A Review of the Most Cited Literature on Knowledge Processes in Organizations

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Abstract

In the past four decades, much has been written about the processes by which organizations obtain, create, share, transform, apply, and retain knowledge. While in practice these various knowledge processes are tightly connected and embedded, they are studied in different and often disconnected branches of literature. To assess the state of affairs in these various branches this paper analyzes the 52 most cited studies on knowledge processes in organizations. By assessing these studies against today’s review criteria the paper aims to uncover flaws in our frame of reference that we may want to avoid in future studies. The conclusion of this analysis is that we should stop introducing new concepts and reifying them and rather start connecting them to one another and to the practice of knowledge processes in a wide variety of empirical settings.

Key words: Knowledge processes; review; citation analysis; knowledge management; organizational learning; organizational memory
Introduction

For over forty years management scholars study the processes by which organizations obtain, create, share, transform, apply, and retain knowledge. We will refer to these processes as knowledge processes in organizations (KPs). In practice, KPs are tightly connected and embedded in the daily work of organizations. An example is the product development process of an engineering firm. In the course of that process, firms conduct research, discuss the findings of that research, and develop new products based on that research. While this involves knowledge creation, knowledge sharing, and knowledge application, these processes appear as a *Gestalt*, virtually inseparable from one another and from the product development process of the firm. In academia, though, KPs are studied in different and often disconnected branches of literature. While others have already pointed at the reification problem involved (Friedman, Lipshitz, and Popper, 2005; Lane, Koka, and Pathak, 2006), the primary concern of this paper is that there is not a single published study covering KPs comprehensively. This means we have a phenomenon that is tightly connected and embedded in practice that is studied in a fragmented manner.

This paper reflects an attempt to bring these various literatures together and assess their collective state of affairs. To that end the paper analyzes the 52 most cited studies on KPs in organizations. Being highly cited, such studies serve as a common frame of reference to which – for whatever reason – we feel inclined to refer to. As such, these studies should tell us something about the state of the field. We examine them on the basis of five criteria that are used in today’s review procedures of key management journals and conferences. By assessing these studies in the light of our current standards the paper aims to uncover flaws in the work we cite that we may want to avoid in future studies. From this examination we draw several directions for future research on KPs.
Review Method

The term ‘knowledge processes’ refers to the broad range of processes by which organizations deal with knowledge. To define this inclusive term we took the union of the knowledge processes put forward in definitions of knowledge management, organizational learning, and organizational memory. Processes included in these definitions are knowledge creation, storage, retrieval, transfer, and application (Alavi and Leidner, 2001); acquisition, conversion, application, and protection (Gold, Malhotra, and Segars, 2001); knowledge acquisition, information distribution, information interpretation, and organizational memory (Huber, 1991); constructing, organizing, storing, distributing, and applying (Pentland, 1995); acquisition, retention, maintenance, search, and retrieval (Stein and Zwass, 1995); and acquisition, retention, and retrieval (Walsh and Ungson, 1991). Based on the similarities and union of these definitions, knowledge processes are defined here as the processes by which organizations obtain, create, share, transform, apply, and retain knowledge.

With this definition as a starting point a list was made of KPs that have hitherto appeared in leading management journals: *Academy of Management Journal, Academy of Management Review, Administrative Science Quarterly, Journal of Management, Journal of Management Studies, Management Science, MIS Quarterly, Organization Science, Organization Studies*, and *Strategic Management Journal*. Scanning these journals yielded close to a hundred KPs – their various appearances excluded. To identify the core KPs among these and the publications in which they have appeared we used citations as indicator. Reasons for citing include criticizing previous work, paying homage to pioneers, providing background reasoning, self-justifying a point, and habitual citing (Brooks, 1985; Garfield, 1965; MacRoberts and MacRoberts, 1996). As such, citations are not a direct indicator of the quality or impact of a particular publication. Nevertheless, highly cited publications – literally – form a common frame of reference that informs us about the identity of a field.
For analyzing citations three tools are broadly available: Google Scholar (GS), Scopus, and Web of Science (WoS). The latter two are superior for bibliographic citation analysis and searching for specific references (Jacso, 2005). However, their general search function – needed for searching relevant publications on particular KPs – performs not as good (Harzing, 2007). For this review, the relevance of a publication in relation to a particular KP was important. It indicates whether a publication is merely highly cited or whether it is also about that KP. Scopus and WoS do not well capture this. For example, when the results of a general search on ‘knowledge management’ are ranked on relevance, their high-ranked publications are hardly or not at all cited. This does not match the intuitive idea of what is a relevant publication, such as Alavi and Leidner’s (2001) literature review on knowledge management. GS, on the other hand, provided results that are both highly cited and relevant. Another advantage of GS is that it covers books, which can be important reference works. Scopus and WoS, for example, do not index seminal books such as Argyris and Schön (1978), Nonaka and Takeuchi (1995), and Weick (1995). Leaving such books out of the analysis does not match the intuition these are important works. Finally, Scopus and WoS show remarkable omissions. In a search on organizational learning, for example, the papers by March (1991) and Huber (1991) are not listed, despite that they have appeared in ISI-rated journals. For these various reasons we concluded GS was most suitable for this study.

We queried GS for all KPs found in the journals mentioned above. By going through the first fifty results – as of 7 July 2009 – it was established whether a KP was studied within management science or only in other fields and whether or not it was a core process within management science. It was impossible to establish justifiable cutoff points beforehand – especially since GS’s database is growing fast. After initial screening of the results generated, an arbitrary threshold of 5,000 results was chosen to distinguish between core and peripheral KPs. After taking into account the other criteria below, thresholds between 4,000 and 8,000
yielded the same final set of core KPs. The fact that a KP generates more than 5,000 results alone does not yet make it a core KP – it also needs citations. Therefore, an additional requirement was that at least one management publication on a particular KP was highly cited. Though a cutoff point was again hard to set in advance, an arbitrary threshold of at least 100 cites was considered appropriate.

After screening the initial list, 19 KPs and 52 publications remained (see Table 1). KPs that did not meet all criteria and that were therefore excluded are knowledge transformation, knowledge codification, knowledge search, knowledge sourcing, knowledge retention, and many other KPs generating less than 1,000 results. Knowledge reuse was included since it almost matched all criteria. The 52 remaining publications were examined against five criteria that are generally used as criteria for good research. The five criteria were derived from the review instructions and assessment forms provided by the annual meeting of the Academy of Management and the leading management journals mentioned above as well as editorial notes on the review process (Clark, Floyd, and Wright, 2006; Feldman, 2003, 2004; Kilduff, 2006, 2007). Criteria related to clarity of writing and fit with the journal or conference were excluded since these do not concern the contents of the publications. The following five criteria were adopted:

1. **Well-defined theory.** The theory or conceptual framework should be understandable, consistent, and free from ambiguities. All elements should be clearly defined and the relationships between them should be explicitly described.

2. **Convincing justification.** The claims made should be justified by supporting them with valid and reliable empirical evidence, by convincing logic and argumentation, and/or by invoking and building on the appropriate extant literature.
3. **Suitable assumptions.** The assumptions underlying the theorizing and research should be specified to demarcate the scope and boundaries of the theory. Assumptions should be realistic and reflect what is known about organizations in organization theory.

4. **Generalizable conclusions.** The conclusions should apply beyond the particular objects and situations that have been studied. They should be generalizable to theory and/or to a broader empirical domain.

5. **Important contribution.** The study should have implications for research and practice. It should provide an interesting new and meaningful contribution to the management literature in terms of theory, empirical knowledge, or management practice.

In assessing the 52 studies against these criteria we focus on problems and magnify these. Since we do not exhibit the positive aspects of the studies, the review is not a judgment of the quality of the individual papers. Rather, by enlarging problematic issues we explicate where future research should deviate from the way it is done in the studies we refer to most.

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**Results**

**Criterion 1: Well-Defined Theory**

The quality of KP theory is contingent upon how well concepts and relationships are specified. An examination of the 52 publications demonstrates there is not much consistency in terminology. An example is the homonymous use of ‘organizational learning’. While Argyris and Schön (1978) use it to refer to changes in organizational behavior, Huber (1991) and Inkpen (1998) refer to cognitive processes and March (1991) to the mutual learning between individuals and collectives. Also synonyms are used frequently. An example is the transformation of tacit knowledge into explicit knowledge, which is both called ‘articulation’
and ‘externalization’ (Hedlund, 1994; Hendriks, 1999; Nonaka, 1994; Nonaka and Takeuchi, 1995). Also, in these same publications ‘knowledge conversion’ is used interchangeably with ‘knowledge creation’ and ‘appropriation’ with ‘socialization’. A final example is the use of ‘knowledge management’ as a substitute for information management (Davenport, De Long, and Beers, 1997; Hansen, Nohria, and Tierney, 1999).

A second observation is that the reviewed studies hardly explicate how concepts relate. Some relationships between two concepts are discussed, such as organizational learning and organizational memory (Argyris and Schön, 1978), organizational learning and knowledge acquisition (Inkpen, 1998), and knowledge creation and integration (Grant, 1996a). Yet, none of them makes the relationship between a focal concept and the broader field of KPs explicit. Some review studies have brought together several concepts within a particular subfield such as knowledge management or absorptive capacity (Alavi and Leidner, 2001; Lane, Koka, and Pathak, 2006; Zahra and George, 2002). However, none attempts to do this across subfields. As a result, the most cited publications on KPs do not provide a coherent conceptual base.

A final observation concerning KP theory is that some of the reviewed studies contain tautological reasoning. An example is Simonin’s (1999) work on knowledge transfer which concludes tacit knowledge causes knowledge ambiguity. Knowledge ambiguity is defined as the extent to which linkages between actions and outcomes, inputs and outputs, and causes and effects are understood. The reasoning is tautological since tacitness already implies that such linkages are not well understood. Another example is Stein and Zwass (1995), who include effectiveness in their definition of organizational memory: “…the means by which knowledge from the past is brought to bear on present activities, thus resulting in higher or lower levels of effectiveness” (1995: 89). By doing so any research investigating whether organizational memories have an effect on effectiveness becomes tautological. A final example appears in work on absorptive capacity. Cohen and Levinthal (1990) define it as the
ability to value, assimilate and apply knowledge. In the subsequent theorizing it is proposed that a firm’s absorptive capacity affects the extent to which it can assimilate external knowledge. This is tautological since absorptive capacity embraces knowledge assimilation.

**Criterion 2: Convincing Justification**

Whether we should prefer one theory above another depends upon its justification. For all studies – empirical and conceptual – this means the argumentation should be sound and based upon existing knowledge. For empirical studies, it means furthermore that the methods and measurements used should be valid and reliable.

Of the 52 studies, 7 are based on qualitative data and 17 on quantitative data. Furthermore, there are 9 studies with anecdotal evidence, 2 with secondary data, 1 simulation, 1 design study, and 15 studies without empirics. In the qualitative studies we find ethnographic, content analysis, and case studies aimed at exploration and theory development (Dyer and Nobeoka, 2000; Gioia and Chittipeddi, 1991; McLure Wasko and Faraj, 2000; Nonaka and Takeuchi, 1995). Also the studies based on secondary data (Brown and Duguid, 1991; Weick, 1993) and those using more anecdotal data (Davenport, De Long, and Beers, 1997; Hansen, Nohria, and Tierney, 1999) are mostly explorative theory development studies. These studies provide a nuanced view of KPs and their relationships with other variables. For example, Grant (1996a, 1996b) provides a comprehensive conceptualization of knowledge integration, Menon and Varadarajan (1992) of knowledge use, and Markus (2001) of knowledge reuse. Furthermore, it is argued that relationships in organizations involve mutual interdependences and circular feed-back and feed-forward loops (Argyris and Schön, 1978; Senge, 1990; Weick, 1995) and can be curve-linear (March, 1991). This nuanced view contrasts the quantitative studies, which almost exclusively test linear relationships, the majority of which by regressions. Other techniques used are structural equation modeling.
(Simonin, 1999; Yli-Renko, Autio, and Sapienza, 2001), chi-square and t-tests (Mowery, Oxley, and Silverman, 1996), and ordered logit analysis (Appleyard, 1996). With the exception of one longitudinal study (Thomas, Clark, and Gioia, 1993) all quantitative studies are cross-sectional. The testing of linear cause-effect relationships cross-sectionally diverges from the variety of relationships suggested above. This means there is an incongruity between the assumptions and theories developed in conceptual, qualitative, and simulation studies, and those tested in the quantitative studies. This indicates the methods used in the quantitative studies are not adequate for studying KPs.

A second issue concerns the validity of measures. For example, to measure a firm’s absorptive capacity, indicators such as R&D intensity, patents, firm age, and firm size are used (Cohen and Levinthal, 1990; Lane, Koka, and Pathak, 2006; Lane and Lubatkin, 1998). Since absorptive capacity is a latent concept indicators are needed. However, the chosen indicators are so indirect that they do not at all measure the absorption of knowledge. Moreover, the same indicators are used to measure other variables. For example, patent citation has also been used to measure inter-firm knowledge transfer (Mowery, Oxley, and Silverman, 1996) and boundary spanning activities (Rosenkopf and Nerkar, 2001). A striking example of using invalid measures is Tsai (2001). In that study, network position is measured as the number of other units an organizational unit has received knowledge from, absorptive capacity as R&D expenditure divided by sales, business unit innovation as the number of new products introduced in a year divided by the target number, and performance as a unit’s return on investment in a year divided by its target return. Thus, what is measured is the effect of the number of knowledge sources used and the R&D/sales ratio on how well units can estimate their number of new products and their ROI. While this could be relevant, it has little to do with what is claimed.
A further issue is that KPs are mostly measured by their outcomes rather than by what is done. Simonin (1999), for example, claims to study ‘the process of knowledge transfer’. What is measured, though, is the share of knowledge of a partner firm that has been acquired. Comparably, Argote and Ingram (2000) argue knowledge transfer can be measured by changes in knowledge or changes in performance. Similar measures are found in Lyles and Salk (1996), Hansen (2002), Tsai (2002), Schultz (2001) and Yli-Renko et al. (2001). This focus on outcomes rather than on processes implies that a large share of the most cited publications on KPs, in fact, have not studied KPs.

Concerning intellectual justification, studies should be based on convincing argumentation. Not all reviewed studies perform well in this respect. For example, the two most cited studies of organizational learning (Argyris and Schön, 1978; Senge, 1990) promote organizational learning as a solution for many problems in organizations. The message is that organizations should become better at learning and in particular at double-loop learning. There is, however, no justification why learning would always be good. Another example is Nonaka’s (1994; Nonaka and Takeuchi, 1995) distinction between tacit and explicit knowledge and the corresponding theory of knowledge creation. Besides that Nonaka stretches Polanyi’s original (1966) ideas, the justification of why this conversion is key to knowledge creation is meager: “While Polanyi articulates the contents of tacit knowledge in a philosophical context, it is also possible to expand his idea in a more practical direction” (Nonaka, 1994: 16). No more justification is given. Nonaka and Takeuchi (1995) provide illustrations of how conversions between tacit and explicit knowledge occur in organizations. These illustrations though do not justify that conversions between tacit and explicit knowledge are the most important knowledge creation mechanisms.
In building a solid knowledge base there is also a responsibility for readers. Considering how the examples above have been taken up in other studies, it seems the lack of justification is not considered problematic. For example, the relevance of organizational learning has been accepted without questioning it (Huber, 1991) or even by becoming defensive: “…learning needs to be compared with other serious alternatives, not with an ideal of perfection. Processes of choice, bargaining, and selection also make mistakes” (Levitt and March, 1988: 336). Similarly, several highly cited publications have uncritically embraced the idea that knowledge creation happens by conversions between tacit and explicit knowledge (Grant, 1996a; Hansen, Nohria, and Tierney, 1999; Hedlund, 1994; Osterloh and Frey, 2000).

A related issue is that the work we refer to hardly accumulates. Typically, concepts and theories are introduced without weaving them into existing work. A possible exception is the continuous development of the absorptive capacity concept by Cohen and Levinthal (1990), Lane and Lubatkin (1998), and Zahra and George (2002). In the most cited studies in other subfields such development has not emerged. This also indicates that citations do not imply existing work is used. For example, although Levitt and March (1988) and Huber (1991) refer to Argyris and Schön’s (1978) work on organizational learning, they develop their models of organizational learning independently. Another interesting example is March’s (1991) article in *Organization Science*. Typically, it is cited whenever the trade-off between exploration and exploitation appears (e.g., Benner and Tushman, 2003; McGrath, 2001). This suggests March would be the first who wrote about this trade-off or that he has substantially developed it. However, March only summarizes the trade-off as a given from earlier literature for his simulation study – which is rarely referred to in studies that cite his work. This means March’s article is not cited for its contribution but for its summary of the literature in the introduction. Illustrative is that Holmqvist (2004), who writes about exploration and exploitation in organizational learning, does not cite March’s paper.
A final issue is that there is little cross-referencing between disciplines in the reviewed publications. Connections are made to cognitive psychology, particularly in work on organizational learning. However, hardly any connection is made to, for example, the information sciences. Studies of organizational memory (Ackerman, 1998; Stein and Zwass, 1995; Walsh and Ungson, 1991) and knowledge management (Alavi and Leidner, 2001) use the term information retrieval without referring to information seeking models developed years before (e.g., Hardy, 1982; Krikelas, 1983; Taylor, 1968). Similarly, ‘knowledge assimilation’ (Cohen and Levinthal, 1990; Hedlund, 1994; Zahra and George, 2002) and ‘knowledge acquisition’ (Inkpen, 1998; Lyles and Salk, 1996) are used without referring to the corresponding literature in the information sciences. This lack of referencing indicates the work we most refer to does not adopt relevant insights from other disciplines.

**Criterion 3: Suitable Assumptions**

The quality of a study depends on its justification but also on its underlying assumptions. In many reviewed publications, KPs are assumed intentional rational processes meant to reduce uncertainty, variety, and conflict. Examples are that single-loop learning should lead to the removal of errors (Argyris and Schön, 1978), that organizations should strive for shared mental models (Senge, 1990), that non-intentional knowledge sharing happens only through luck or haphazard events (Dyer and Nobeoka, 2000), and that knowledge retrieval takes place by targeted search only (Ackerman, 1998). Such assumptions are at odds with what is long known about human and organizational behavior: it can be unintentional, irrational, uncertainty increasing, divergent, and conflicting. Some studies acknowledge this by arguing that KPs can be unintentional (Huber, 1991; Walsh and Ungson, 1991) and that action can go before sensemaking (Weick, 1995). The fact that these are exceptions though implies the work we refer to most is biased towards intentional rational processes.
An assumption implicitly held is that KPs can only be successful. Above, it was noted that processes are measured and even defined by their outcomes. This makes it impossible to distinguish unsuccessful processes from processes that have not been performed. For example, with Argote and Ingram’s (2000) definition of knowledge transfer mentioned earlier, when the one unit is not affected by the other unit, we cannot know whether effort has been put in transferring knowledge or whether nothing has been done at all. Related is that KPs are typically conceived of as single acts rather than processes. A process, however, is a set of actions that can lead to a particular outcome; it is not the outcome itself nor is it a single act (Szulanski, 2000). Another widely held assumption is that KPs are beneficial for organizations. Only few studies recognize that KPs can also be harmful or without effect. For example, Walsh and Ungson (1991) and Weick (1995) remark that organizational memories can make organizations blind and Huber (1991) argues organizational learning does not always increase effectiveness. Comparably, Stein and Zwass (1995) argue it depends on the function of an organizational memory whether it will be beneficial for an organization or not.

Three further observations about the assumptions on organizations were made. The first is that organizations are mostly assumed to be instruments of managers who can manipulate them as they like. Illustrative are words such as ‘building’, ‘creating’, and ‘design’ in the titles of publications (Davenport, De Long, and Beers, 1997; Dyer and Nobeoka, 2000; Galbraith, 1974; Tushman and Nadler, 1978). Additional examples are Hansen et al. (1999), who assume managers can freely choose between codification and personalization strategies and Senge (1990), who assumes learning organizations can be built. Such assumptions do not take into account decades of organization theory that acknowledges organizations also have characteristics of natural systems whose evolution occurs beyond the reach of managers (Cyert and March, 1963; March and Simon, 1958; Weick, 1969).
Another widely held assumption is that organizations are homogenous systems. In some studies organizations are taken as single units acting as individuals (e.g., Fiol and Lyles, 1985; Levitt and March, 1988). More refined are views on organizations as collections of units (Hansen, 2002; Rosenkopf and Nerkar, 2001; Szulanski, 2000; Tsai, 2002), collections of individuals (Gioia and Chittipeddi, 1991; March, 1991; Weick, 1995), and communities of communities (Brown and Duguid, 1991). In such views, it is assumed organizations consist of components between which interactions take place. The reviewed studies hardly recognize, though, that individuals and units within the organizations are specialized and have different functions – something argued by organization theorists decades ago (Lawrence and Lorsch, 1967). An exception is Markus’ (2001) study of different roles in knowledge reuse.

The third assumption widely present is that organizations are closed systems. In some studies organizations are completely closed. Examples are studies of knowledge management (Alavi and Leidner, 2001) and knowledge transfer (Hansen, 2002), which only consider internal processes. Other studies assume organizations are open to information from the environment but closed in other respects. Examples are studies of knowledge acquisition, (Inkpen, 1998; Lyles and Salk, 1996), boundary spanning (Rosenkopf and Nerkar, 2001; Tushman and Scanlan, 1981), information processing (Galbraith, 1974; Tushman and Nadler, 1978) and absorptive capacity (Cohen and Levinthal, 1990; Zahra and George, 2002). Only few studies assume a more open model of organizations. In those studies it is assumed that organizational members come from outside and that external stakeholders can influence organizations (Gioia and Chittipeddi, 1991), that part of the organizational memory can be located outside an organization (Walsh and Ungson, 1991), and that organizations shape their environments (Weick, 1995).

**Criterion 4: Generalizable Conclusions**
To have broader implications than only to the objects and situations investigated, the conclusions of KP studies should be generalizable to theory and/or to a broader empirical domain. In this respect, the empirical domain to which the reviewed studies generalize may be much narrower than suggested. An example is Dyer and Nobeaka’s (2000) study of knowledge sharing at Toyota. Their results are generalizable to powerful organizations able to dictate a network of interchangeable suppliers. The few such organizations that exist are probably so different that applying Toyota’s approach is not an option. Yet, the conclusions are presented as if broadly applicable to all kinds of organizations. The same applies to Rosenkopf and Nerkar’s (2001) study of boundary spanning activities of the 22 most actively patenting firms in the optical disk industry. While it may provide useful insights on the firms investigated, it is questionable whether these generalize to firms not investigated – even within that same industry – since these are less active in patenting.

These examples point at a more general issue. In the concepts and titles of many reviewed studies, terms are used such as ‘organizational’, ‘inter-organizational’, and ‘organizations’. Such terms suggest that the results apply to organizations in general. The empirical research, however, is almost exclusively done in large, successful private firms in North America, Europe and Japan. Such firms account for less than 1 % of all organizations. One might argue large successful firms are the leader firms that serve as role models for other organizations. This does not hold however, since small firms are not simply little big firms (Welsh, White, and Dowell, 1981). Moreover, successful firms should be different from unsuccessful firms – how otherwise can their success be explained? The general point here is that many studies assume there is an ‘average organization’ to which all research can and should be generalized but that their empirical research is conducted in specific types of organization. The focus on large successful private Western firms involves a strong bias. This bias makes that the generalizability of research on, for example, knowledge acquisition in international joint
ventures (Inkpen, 1998; Lyles and Salk, 1996), knowledge transfer between units of the firm (Hansen, 2002; Tsai, 2001), sense making and sense giving by top managers (Thomas, Clark, and Gioia, 1993), and knowledge flows to and from subsidiaries (Gupta and Govindarajan, 2000) to small firms is questionable at the least.

**Criterion 5: Important Contribution**

A study makes a contribution when something new is found or conceptualized. In this respect, not all highly cited publications seem to make a contribution. An example is Osterloh and Frey (2000) who conclude that intrinsic motivation is important for firms, that crowding effects make both intrinsic and extrinsic motivation important, that some organizational forms are more effective for supporting motivation and knowledge transfer, and that firms might be better able to manage motivation than markets. These conclusions are exactly the assumptions from which the research started, suggesting that nothing new has been found.

A related issue is the challenge of avoiding obvious research. While not formally tautological, the conclusions of some studies are so obvious that we can question whether a contribution has been made. For example, Mowery et al. (1996) conclude knowledge is better transferred in close cooperation than in loose contract-based alliances. Given the difficulties involved in transferring tacit knowledge it is unclear how the opposite could be the case. Similarly, Argote and Ingram (2000) conclude the transfer of a collection of knowledge reservoirs is more difficult than the transfer of single knowledge reservoirs. This is true almost by definition since multiple reservoirs are involved in the transfer of a network of reservoirs. Other examples are Lyles and Salk’s (1996) claim that a firm’s capacity to learn affects how much is learned; and Inkpen’s (1998) claim that knowledge accessibility affects how much is learned. In both cases, it is unclear how such claims could not be true.
It is widely held that management research should have implications for practice and there is broad concern we fail to deliver (Daft and Lewin, 1990; Gulati, 2007; James and Denyer, 2009; Shrivastava, 1987; Vermeulen, 2005). We think that not every study should necessarily provide compelling prescriptions for practice. However, if prescriptions are made, they also should be relevant and justified. Some of the reviewed studies make such prescriptions. It is argued, for example, organizations should be organized according to the dimensions of tacit and explicit knowledge and extrinsic and intrinsic motivation (Osterloh and Frey, 2000); that they should be organized as hypertext organizations enabling knowledge creation (Nonaka, 1991, 1994; Nonaka and Takeuchi, 1995); such that double-loop learning is enabled (Argyris and Schón, 1978, 1996); such that information processing is optimized (Galbraith, 1974; Tushman and Nadler, 1978); or such that personal mastery, mental models, shared vision, team learning, and system thinking is facilitated (Senge, 1990). It may be that following such advice leads to improvements on certain performance indicators. Consider, however, what would happen if organizations would be completely organized according to such advices. Would they only follow Senge’s advices, for example, they end up with an organization where everybody thinks similar and works in perfect harmony. Such organizations would hardly be able to innovate or even sustain. The point with these prescriptions is that in real firms, processes are interconnected and outcomes are multidimensional.

A final issue is how useful the practical implications are to practitioners. For example, Hansen et al. (1999) argue firms with a standardized and mature product relying on explicit knowledge should focus on the documentation and exploitation of knowledge. Similarly, firms with a customized and innovative product relying on tacit knowledge should focus on interpersonal relations and exploration of knowledge. Comparably, Hedlund (1994) argues both division and combination of knowledge are important and Davenport et al. (1997) conclude success factors for knowledge management include a supportive culture and an
appropriate infrastructure. For each of these practical implications we may ask whether reasonably informed practitioners would not already know them.

**Intermediate Conclusion**

The above review demonstrates that the studies which are most cited in the current literature on KPs contain significant flaws on all five criteria (see Table 2). Part of what has been said above may simply be a reminder of what we already know about how to conduct good research. In that respect, we may be guilty of trying to remake some arguments that have already been made quite adequately before – sometimes by the authors themselves when listing the limitations of their studies. Being aware of limitations and mentioning them, though, does not mean they should not be overcome in future studies. As the field’s own organizational learning theory suggests, there should be a constant interaction between what we know and how we act. This means that, if we intend to make progress in studying KPs, we should learn from the past. Based on what we can learn from the flaws found, the next section provides suggestions for future theorizing and research.

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**Future Directions**

Our review has uncovered several flaws in the most cited studies of KPs. In the remainder of this paper we spell out future directions aimed at the avoidance of these flaws in future theorizing and research on KPs. These directions are summarized in Table 3.

**Direction 1: Towards Well-Defined Theory**
Literature reviews such as Fiol and Lyles (1985), Levitt and March (1988), Huber (1991), and Alavi and Leidner (2001) have tried to provide more clarity and consistency in the terminology used by systematically categorizing KPs. Considering the lack of consistency today, they have not yet been very successful. One factor hindering the development of a consistent conceptual base is the use of natural language. While we use homonyms and synonyms to animate our daily language, their use is harmful in academia. Moreover, since natural language is used for the theoretical concepts and the processes in practice, we lack a clear means to distinguish between the two (Tsoukas and Vladimirou, 2001) – resulting in a reification of concepts (Friedman, Lipshitz, and Popper, 2005; Lane, Koka, and Pathak, 2006). Typically, we make clear we mean a concept by referring to a scholar that defined a term. This approach is not very effective because it creates multiple versions of concepts every time it is refined. Also, it connects concepts to individual scholars rather than to a conceptual knowledge base. To provide more clarity, targeted efforts should be made in reaching agreement on the relationships between concepts and observables. Examples are Gourlay (2006b) who has provided more conceptual clarity on the notion of tacit knowledge, Chiva and Alegre (2005) who clarify the relationship between organizational learning and organizational knowledge, and Spender (2008) who scrutinizes the relationship between organizational learning and knowledge management.

Further conceptual clarity can be created when we work towards formalization of the academic language. By applying more formal language – i.e., by using symbols in addition to natural language – we can distinguish more clearly between theoretical concepts and processes in practice and between different concepts. Moreover, formalization also helps to detect inconsistencies, incoherencies, and tautologies that are hard to detect when using natural language (Peli, Bruggeman, Masuch, and O’Nuallain, 1994).
Another factor that may cause ambiguity is that scholars focus on the uniqueness and novelty of their own terms rather than on how these relate to the existing conceptual base. As it stimulates variety and prevents a too early consensus, this approach has advantages in the early phases of a field. Yet, it also hinders the establishment of a solid conceptual base on which we can build further. After four decades of research on KPs it is probably time to change the focus from developing new concepts to the selection of core concepts and the establishment of a common ground. For future studies this implies the creation of new concepts would be minimized and that existing concepts are used and further developed. In addition, specific efforts should be made to develop a hierarchical taxonomy of KPs. By providing a shared frame of reference, such taxonomy can reduce the ambiguity and disconnectedness of terms. Since the field is a decentralized network of scholars it is probably best implemented through a living online repository where individual scholars can contribute concepts.

**Direction 2: Towards Convincing Justification**

The review revealed flaws in the empirical and intellectual justification of the studies. To avoid these flaws in future KP research, several directions can help. First, to develop a clearer distinction between KPs and their outcomes deeper insights into the processes are needed. Since KPs involve human actions, studying them means investigating what people do when they process knowledge. Moreover, since actions are driven by purposes and initiatives, it also means paying attention to why people perform KPs – and why they do not. To do so, more process-based research rather than variance-based research is needed (Van de Ven, 2007). In process-based research it is explicitly recognized that processes are sets of actions that should be studied over time. It is not limited to qualitative studies; process-based research can also be conducted quantitatively by longitudinal studies of people’s behaviors.
Particularly such quantitative research will be helpful to reconnect quantitative research on KPs to qualitative and conceptual work.

Second, the field should broaden its repertoire of quantitative techniques and types of data used. The reviewed studies’ preference for cross-sectionally testing linear relationships and measuring processes by outcomes is still seen in recent studies. Availability, familiarity, and conventions in the field make it tempting to conduct such research. However, with the increased availability of digital data (such as e-mail, agendas, and management information systems) new opportunities have appeared to conduct large-scale research on the actual behavior of people rather than only perceptions in a questionnaire. Not all data will be easily accessible to researchers, for example for privacy reasons. However, we should be able to develop techniques and protocols to capture such data. With the increased digitalization of data, also new analysis techniques are being developed that allow for more data driven exploratory and simulation-based research. An example is Kaufman’s NK(C) modeling (McKelvey and Yuan, 2004; Rivkin, 2000) that could be used to model the co-evolution of KPs. With the emergence of new linguistic and visualization software future quantitative research should move towards conducting analyses that are more in line with the conceptual and qualitative work. The work of McLure Wasko and Faraj (2000) is an example of this.

Third, in accordance with the suggested clarification efforts and repository of KP concepts, we also suggest creating more openness in measurement development and developing a repository of KP measures. An example in psychology is the International Personality Item Pool (http://ipip.ori.org). By building such a repository, transparency of measures is created. This helps future researchers to avoid reinventing measures. Moreover, it also prevents that one indicator (e.g. the number of patents or a firm’s R&D investments) is used to measure several concepts.
Finally, from other disciplines in the social and natural sciences we can observe that selection and retention of concepts is facilitated by a constructive debate and by building further upon previous work. While we do not necessarily advocate the sometimes fierce discussions between scholars in sociology, moving in that direction will benefit the field. In that respect the dialogue-type sections of journals such as *Academy of Management Review* and *Journal of Management Inquiry*, the ‘Point-Counterpoint’ sections in *Journal of Management Studies*, and a critical journal as *Management Learning* are good examples.

**Direction 3: Suitable Assumptions**

The review demonstrated that the most cited studies focus on intentional successful processes, leaving the understanding of non-intentional and unsuccessful processes far less developed. For better understanding KPs and the reasons why they are successful or unsuccessful it is important that such other types of processes are researched more. While there are promising developments in more recent studies of KPs (e.g., Hicks, Nair, and Wilderom, 2009; Tsoukas, 2009), we believe there still is an overemphasis on intentional successful processes, particular in quantitative studies.

One way to better understand the contribution of KPs to organizations is by paying more attention to their strategic value. The reviewed studies have been most concerned with describing KPs operationally, in terms of their effect on knowledge. As such, they have enhanced the understanding of what KPs are, but not so much why these are so important. If we aim to develop a knowledge-based view of organizations, much remains to be done. To that end, it is suggested to take components of the knowledge-based view – such as heterogeneity, value creation, inimitability, causal ambiguity, and sustainability and explore how KPs contribute to these.
We observed a disconnect between organization theory and how organizations were presented in the KP studies. If we embrace organization theory in our studies, this opens up new directions for research on KPs. Concerning openness, it means we expand our research beyond the legal boundaries of organizations (cf. Santos and Eisenhardt, 2005). It also means researching the KPs individuals perform in their specialized roles within and outside the organization. Within organizations it means taking into account the differentiation and specialization between individuals and between organizational units. Outside the organization it means recognizing that being a member of an organization is only one role people play part of the day. Also in their private lives they process knowledge and this affects the organization they work for. When we accept that organizations are not fully designed but also naturally evolving this puts limits to the manageability of KPs. The reviewed studies have focused on processes that can be managed or has assumed these can be managed. Future studies should also investigate to what extent and how KPs evolve naturally and where the limitations of management lie in this respect.

**Direction 4: Generalizable Conclusions**

The assessment of the reviewed studies against the fourth criterion indicated a use of too general terminology compared to the limited empirical base of the research. From these observations we can draw two directions for future research. The first is that we should be more accurate in the claims we make, such that their results at least generalize to those organizations we say they do. Examples in that direction are Gupta and Govindarajan (2000), Lyles and Salk (1996) and Hansen (2002) who explicitly mention that their study applies to, respectively, multinational corporations, international joint ventures, and multi-unit companies.
The second direction is that more research should be conducted on the over 99% of organizations that are not large successful western firms. We should further broaden the empirical base to organizations such as small and medium sized enterprises, municipalities, schools, the rotary, tribes, isolated communities, firms that went bankrupt, corrupted organizations, and firms based on new technologies (see also Dougherty, 2007). An example in that direction is Hemetsberger and Reinhardt’s (2006) study on online communities.

Studying different organizations is beneficial in at least three ways. First, it is likely that the 99% of other organizations need support more than the 1% currently studied. By specifically researching them we can find and address their specific problems. Second, much can be learned from researching a large variety of organizations. By investigating other organizations we will also learn more about large successful western firms, since we can compare the results for the various types of organization. Finally, broadening the empirical base will make the conclusions of research on KPs more widely generalizable. Specifically replication studies in several domains – which have rarely been published so far and are explicitly rejected by some journals – are helpful for that.

Getting access to other types of organizations and getting funding to conduct research may be difficult. On the other hand, governments such as the European Union explicitly solicit more for research on small and unsuccessful firms than on large successful firms. Moreover, examples such as Gioia et al., who study sensemaking in hospitals (Thomas, Clark, and Gioia, 1993) and in a university (Gioia and Chittipeddi, 1991) and Weick (1993) who studies a group of smokejumpers, demonstrate that KPs can be effectively studied in organizations other than large successful firms.

**Direction 5: Important Contribution**
With respect to the final criterion one could argue that research obviousness is inevitably connected to doing research: it simply means empirical confirmation of theories. However, the issue here starts with the questions that are asked. In the examples given earlier, questions and propositions were safely chosen. As a result, the empirical research could hardly anymore provide new insights. To avoid theorizing and research on KPs processes rendering into obviousness, future research should be based on less obvious questions.

Following up on Davis (1971), Weick (1989) compellingly argues there are four basic possible reactions to the outcomes of a study: ‘that’s interesting’ (an assumption of moderate strength is disconfirmed), ‘that’s absurd’ (a strong assumption is disconfirmed), ‘that’s irrelevant’ (no assumption is activated), and ‘that’s obvious’ (a strong assumption is confirmed). Following their suggestions, future research on KPs should challenge assumptions of moderate strength. To specify what is meant by ‘interesting’ Davis outlines twelve types of propositions that challenge assumptions of moderate strength. These concern the organization, composition, abstraction, generalization, stabilization, function, and evaluation of single phenomena and the co-relation, co-existence, co-variation, opposition, and causation of multiple phenomena.

The issues concerning practical implications relate to a particular vision on the link between academia and practice. It is often assumed that practical implications should be compelling prescriptions. However, management science has implications for practice in other ways as well. The concepts we generate make phenomena intelligible and accessible for practitioners that were previously inaccurately or incompletely understood (Prewitt, 1981). Given the double hermeneutic of the social sciences (Giddens, 1987) – by which our concepts and theory become part of everyday language and life – our theories will inevitably impact managerial thought and practice. Therefore, rather than worrying about a lack of impact we should maybe worry about the field’s evident impact (see also Ghoshal, 2005).
Finally, we may also want to extend the types of knowledge we develop in relation to practice. In his role as program chair of the 2008 AOM meeting, Jim Walsh drew attention to three types of knowledge distinguished by Aristotle: *episteme* (general, universal truths), *techne* (specific, context-dependent applied knowledge), and *phronesis* (practical wisdom or prudence, knowing how to act in particular situations). The focus of the reviewed studies has been on *episteme* (in their research contributions) and *techne* (in their practical implications). Given that Aristotle considered *phronesis* as the most important type of knowledge (Flyvbjerg, 2001) it might also be the most relevant knowledge for managers. Taking that into account, future research on KPs should perhaps better facilitate practitioners in developing practical wisdom. Doing that implies that we better take into account other KPs and outcomes than exclusively those selected in our studies and that we think more about the potential negative consequences our advices might have.

--- Insert Table 3 about here ---

**Conclusion**

By assessing the 52 most cited studies on KPs against five criteria used in review procedures, this paper has exposed several flaws in the work on KPs we most refers to. To address these flaws, several directions for future research were suggested. Despite the breadth of these directions, there is a common theme underlying them. The review shows a field in which new concepts are thrown in enthusiastically but without much concern for the existing knowledge base and for the daily practice in which organizations deal with KPs. To improve work on KPs the conclusion of this paper is therefore that we should stop introducing new concepts and reifying them and rather start connecting them to one another and to the practice of knowledge processes in a wide variety of empirical settings.
Executing this is easier said than done. While some directions can be executed by individual researchers, others require the efforts of many, including reviewers and editors. Bounded by the tenure system, disciplinary organization, and journal traditions of the academic system and driven by the human nature to develop routine behaviors, there is a strong tendency for continuing the same way of working. This review indicates though that by making changes in the way we study KPs much can be gained.

This study stands not in isolation. Some of the flaws have been stressed in previous studies on KPs (Alvesson and Kärreman, 2001; Friedman, Lipshitz, and Popper, 2005; Gourlay, 2006a; Perriton, 2009). Moreover, our observations echo critical discussions on the state of affairs in the social sciences and the field of management studies in particular (Ghoshal, 2005; Hill, 2006; Khurana, 2007; Starbuck, 2006). Such discussions show that many of the flaws found in this study are not limited to the field of KPs. By providing specific directions for future research on KPs we hope this paper contributes to those discussions and helps to conduct research on KPs that is more rigorous and relevant.

Of course, this study has limitations as well. Most pressingly, we have reviewed the 52 most cited studies in the field of KPs and not the entire field. As such, it cannot be claimed that the observations apply to the entire field. On average, the reviewed studies are 15 years old and even the three most recent studies are 7 years old already. This means there is a clear time lag between current studies and the most cited studies. Our observations would not have substantial implications if it could be demonstrated that current studies have overcome the flaws found in the most cited studies. However, while several exceptions of course exist, a lot of the flaws mentioned have not been broadly addressed in the mainstream research on KPs.

Because of the breadth of our analysis, we have not been able to pay attention to all the details of the 52 studies. Many of them deserve more in-depth evaluations, in particular with respect to their strong points and contributions. Despite our critical analysis, we appreciate
that these studies have greatly contributed to and shaped the field. By uncovering and magnifying their problems we hope to have facilitated the further theorizing and research on this important topic.

References


http://www.harzing.com/pop_gs.htm
Jacso, P. (2005) 'As We May Search - Comparison of Major Features of the Web of Science, Scopus, and Google Scholar Citation-Based and Citation-Enhanced Databases', Current Science 89(9): 1537-47.


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<tr>
<th>Concepts &amp; Studies</th>
<th>Results &amp; Citations</th>
<th>Concepts &amp; Studies</th>
<th>Results &amp; Citations</th>
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<td>29,800</td>
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<td>(Galbraith, 1974)</td>
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<td>5,928</td>
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<td>(Tushman and Nadler, 1978)</td>
<td>707</td>
<td>(Weick, 1993)</td>
<td>862</td>
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<td>(McLure Wasko and Faraj, 2000)</td>
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<td>(Hansen, Nohria, and Tierney, 1999)</td>
<td>1,924</td>
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<td>(Davenport, De Long, and Beers, 1997)</td>
<td>1,242</td>
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<td>(Hedlund, 1994)</td>
<td>963</td>
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<td>Knowledge acquisition</td>
<td>206,000</td>
<td>Knowledge integration</td>
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<td>(Lyles and Salk, 1996)</td>
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<td>(Grant, 1996a)</td>
<td>1,879</td>
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<td>(Yli-Renko, Autio, and Sapienza, 2001)</td>
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<td>(Inkpen, 1998)</td>
<td>256</td>
<td>Boundary spanning</td>
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<td>Organizational learning</td>
<td>112,000</td>
<td>Organizational memory</td>
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<td>(Senge, 1990)</td>
<td>12,559</td>
<td>(Walsh and Ungson, 1991)</td>
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<td>(Argyris and Schön, 1978)</td>
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<td>(Stein and Zwass, 1995)</td>
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<td>(March, 1991)</td>
<td>4,664</td>
<td>(Ackerman, 1998)</td>
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<td>(Brown and Duguid, 1991)</td>
<td>3,976</td>
<td>Knowledge use</td>
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<td>(Huber, 1991)</td>
<td>3,203</td>
<td>(Menon and Varadarajan, 1992)</td>
<td>308</td>
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<td>(Levitt and March, 1988)</td>
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<td>(Fiol and Lyles, 1985)</td>
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<td>(Storck and Hill, 2000)</td>
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<td>Knowledge sharing</td>
<td>73,600</td>
<td>Knowledge accumulation</td>
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<td>(Dyer and Nobeoka, 2000)</td>
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<td>(Hansen, 2002)</td>
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<td>Knowledge application</td>
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<td>(Hendriks, 1999)</td>
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<td>(Grant, 1996b)</td>
<td>3,751</td>
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<td>(Tsai, 2002)</td>
<td>254</td>
<td>Knowledge flow</td>
<td>8,030</td>
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<td>Knowledge transfer</td>
<td>68,400</td>
<td>(Gupta and Govindarajan, 2000)</td>
<td>1,425</td>
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<td>(Mowery, Oxley, and Silverman, 1996)</td>
<td>1,099</td>
<td>(Appleyard, 1996)</td>
<td>328</td>
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<td>(Argote and Ingram, 2000)</td>
<td>721</td>
<td>(Schultz, 2001)</td>
<td>247</td>
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<td>(Simonin, 1999)</td>
<td>695</td>
<td>Knowledge reuse</td>
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<td>(Tsai, 2001)</td>
<td>568</td>
<td>(Markus, 2001)</td>
<td>341</td>
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<tr>
<td>(Osterloh and Frey, 2000)</td>
<td>551</td>
<td></td>
<td></td>
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<tr>
<td>(Szulanski, 2000)</td>
<td>462</td>
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<tr>
<td>Knowledge creation</td>
<td>60,700</td>
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<tr>
<td>(Nonaka, 1991)</td>
<td>10,703</td>
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<td>(Nonaka, 1994)</td>
<td>5,799</td>
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<tr>
<td>(Nonaka and Takeuchi, 1995)</td>
<td>5,792</td>
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<td>Absorptive capacity</td>
<td>38,400</td>
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<tr>
<td>(Cohen and Levinthal, 1990)</td>
<td>8,262</td>
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<td></td>
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<td>(Lane and Lubatkin, 1998)</td>
<td>1,306</td>
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<td></td>
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<tr>
<td>(Zahra and George, 2002)</td>
<td>1,027</td>
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### Table 2

Flaws in the Most Cited Studies on Knowledge Processes in Organizations

<table>
<thead>
<tr>
<th>Criterion</th>
<th>Issues found</th>
</tr>
</thead>
<tbody>
<tr>
<td>Well-defined theory</td>
<td>Inconsistent and fragmented terminology</td>
</tr>
<tr>
<td></td>
<td>Tautological reasoning</td>
</tr>
<tr>
<td>Convincing justification</td>
<td>Incongruity between quantitative research and other types of research</td>
</tr>
<tr>
<td></td>
<td>Invalid, too indirect measures and processes measured by outcomes</td>
</tr>
<tr>
<td></td>
<td>Unjustified positing of ideas and uncritical adoption of ideas</td>
</tr>
<tr>
<td></td>
<td>Little accumulation of knowledge</td>
</tr>
<tr>
<td></td>
<td>Few connections between different fields of research</td>
</tr>
<tr>
<td>Suitable assumptions</td>
<td>Too narrow scope of processes investigated</td>
</tr>
<tr>
<td></td>
<td>Assumption that processes are always successful and beneficial</td>
</tr>
<tr>
<td></td>
<td>Organizations assumed to be designed, closed, and homogeneous</td>
</tr>
<tr>
<td></td>
<td>Assumptions on organizations not in line with extant organization theory</td>
</tr>
<tr>
<td>Generalizable conclusions</td>
<td>Overly general terminology in concepts and titles, thereby claiming too much</td>
</tr>
<tr>
<td></td>
<td>Limited empirical base, typically large successful Western firms</td>
</tr>
<tr>
<td>Important contribution</td>
<td>Conclusions restate assumptions</td>
</tr>
<tr>
<td></td>
<td>Obvious research resulting from asking overly safe research questions</td>
</tr>
<tr>
<td></td>
<td>Practical implications stated without concern to broader context</td>
</tr>
<tr>
<td></td>
<td>Obvious implications stating what practitioners already know</td>
</tr>
</tbody>
</table>

### Table 3

Directions for Future Research on Knowledge Processes in Organizations

<table>
<thead>
<tr>
<th>Criterion</th>
<th>Directions suggested</th>
</tr>
</thead>
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<tr>
<td>Well-defined theory</td>
<td>Targeted efforts to clarify the relationship between concepts and observables</td>
</tr>
<tr>
<td></td>
<td>Formalization of concepts by using symbols</td>
</tr>
<tr>
<td></td>
<td>Developing a hierarchical taxonomy and repository of concepts</td>
</tr>
<tr>
<td>Convincing justification</td>
<td>Conducting more process-based research</td>
</tr>
<tr>
<td></td>
<td>Expanding the types of data and techniques used for quantitative research</td>
</tr>
<tr>
<td></td>
<td>Create openness in measurement development and a repository of measures</td>
</tr>
<tr>
<td></td>
<td>Reading, questioning, and building upon previous work</td>
</tr>
<tr>
<td>Suitable assumptions</td>
<td>Studying more non-intentional and unsuccessful knowledge processes</td>
</tr>
<tr>
<td></td>
<td>Relating organizational knowledge processes to the knowledge-based view</td>
</tr>
<tr>
<td></td>
<td>Incorporating models of organizations that are in line with organization theory</td>
</tr>
<tr>
<td>Generalizable conclusions</td>
<td>Using more specific terminology in concepts and titles</td>
</tr>
<tr>
<td></td>
<td>Broadening the empirical base by researching a variety of organizations</td>
</tr>
<tr>
<td>Important contribution</td>
<td>Conducting research based on ‘interesting’ questions</td>
</tr>
<tr>
<td></td>
<td>Developing <em>phronesis</em> type of knowledge</td>
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