action research is not too successful with regard to the criterion of publication success. As one of its proponents admits: "Action research may be something that the world needs, but it is also something that the world seldom wants" (Gustavsen, 2003, p. 93). Results from action research rarely find their way into more prestigious journals. They are most frequently published in journals that specialize on action research like *Action Research* or *Concepts and Transformation* or in edited volumes. They do not fit into the scheme of mainstream rigorous research.

The criterion of truth, the ultimate criterion of scientific rigor, tends to conflict with the action research criterion of usefulness:

[S]olutions . . . are rarely subject to scholarly scrutiny, and are intended for implementation. Some would go as far as saying that it might not be necessary or useful for such research to be concerned about truth. (Argyris, 2003, p. 425, 446).

Besides action research, there are other forms of collaboration that inform problem solving in practice and are not regarded as rigorous research either, such as consultation by management scholars (Werr & Greiner, 2008), management development interventions (Adler & Beer, 2008), or executive training, which often includes the discussion of actual management problems (Tushman, O'Reilly, Fennollosa, Kleinbaum, & McGrath, 2007).

We hold that action research and other forms of collaborations focusing on the solution of practical problems are, from the perspective of practice, more efficient than collaborative research. Instead of transposing practical problems into scientific ones and generating research that has to find a balance between rigor and relevance and which then has to be applied in processes that generate solutions for practical

problems, researchers bring their knowledge directly to bear in problem-solving processes.

A Critical Look Into Reports on Collaborative Research

Our analysis of the problems of collaborative research can be summarized in the following theses for which we found some support in the literature:

Hypothesis 1: Because practitioners and academics engage in "collaborative research" for different motives and on the basis of different logics, they do not really jointly produce research in collaborative research projects. The academics do research and the practitioners concentrate on activities in their context.

The proponents of collaborative research recommend creating "win-win outcomes" (Hinkin, Holtom, & Klag, 2007; Werr & Greiner, 2008, p. 102) for participants with different interests and motives (Pasmore et al., 2008). Exposure to the "real world" (Werr & Greiner, 2008, p. 105), including the collection of "rich data" and output in the form of publications, presentations at conferences, and dissertations (Knight & Pettigrew, 2007; Knights et al., 2008) are considered the most valuable rewards for academics. However, these are not at all attractive compensations for managers. "[I]t is simply not realistic to expect most managers to invest in joint publications" (Adler & Beer, 2008, p. 552). Managers do not even read scientific management journals (Fry, Walters, & Scheuermann, 1985; Gopinath & Hoffman, 1995; McKenzie, Wright, Ball, & Baron, 2002); why should they be inclined to publish in them? Managers do not perceive themselves as coresearchers. To name them as such implies a "blurring of roles" (Knight & Pettigrew, 2007, p. 6).

Amabile et al. (2001) recommend clearly separating roles and incentives for practitioners and researchers. In forum discussions, researchers should present *their* intermediate results and discuss them with the practitioners. In other words, the researchers do research and then tell practitioners about it! A more intensive collaboration with regard to research also did not take place in a collaborative project that Golden-Biddle et al. (2003) describe. The researchers decided to share results of their analyses with practitioners "along the way." Practitioners and researchers discuss research results produced by researchers or intermediate results; they are not jointly doing research. This finding coincides with one by Rynes and McNatt (1999) who, in a survey of 163 collaborative projects, found that 80% of academic respondents claimed that they had the initial idea for the research and 82% that

they absorbed the majority of the effort and responsibility for conducting the research.

On the basis of a number of collaborative projects they were involved in, Beech, MacIntosh, and MacLean (2010) found that because of communication barriers, genuine collaboration takes place in short intervals although the collaborators remain separated in their roles:

First, the people involved in the dialogue are separated and arranged hierarchically in what are often seen as master/apprentice roles, though the casting of the roles may be contested with each thinking of the other as the apprentice. Secondly, the intentions embedded in the dialogues are mainly mono-directional . . . Thirdly, the orientation and trajectories were that each group conducted its main activities separately, but came together for events during which there was an exchange of inputs. (p. 1352)

If one looks into articles jointly authored by practitioners and scholars, one finds that often the “practitioners,” at the time of publication, are affiliates of universities (see, for example, Bartunek, Foster-Fishman, & Keys, 1996; Bartunek, Lacey, & Wood, 1992), occupy a double role of change agents and researchers (see, for example, Bartunek, Rousseau, Rudolph, & Depalma, 2006; Bartunek, Walsh, & Lacey, 2000), or are consultants (presumably holding a PhD; see, for example, Gill & Hodgkinson, 2007). These articles need not be more relevant for practice than articles authored by “pure” academic authors. In a blind test, they could probably not be identified as belonging to a specific category of publications on management research. How could they? They have to go through the same review process with its emphasis on rigor (Colquitt & Ireland, 2009).

Hypothesis 2: Projects of collaborative research do not generate results that are simultaneously rigorous and relevant. Their output rather reflects a trade-off between rigor and relevance.

Researchers are interested in generalizability of results and practitioners in applicability of results to specific contexts. Practitioners find success stories of individual organizations highly instructive (“Perhaps we should try out this plausible strategy!”), whereas researchers can accept neither statements of success based on only one observation (“We do not have evidence that this strategy is successful in general!”) nor the use of case studies alone as a method for hypothesis generation, not for hypothesis testing (Eisenhardt, 1989). The trade-off between rigor and relevance follows from these differences in the logics of the two systems. The assertion that relevance and rigor can be achieved through the kind of research prevailing in management science is

also in contrast to the observation that the scientification of management science has resulted in a continuously widening rigor-relevance gap, as the increasing frequency of publications on this issue indicates. On closer inspection, however, it becomes clear that some authors who propagate collaborative research concede a trade-off, for example, when speaking of collaborative research as representing an “arbitrage strategy for surpassing the dual hurdles of relevance and rigor in the conduct of fundamental research of complex problems” (Van de Ven & Johnson, 2006, p. 815) or of “a broadening of the idea of rigor in the context of an applied social science” (Hodgkinson, Herriot, & Anderson, 2001, p. 545). It seems though, that the position that management research should be able to take the double hurdles of rigor and relevance in one stride is weakening. For example, in a recent contribution Hodgkinson and Rousseau (2009; their emphasis) argue that it is a “fallacy . . . to place the burden of rigor and relevance on single studies. . . . [I]t is ultimately the body of scientific evidence that must be both rigorous and relevant” (p. 540). In other words, actionable knowledge can be produced independently from rigorous research.

The Usefulness of Productive Friction 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* on current practices. If science loses its distance to its research objects, for example, 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. It would no longer be able to fulfill its genuine function (Luhmann, 2005b). In this vein, Kimberly (2007) 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.(p. 144)

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 rigor. In this vein,

Kilduff and Keleman (2001) doubt that reference to McDonalds' Hamburger University in Illinois "is likely to provide a model for academic research that promotes long-term interests in organizations and society" and conclude "To ask practitioners to play a major role in setting the research agenda is to risk condemning business-school research to a permanent triviality" (p. S56, S58). 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 & Wellstein, 2007; Luhmann, 1992a; Mohe & Seidl, 2011). We think that a fruitful exchange between management researchers and practitioners is possible as long as research is not the intended output (for consulting by academics see Pollitt, 2006).

A first precondition for such a fruitful knowledge exchange is that the systems of practice and science are capable of switching contexts. Such a mutual understanding can be fostered by "bilingual facilitators" (Bosch, Kraetsch, & Renn, 2001) who resemble "semiotic brokers" (Tenkasi & Hay, 2008, p. 68) or participants with "contributory" and "interactional expertise" (Collins & Evans, 2002, p. 256). These facilitators are, for example, members of a company who are "familiar with the complex implicit pragmatic rules of the context in which they act" (Bosch et al., 2001, p. 209). They are thus capable of developing a transfer (not a translation) capability "that results from a parallel and intensive familiarity with two different pragmatic contexts" (Bosch et al., 2001, p. 209). This capability enables these facilitators to constructively transform statements on facts or relationships in one context into metaphors that can take on specific meanings in another context.

Facilitators should be able to speak the languages of practice and science as well as to develop 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 scientific concepts and to provide interpretations that practitioners might find inspiring. Such dual competence is created when, for example, PhDs in management take jobs in companies but stay involved to some degree in science or when researchers engage for some time in practice projects (Luhmann, 2005b). Bilingual facilitators can help to produce "productive friction" as self-constructed reinterpretations of scientific knowledge.

Conclusions

In this article, we showed on the basis of psycholinguistic theories and analysis of the intensive collaboration between

researchers and nonresearchers in social movements as well as system theory, that crucial assumptions of collaborative research do not hold—in particular—the assumptions that practitioners without research competence can collaboratively produce research with scholars, that practitioners are motivated to engage in research, that scientific knowledge can be integrated with practical knowledge, and that a trade-off between rigor and relevance can be avoided.

These results leave us with the question of why concepts like collaborative research or "engaged scholarship" (Van de Ven, 2007) are so popular in the management science discourse on the rigor-relevance gap. Our assumption is that they serve as symbolic labels in a decoupling process. Institutional theory holds that, in decoupling processes, "[g]oals are made ambiguous or vacuous and categorical ends are substituted for technical ends" (Meyer & Rowan, 1977, p. 357). In the discourse on the rigor-relevance gap, management scholars emphasize collaborative research as a powerful gap-bridging approach and, by applying this label to a broad range of divergent activities, keep its definition ambiguous. In this way, 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. Characteristics of the product such as "knowledge which is actionable" are no longer discussed as criteria for obtaining relevance but rather the appropriateness of the process of knowledge production is brought to the forefront. Management scholars thus "provide rational accountings after failures" (Meyer & Rowan, 1977, p. 350). Joining forces with the users of management research in attacking the problem of a lack of relevance carries a high symbolic value for the seriousness of the approach, regardless of its share in the overall research practices.

The intensity of the discussion and euphonic buzzwords like "collaborative research" or "engaged scholarship" might obscure the fact that the system of management research is continuing to perpetuate itself—including the rigor-relevance gap. 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.

Toward the end of our analysis, we pointed out that under specific conditions, researchers and practitioners may be able to productively antagonize each other. We assume that just this happens in many of the encounters between practitioners and researchers that are labeled collaborative research projects. That fruitful mutual friction is what can realistically be expected from collaborative encounters and can also be concluded from observations by Beech et al. (2010):

Shared action is produced in the immediate setting, but in addition resonances take place as each group

returns to its own context, or in subsequent joint activities (or a combination of both). . . . 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. (p. 1364)

The task of specifying the conditions under which management research inspires practice and practice inspires researchers is important. Efforts in this direction would benefit if we could dispose of the notion that the immediate outcomes of these projects have to be *research*. We agree with Styhre (2009) that

Action research efforts [and, as we would like to add, other forms of collaborative research] are not very likely to pose a threat to traditional academic, scholarly research. Instead, action research can be seen as a complementary approach that will reinforce rather than weaken the more traditional field of management research. (p. 30)

We remember that in response to the critique of not being scientific enough for claiming a place at the university, management research imported the logic of science from these fields. Pursuing collaborative research on a broader scale would endanger its identity as a field of genuine research. Medical research is often referred to as an example for management research (see, for example, Bennis & O'Toole, 2005). However, in this field, rigor is not achieved by doing research in collaboration with patients but by systematically testing out new approaches for diagnosis and treatment *on* patients. Of course, patients are much more easily accessible for medical researchers experimenting with new approaches than practitioners for management researchers. Of course, medical researchers who are also physicians are supposed to maintain a dialogue with patients in which they explain their approaches and investigate the patients' needs. Management researchers should not communicate with managers as if they were patients but, as we pointed out above, they should engage into a dialogue that produces productive frictions. More research is needed on how to conduct such a dialogue to make it most productive for both sides.

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