



COMMENTARY

Gratitude, nostalgia and what now? Knowledge acquisition and learning a decade later

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Abstract

We look at the development of absorptive capacity, organizational learning and IJV research literatures since 1996, using our 1996 *JIBS* paper as our 'centering point'. Taking stock of the timing and patterns of citations to this paper opened up a unique window across time, venues and topics. This permitted us to more clearly position the paper *vis-à-vis* the contemporary corpus of the different literatures that provided the original context and impetus for us in the early 1990s. The paper has been embraced and carried forward by the burgeoning community of international business (IB) scholars on transitional economies and on IJVs. Although the Hungarian setting circa 1989–early 1990s was one we tended to view as a context to address and test theory-based issues and assumptions from a broader management literature, most indications of 'outward' diffusion of our ideas beyond the IB community have been recent. Indeed, over half the citations have occurred after 2003. We project our take on the current landscape toward the future, providing suggestions about research opportunities.

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Introduction

Thank you, Palgrave Macmillan, Arie Lewin, and the Academy of International Business Selection Committee members. It is a great honor to receive such a public recognition.¹ This was our first collaboration, and publication of this article in 1996 made a mark on each of our careers, leaving aside this award. As the first 'test' of how reviewers and scholars would receive our effort to make and actually test empirically a model about organizational learning with primary data, publication of the paper would serve as a milestone marking progress in our research. Additionally, especially for Marjorie Lyles, this was a strong external sign of validation that certainly made obtaining more funding and supporting institutions for her ambitious multi-year, multi-collaboration research agenda possible.

Upon learning we would receive this award, we soon settled down and attempted to more formally reconstruct and reflect upon the project at the time we did it. Doing so has helped provide a baseline for looking at the last decade. *JIBS* published 42 papers in 1996. With a number of excellent and very highly visible papers

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among them, it stimulated a more formal conversation between us about impact and contribution that we wish to share with you. In systematically examining citation patterns for our article, we found that, after a slow start, the paper really picked up steam over the last 2 years. Looking at how articles used our paper, we found considerable variation. However, happily, sadly or indifferently, depending upon your point of view, we find that our paper often buttressed critical aspects of the citing authors' agendas, while the validity and generalizability of our model and findings by carrying this research into other contexts still remains to be done. Given many espoused models of how science is supposed to work, it is interesting that after a decade, and despite visibility, the possible important theoretical and empirical work that might build directly on this paper does not seem to have been done.

Nostalgia and zeitgeist

Zeitgeist

From a broad perspective, themes concerning organizational learning generally (see Organization Science, 1990, special issue) and in joint venture contexts in particular (Hamel *et al.*, 1989; Hamel, 1991) already began an upswing in interest and conceptual development in the late 1980s. There also was much activity in the literature concerning national cultural differences, with more scholarly interest in the impact of culture on IJVs (Lane and Beamish, 1990). It is not a big stretch to say that these topics tended to be long on theory development, with little in the way of research studies. Finally, the fall of the Berlin Wall turned out to be a watershed event that stimulated considerable interest in transitioning economies, although the degree to which this would become a major area of inquiry only became evident once our data collection was under way.

Nostalgia

It was in this context that the project began. Marjorie Lyles received a unique invitation in 1990 to assist the Hungarian government. In 1990 she had collected data on new private ventures in Hungary, from which she could supply reports to the Blue Ribbon Commission. Although exactly when Salk (the rookie) and (seasoned scholar) Lyles first crossed paths is unclear, we both concur that the collaboration commenced during construction of the questionnaire. The questionnaire needed to

accommodate the Commission's needs, while providing a vehicle to expand on earlier work that Marjorie had already published in the area of organization learning (Lyles, 1988; Lyles and Schwenk, 1992). Meanwhile, Jane Salk had recently terminated fieldwork looking at group processes in IJVs (Salk, 1996; Salk and Shenkar, 2001). For her, the survey provided a vehicle to look at how structure affects IJV development and performance, including the way partners were organized, sources and types of (inter)dependence found in the IJV system, cultural differences and member interactions. We submitted the paper for a special issue on alliances. This had appeal, because that fitted the context of the paper as well as our primary research identities.

Major contribution (as seen then)

If we sat back to identify what we thought our article would contribute at the time of publication, we would naturally have to include the IJV arena, in terms of what organizational features, control structures and interfaces across the parents led a local IJV partner to acquire and assimilate knowledge from the foreign partner. However, we drew heavily upon the more general developments in organizational learning, and sought to address some more fundamental knowledge gaps.

Although adapted for the IJV context, we established a testable model of absorptive capacity. This paper was to our knowledge the first to operationalize and test a model of absorptive capacity. We also for the first time could test for mediation effects of knowledge acquired on performance. As stated then:

We establish empirically that the absorptive capacity of IJV organizations (Cohen and Leventhal, 1990) has a strong relationship with both the ability to assimilate (knowledge acquisition) and apply new knowledge (performance). (Lyles and Salk, 1996: 898)

Clearly the need for learning in transitional economic contexts highlighted the importance of our study *in vivo*, although we probably did not fully appreciate how big that field of study would become until close to the time our article was published.

As to the results, we had found that an assumed negative effect of cultural conflicts on IJVs turned out to matter only under particular conditions. The effect of higher levels of conflict was contingent upon ownership structures and controls: for example, a comparison of the conflict measure for

high and low knowledge-acquiring IJVs was not significant (Lyles and Salk, 1996). Although we found that the relationship of knowledge acquired and performance was significant, the strength of that relationship varied across types or dimensions of performance. Moreover, we found that a number of factors either contributed directly to performance and not knowledge acquisition or *vice versa*, strongly suggesting the need for research that could see the degree to which these findings hold across time and contexts.

Lyles and Salk (1996) over the last decade (as seen now)

Curiosity led us to take a more systematic look at which articles cited our work. We chose the *Social Science Citation Index* (under *Web of Science*) and found a list of 67 citations as of August 2006. This of course has some limitations in that it does not pick up certain specialty journals, journals with a particular linguistic, geographical following, books, and so forth. Figure 1 shows the plot of these citations since 1996. There is a clear upward annual trend since 2003 in the number of articles citing Lyles and Salk (1996).

We were also curious to see the timing of the citations, and what aspects of our article were used. The articles in *JIBS* that cited our work were mostly expanding on research on alliances and emerging economies (Meyer, 2004; Meyer and Peng, 2005). The remainder of citations spread out over a rather broad array of topic areas. These include organizational learning and resource-based theories, parental control and other antecedents to knowledge transfer, knowledge spillovers, and relational issues in alliances and joint ventures (e.g., Wong and Ellis, 2002; Minbaeva *et al.*, 2003; Simonin, 2004; Zhang *et al.*, 2003). Simonin's work in particular expanded the theoretical development of the processes of

organizational learning and learning capabilities. Some articles citing our article addressed cross-cultural differences and headquarters–subsidiary knowledge flow.

The next highest number of references to Lyles and Salk was in the *Strategic Management Journal*, where the topics differed a bit from those in *JIBS*. These articles address the impact of the country effects on the success of units or firms. Thus Kriauciunas and Kale (2006) address how imprinting impacts know-how of firms in emerging economies. Makino *et al.* (2004) address country effects on variation in the performance of affiliates, and Tsang (2002) addresses directly knowledge acquisition gained from joint venturing experience.

What we gleaned from this is that many authors have so far used Lyles and Salk (1996) as a supportive reference for their own work, and much of that research lies in the alliance and emerging economies literature. What we also found was that, in the most recent years, a number of the citing articles are being published in areas tangential to the earlier papers. Few studies, besides the continuing work of Lyles in her extensions of the study (Steensma and Lyles, 2000; Lane *et al.*, 2001; Dhanaraj *et al.*, 2004; Barden *et al.*, 2005; Steensma *et al.*, 2005), attempted to directly adopt our variables or our model of knowledge transfer, or replicated our results in other emerging economies. There are exceptions. For example, one excellent paper by Beamish and Berdrow (2003) is in *Long Range Planning* and looks at mapping out knowledge opportunities between joint venture partners. Contrary to our results they find no direct relationship between learning and performance.

Impact: That obscure object of desire?

For us, somewhat unexpectedly, the most interesting patterns have to do with the temporal pattern in Figure 1 and, additionally, where we do not seem to be cited. To fully appreciate why these are interesting, it is worth stepping back to think about how contribution and impact can be theorized and predicted to operate. Citations often are used as a proxy for impact and contribution (by promotion and tenure committees as well as in research). If we look at academics as participating in scholarly communities (Crane, 1972; Abrahamson, 1991; Abrahamson and Fairchild, 1999; Brown and Duguid, 2001), this view of scientific practice as embedded in a social community sees a field as shaped over time by the social processes and constructions of its members. From this

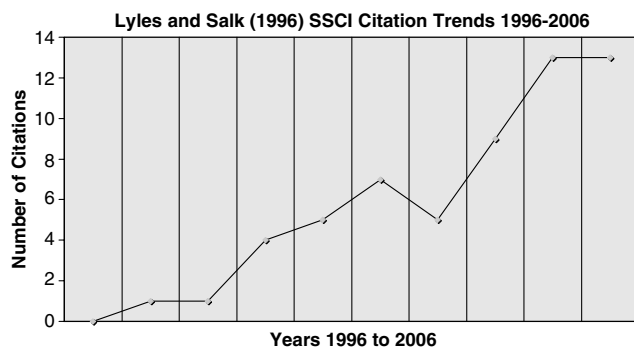


Figure 1 Lyles and Salk (1996) SSCI citation trends 1996–2006

perspective, those in powerful positions – editors, reviewers, etc. – and the norms of the review process, combined with resource scarcity (number of articles that can be accepted) suggest that being accepted for publication in an important outlet such as *JIBS*, in itself, legitimates all published papers as contributions.

If all papers ‘contribute’, then what is impact? Definitions vary, but it seems that a core aspect of impact surrounds the ‘noticing of a scholarly work and its incorporation into the shaping of knowledge and debates in subsequent research’. Murray Davis (1971) in his PhD seminar classic ‘That’s Interesting’ incorporates time into choosing impact or, as he calls it, ‘influential scholarship’. This sample was chosen from the ranks of the great sociologists whose reputations and works have stood the test of time (Marks, Durkheim, etc.) and Davis suggests that impact might be seen as related to time for which a given publication scholarship continues to be read. Meanwhile the sociologist Robert K. Merton (1965) suggests a notion of science that still dominates in our ranks (taking a metaphor attributed to Isaac Newton) as ‘building upon the shoulders of giants’. An article with impact will extend past scholarship and will lead to further research that eventually expands knowledge. Hence it is unclear whether to count referencing over time, or look at where a citation appeared and what it contributed, which would typically entail some notions of obsolescence.

Returning to Figure 1, our paper hardly ‘took off’ immediately. The figure suggests that it is becoming more noticed and perceived as relevant today. Meanwhile, Steensma and Lyles (2000) and Lane *et al.* (2001) are studies that used data from a resurvey of the same population of Hungarian IJVs three years later and enabled the authors to refine the initial knowledge acquisition model and to empirically address matters of process and time dependence that could not be incorporated in the 1996 study. Although these articles resulted in refinements and changes to the initial model, and deepened understanding of time and context dependence, these are not in any obvious way influencing the degree to which Lyles and Salk is receiving citations, still more ‘noticed’, and in that sense relevant to current work.

Hence in terms of impact, variously defined, the citation contexts concur with the impression that much of what can be mined to expand and extend current knowledge still awaits prospectors, both in domains where we are known, as well as in the

more general managerial literature on organizational learning. We seem to have appeal in building and buttressing others’ topics and ideas, although there is little notice of building on the core of the paper.

Discussion and conclusions

From citation patterns and search for the areas and ways in which Lyles and Salk (1996) has had an impact upon the field, a picture of scholarly activity emerges that potentially has broader implications for research on inter-organizational knowledge acquisition and inter-organizational learning. If impact entails continuity, with a community agenda of refining theory and empirical findings to create a more holistic account of what is known, perhaps 10 years has not sufficed; or the notion of systematic expansion of a body of knowledge based upon past scholarship does not operate in sync with temporal citation patterns? This would be a worthy topic for follow-up research, but lies outside the purview of this paper.

What we can reasonably conclude (even if the mechanisms are unclear) is that, at least in socially constructed terms, Lyles and Salk (1996) remains active in a contemporary or current corpus of knowledge in IJV and transitional economies research and inter-organizational research, often closely related to strategy but mostly in the IB field. That said, this suggests that more replication and extension of the basic model and empirical aspects remains worthwhile for future research. For example, while the other articles arising from the stream of work connected to Marjorie Lyles’ study extended the testing and refinement of our 1996 article across time, it remains for scholars to use this model in contexts besides IJVs and transitional economies. This is an example of where empirical findings and theory building done in the international context might serve as a basis for research and theory building outside the international domain, as so many scholars underscore as a potential strength of IB (Toyne and Nigh, 1997; Shenkar, 2004).

We find that there is still a critical need for scholars to attempt to bring more commensurability to research by attempting to build upon prior models and definitions of knowledge transfer, absorptive capacity, and the processes thought to underlie them. As it stands, we do not see the field as having ‘matured’ to the extent we might have wished to see, looking back over 10 years. We are not whining about the fate of our work here, nor do

we exempt ourselves from acting to shape this more general pattern. That clarified, the more general theoretical and operational disconnects that we find in the organization learning and knowledge transfer literatures could not be pushed to the periphery of our attention. A recently published meta-analysis of research on transaction costs economics (David and Han, 2004) suggests that this might be a more general issue in contemporary management studies. In their analysis of TCE, its key constructs and corpus of findings, they found that few studies build upon prior definitions of key terms, proxies and measurements in a way that would allow for greater convergence over time. This 'reinventing of the wheel' has led to divergent findings that might largely be an artifact of the lack of commonly accepted definitions, measures and methods. This seems to be an issue for inter-organizational learning and knowledge transfer research as well, and suggests that there is still substantial opportunity to make contributions to the field by extending research and theory.

Lyles and Salk (1996) was explicitly set up with a clear sense of the urgency for process-based research. While our later research, including Lane *et al.* (2001), picks up and refines a process viewpoint inherent in the original methods and study design, the knowledge transfer and organizational learning literatures continue to rely upon relatively static or mechanistic approaches. Future research needs to develop common (or at least

relatively commensurate) definitions and provide some sense of cumulative knowledge about organizational learning. Ultimately, a better theoretical and empirical documentation of process is, we assert, essential in ripening and maturing knowledge in this area. Hence the need for more process-based research and exploratory research (Parkhe, 1993; Doz 1996) is as critical today as it was a decade ago.

In conclusion, although Lyles and Salk (1996) has been fortunate in receiving attention in the form of citations and, most recently of course, in receiving the Decade Award. Meanwhile, a decade after its publication, it contains a number of unresearched or under-researched topics, and issues – theoretical and methodological – where scholars ferreting out these opportunities can still have a large impact on the field(s) touched by the paper. Will we see a different story in another five years?

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Note

¹I must, however, confess that the 'positive introspection' was preceded in my case by the temporal tap on the shoulder reminding me that a decade passes by very quickly! (JS)

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