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A memoir and reflection: knowledge and an evolutionary theory of the multinational firm 10 years later

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Abstract
The production of the 1993 article awarded the JIBS Decade Award was written during a time when ideas regarding knowledge and the international expansion of the firm confronted a hostile audience. The sources of these ideas were directly related in Winter's and Roger's prior work, but also to a broader literature on 'category errors' and technology transfer. The theory of the firm as a social community is a distinctly sociological theory and, though sharing many key ideas with the resource-based view that developed at the same time, is deeply opposed to engineering conceptions of firms as Lego-modular pieces that can be easily shifted, bought, and sold. We describe our individual biographies and the subsequent intellectual development of the concepts in our article.
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Introduction

The JIBS Decade Award is a great honor for an article that was accepted at an important time in our careers. Its publication culminated a short but intensive review process that was very deftly guided by Paul Beamish, the editor of JIBS at that time, and to whom we remain most grateful. We had two distinct ambitions in the article. The first was to show that work on the concept of knowledge could be *empirical* and *measured* by reasonable constructs that can be replicated, and hence rejected, accepted, or improved. The second was to *balance* the at the time overwhelming emphasis on transactions cost economics as an explanation for direct investment and as a theory of the firm with a perspective that allowed for a wider, more humanistic understanding of human motivation in the context of social communities.

This article, and others we wrote, is often seen as a rejection of transaction cost economics and the institutional resolution of incentive incompatibility. We surely saw our work as criticizing this perspective of economic behavior as exaggerated in importance, as exemplified by the following quote from our letter to one of the reviewers:

> We would also certainly admit that transaction costs exist, and a firm may engage in an activity due to opportunistic behavior. At the same time, we would

argue that this explanation has been greatly exaggerated as the only way by which to explain institutional choice.

At the same time, we adopted the empirical philosophy established by Oliver Williamson (1981) by whom we were much influenced. Our work collected 'micro-analytic' data and we sought to provide measures of knowledge that could parallel the methodological constructs used to measure 'transactions costs'. We recognized that a theory is far more powerful if it provides a pair of methodological rails by which future empirical work may more easily travel.

We especially welcomed the publication, as we had faced a rather torturous 4-year process in publishing our first piece on knowledge in Organization Science in Kogut and Zander (1992). That article, called 'Knowledge of the firm, Combinative Capabilities, and the International Transfer of Technology', was number 16 in the Organization Science records. We had trouble in many places, including the criticism of our reporting of field research as failing to meet scientific standards; these cases in the end had to come out. Its eventual acceptance was an editor's decision to intervene in our favor; the editor was Arie Lewin. By the time it was accepted, the paper showed all the signs of a tough review process. We were pleased that we were allowed to edit it extensively for final publication that appeared 4 years after its submission.

At the same time of writing the theory piece, we designed and implemented a questionnaire that resulted in a working paper by 1990. The first version of the 1990 paper was submitted also to Organization Science and it was meant to parallel the theory article. It was rejected, with criticism of measures and little discussion of the importance of the overall exercise. It seemed only to infuriate our reviewers more that we showed algebraically why our measures were sound. This rejected article was picked up by a special issue on 'European Research' for Organization Science, was still under review in 1993, and only to be published in Zander and Kogut (1995). Its publication did not look promising at the time we submitted the JIBS article.

Fortunately, some of this effort was reported in the original thesis of Udo who focused on a question that had preoccupied him for some time regarding voluntary and involuntary international dissemination of cutting edge technology[1]. Still, we had the sensation of reliving a nightmare when the early reviews of our article submitted to JIBS were rather ferocious. The back-and-forth was quite intense. To give you a flavor of the sometimes-heated exchanges during the review process, we have saved some representative answers to reviewers and the editor for posterity in Appendix A. However, Paul Beamish chose to publish the article and let the argument spill over into a subsequent published debate in JIBS. He could have made a different decision.

Prior to its publication, the JIBS article failed to win the Haynes Prize at the Academy of International Business meetings held in Brussels, but it was a runner-up. The committee, chaired by John Dunning, did not provide any opinion. We were thus pessimistic regarding its acceptance by our colleagues. We received a few private and published compliments on the paper, which were gratifying. Over time, we distilled a drift in the work of those who were not well disposed in the direction of knowledge in the first place. Of course, we were clear to note in our article that the international field consisted already of very original treatments of knowledge, such as in the writings of Jean Francois Hennart (1982) and in the forever-remarkable 'book' of Peter Buckley and Casson (1976).

All of the above comments lead to a simple conclusion: the presentation of the JIBS Decade Award could not be easily discerned in the tea leaves analyzed from the reviews or the paper's reception by our colleagues. If this article has come to be recognized, it may be because it reflects the great excitement that we felt in thinking through our ideas, working out the theory, and designing the empirical studies. Our articles were part and parcel of the evolution of our personal relationship with each other and our careers, which were at very different points when we started to work.

Since we began at different points, it might be of interest to understand what the two of us brought to the table and how the writing of these articles meant originally different things to us as well as to our careers:

Bruce Kogut

We wrote these articles after having met in Stockholm when I took a leave of absence from the Wharton School. The academic year of 1987 and 1988 was a very happy and

Udo Zander

Go West young man

California once again proved its power to challenge established ways of thinking and doing. Within 15 min

productive period, following a slow beginning of an academic career. Monika and I arrived in August and rented a bright wood-floor apartment on Tomtebogatan ('little elf's home street'). Our daughter Emily was born in September in Danderyd. The Institute of International Business provided a chance to teach and then to get home easily to see what happened that day. Winter comes early to Sweden, and it was a winter filled with ample snow. Emily's carriage was built like a Volvo and made its way over soft ice and down steel railings into the subway. On one bright Sunday day, we went for a walk with friends on the frozen lake of Mälaren in central Stockholm. With no trees and buildings, the ice is a Breughel canvas, filled only with the strolling, skating, playing of hundreds of people.

This year was a needed break from Wharton. The Department of Management had yet to consolidate into the research community that came to exist later. The international group was lead by an extraordinary individual, Russ Root, who provided solid support in a confusing environment. The department had been sued for sexual discrimination (the first of what became a series of such disputes) and the case went to the Supreme Court; yet, there was almost total institutional silence over the issues, or admission that there was an issue. A chair of the department called in assistant faculty to provide references to his papers, or to provide advice on consulting in preparation for a meeting with the client the next day. Searches for chairs resulted in brutal and unfair procedures; in one occasion, a senior offer was made to an individual despite established evidence regarding plagiarism – hardly reassuring to young researchers.

My research was going forward, but had yet to be much published. My thesis on East Germany failed to provoke interest, and my early articles on the value chain, rent-generating assets, and options were making more an impact in applied journals and on practitioners than in research circles. Mike Porter had published on the value chain at the same time and he was writing on international strategy; the market share of ideas seemed more like a business than academics. I was uneasy writing more in this area, as I wanted to be a researcher in an academic community.

The early efforts did not always work out. I had written a long working paper, published as a Reginald H Jones Center working paper, on transaction costs. A remarkable colleague, Graham Astley, read it and, as usual, provided in depth comments summarized in one line: it was all critique, no resolution. I had been fixated on a graph that Williamson published in an American Journal of Sociology article in 1981 that showed a firm consisting of boxes representing activities (e.g. marketing) that it contracts, boxes that it decides to internalize because of transactional dilemmas, and boxes representing its economic advantages. I was interested in understanding why a firm would have an advantage, and I had only vague ideas and musings about the Selznick concept of distinctive competence.

On the way home from Wharton – I lived in Powelton Village a lively district in West Philadelphia that was still then quite early in redevelopment, I ran across a woman who was screaming and swearing on the top of her lungs. The thought flashed across my mind, 'that's how I feel' I said to myself, and concluded she must be a former assistant professor at the department of management.

from my arrival at the gates of Golden Gate Park north of Berkeley, I had been let into the closed Racecourse at 6 am by a friendly gatekeeper, had met with a leading trainer of thoroughbreds and been offered to immediately get on one of his racehorses for a workout on the dirt track. Some of his stable-hands helped me out and later became friends and invited me to visit Napa Valley vineyards where they also worked. For a young, very shy Swede on a Fulbright Scholarship who had at best hoped to see the track from the outside and possibly ask someone when it would be open, the experience of finding so much interest from others for his passion and an urge to learn from his foreign knowledge was a surprising eye-opener. My life was suddenly so easy. People came to me and talked and were interested in what I brought to the table.

The same story repeated itself at Berkeley. Glenn Carroll was the friendly man at the gate making me feel welcome and listening to my stories of European multinational companies (MNCs) and their strategies and organization during long sessions in his Barrows Hall office. He also introduced me to organizational sociology in general, and population ecology and research design in particular. The great intellects of Glenn and other Berkeley faculty like Charles O'Reilly, Barry Staw, and David Vogel inspired me. Ideas picked up in class were constantly discussed in the illustrious group of Ph.D. candidates including Bill Barnett, Heather Haveman, Will Mitchell, and Anand Swaminathan. In the process we did have a lot of fun, and a Swedish contribution to the social hours was the installation of a locker in the Ph.D. lounge to improve beer-hauling logistics. An exchange program with Stanford allowed me to enter the fascinating worlds of John Meyer, Dick Scott, and Jim March. The stream of great scholars passing through Berkeley was also endless and their eagerness to philosophize with the willing was truly astonishing. Their passion and urge to discuss theories and findings often carried the seminar discussions into long hours at local restaurants around Telegraph Avenue.

In general, my economics and business administration background was challenged and enriched, and I started realizing that the sociological grounding was something that I would have needed during my earlier extended field studies of headquarter-subsidiary relationships and internationally dispersed R&D activities in MNCs (see Håkanson and Zander (1986). The sociological viewpoint resounded well with my 'over-socialized' European bias for understanding social organization and stratification of society and the often subtle and sophisticated ways to uphold culturally very different systems. I still remember being baffled when reading Oliver Williamson's (1975) statement 'In the beginning there were markets…' and wondering what his great book would have looked like if he had started off with hierarchies as the original state. The argument of course would become even more interesting if hierarchies were not such a fundamental state of nature, as suggested by my main advisor back in Stockholm, Gunnar Hedlund (1986), who kept writing about the hypermodern MNC as a 'heterarchy'. During the many courses in organizational sociology, my long-term interest in and debate with Gunnar over European guilds and apprenticeship systems and their transfer of knowledge eventually found a theoretical home.

That night, Monika and I went out for dinner, unaware that Emily was already a growing reality. We talked and decided that I would ask for a leave of absence, which was granted shortly afterwards, but with no stop in the tenure clock.

The Institute of International Business was located in a basement above the Stockholm School of Economics. The director Gunnar Hedlund was on leave that year; Lars Håkanson was the acting director; the place was run then, as now, by the tolerant smiles of a very able staff who provided help, coffee, and friendship to the many doctoral students who populated the caves. There was a rectangular seminar room with an oval table surrounded by chairs that required mental exertion to figure out how to use them and what parts of the body should be placed where. It was around this table that Udo and I did a lot of our best work. By the end of my stay, Udo and I had a working paper on knowledge and started the empirical design and questionnaire – which Udo implemented alone. In addition, I had completed the first paper on joint ventures and stability and Nalin Kulatilaka and I wrote a Jones Center working paper on direct investment and options (which did not appear until 1994 in Management Science).

The leave of absence, which had seemed risky, turned out to be the pivotal year in my professional life. If it was fruitful, much of this had to do with the fun that Udo and I had in drawing up this research. He was freshly back from Berkeley and Stanford, with boxes of notes from courses with Jim March, Glenn Carroll, and Dick Scott, along with papers written for a lecture series organized by David Teece (1985); one of these papers was written by Sid Winter (1987).

The paper made an immediate impact on me, as it had on Udo. I had already speculated in the review article on joint ventures that was published in the Strategic Management Journal that organizational learning and tacit knowledge represented a theory of the firm distinct from transaction costs or from market positioning. I wanted to develop a better articulation of this point, as well as empirical work. In particular, I was thinking about a paper by Gordon Walker and David Weber and how they built constructs from data. I also thought of my discussions with Erin Anderson and mused on her observation: econometrics is useful when you have bad data, so why not start off with good data. I thought she was right, but didn't know just then how could this be done.

I don't remember the exact time Udo and I met, or how we started to discuss these issues. I recall he was always happy and filled with ideas and like me, always willing to have a beer at the end of the day. He was interested in innovation and its diffusion and the work of Rogers. I was interested in knowledge, why firms differed in productivity and best practices, and why joint ventures provided an organizational way to transfer knowledge. This was the starting point of our discussions. We had a year in the basement of Holländargatan, watching days grow ever shorter into eternal night and then ever longer into eternal day, to develop our ideas in silence.

The constant theoretical Berkeley debates on evolution, selection, variation, and retention also lead to empirical observations. Coming from a sparsely populated country near the North Pole, I started reflected on the selection-based logic of the American system I saw around me. An example was the infamous Berkeley OB-group job talks we were allowed to sit in on. In Sweden, my feeling was, we could not afford to let anyone stay behind since his or her particular talent would most probably be needed for group survival at some point. Here I found myself in a world where it was possible to choose and develop people without necessarily having everyone else on board, which was very refreshing (but also a little scary). It turned out that I found the highly selective environment extremely stimulating and energizing. I was constantly testing my ideas for a dissertation and at the same time my own abilities as an intellectual. With my European Schumpeterian beliefs about the nature of capitalism and my interest in international business, I ironically again and again saw international forces creatively destroy my ideas for a dissertation topic. Plans to study the possibility of purely project-based multinational organizations, developing an institutional theory of multinationals, examining structural inertia, ownership, and clan structure in MNCs, and studying the prospect for MNCs to employ their organizational and logistics skills to help the world population in crisis situations were among the dumped topics.

Then one day David Teece announced the Transamerica Seminar and I got to listen to Sid Winter talking about his ideas on knowledge and competence assets later to appear in the 1987 book chapter, Dick Rumelt's description of entrepreneurs trying to fence in their luckily discovered wells of wealth, and Giovanni Dosi's lecture on technological trajectories surrealistically delivered while chain-smoking at the Berkeley Faculty Club. The rather loose set of ideas presented at the seminar struck a cord within me and later developed into my Ph.D. thesis on voluntary and involuntary dissemination of technology. For some time I had set my mind on collecting data about the commercialization of major innovations in Swedish multinationals, and now finally found myself with some interesting ideas about how to characterize firm knowledge. An excursion to Southern California to meet Everett Rogers and talk about dissemination of technology furthered my thinking; see his book Rogers (1962).

Returning to Stockholm School of Economics for family reasons, after having prolonged my stay in Berkeley by making money through painting apartments every weekend, I found that I had a new office neighbor at the Institute of International Business. After a year and a half in the US, I was naturally attracted by Bruce's passion for his research and the urge to learn that I had grown to like so much. Not knowing I had met a Californian with roots in the same parts of Europe as myself, we started chatting…

Resource-based view of the firm

At about the same time that ideas on knowledge began to be published, there appeared concomitant ideas that came be to be labeled the resource-based view (RBV). A recurrent comment has been that we were 'really presenting the RBV'. The knowledge

and RBVs of the firm are often regarded as similar and indeed they are. Their differences reflect their origins, as they sought to answer different questions. RBV is a theory of strategy and of sustainable rents. Its origins lie in the early articles by Birger Wernerfelt (1984) who brought a micro-economic foundation to understanding advantages. In many ways, he combined the approach of Edmund Malinvaud on activity analysis with Bain's (1956) emphasis on barriers to entry. A conceptual leap was given by Lippman and Rumelt (1982) in their article on uncertain imitability, in which heterogeneity in competences and, hence in profits, could be sustained in equilibrium; firms could not imitate each other or only with uncertainty. Yet, there appeared much swept under this rug: what was uncertain? Why can't the most productive firm just grow instantaneously to serve the entire market?

The 1986 article by Jay Barney in Management Science missed by and large the importance of uncertain imitability, arguing instead that with perfect and complete capital markets, rents should be zero for all firms. Since there was no behavioral discussion of what constitutes culture or heterogeneity, the implication was that *assets* could be purchased and combined. A far more nuanced treatment of assets was the riposte by Dierickx and Cool (1989) who noted that competitive advantage, like a British lawn, could only be developed over time. Imitation was difficult, as was growth. Still, why firms could not acquire advantages, or simply fire and hire to create new ones, was not profoundly addressed.

This interesting legacy has tended toward an *engineering* view of strategy. We call this a 'Lego' view of the firm, in which knowledge can be captured in modules and transferred, sold, and bought in firms and in markets. The strategic job of the manager is to find the right mix of modules in anticipation of the market. Hence, the insight of Makadok and Walker (2000) that prediction is a capability that requires no resources is incisive. The manager need only be the Kirzner entrepreneur who foresees opportunities better than others.

The early empirical work on resources relied upon the traditional measures of asset allocation, such as research and investment expenditures. This is not surprising, as there was no indication what resources might be. Since then, there is an increasing sophistication in econometrics and a separation of resources from capabilities. Still, the empirical work continues to stumble on definitions of resources.

It is not surprising that the strategy viewpoint has focused on what are sources of excess rents (profitability). The RBV of the firm permitted a turning away from industry position towards questions that researchers in business schools knew better than economists, namely, how firms work. Oddly, in our view, this 'inward turn' was excessive and overlooked that resources evolve in reference to a market and, hence, to competitors and their positioning. This is not for us to criticize in these pages. Rather, we have a more simple complaint: in the absence of a behavioral foundation, choosing resources is simply a highly complex dynamic programming problem. In other words, the RBV of the firm started off wrong by arguing market behavior don't matter and by being negligent in explaining why are things sticky in the first place.

There are some problems that are rightfully treated in this engineering fashion, but many of the so-called resource advantages of a firm defy such an analysis. This criticism is identical to the 'functional fallacy' in which history is understood as action explained by consequences. To abbreviate our argument, consider this characterization. An important resource for the firm is the commitment of people to the firm. Firms with higher commitments consist of hard to imitate resources that give them an advantage in the market. The strategy of the firm then is to increase its value by increasing commitment, that is, in order to increase value, firms signal to employees that they are committed.

Our observation is: the best signaling to employees may be evidence that top management stands by its employees independent of the goal to increase the value of the firm. As anyone knows from having watched Internet firms try to create 'communities', people commit to each other; they do not commit to AOL, Microsoft, unless these abstractions become part of an identity. The orientation of a knowledge perspective is to understand the origins of advantages in firms as social communities that are resistant to manipulation by social engineering theories of strategic leadership. We are very skeptical that strategic theories of resources will advance in the absence of a more profound investigation of the cognitive *and* social foundations of knowledge. It is not simply enough to note, however belatedly, that strategies are cognitive; it is critical to understand that strategies are made in social communities located

in institutional settings. Strategy is a *situated* practice, our European colleagues would conclude, and we agree.

The firms as knowledge

Because of the focus on profits and value, the RBV of the firm has a very important advantage over the literature on knowledge: it is a more cohesive endeavor in the sense of a sociology of science. This coherence derives greatly from the intellectual role played by Barney (1991) and his article published in the Journal of Management that he edited. We confess to a distinct frustration at the time that while our article at Organization Science was submitted in 1988, we observed this article and others published far more rapidly by more efficient journals or through special issues.

This frustration at the time has an important historical value: the knowledge-based view of the firm developed in parallel and largely independently of efforts in strategy. Fredrick Hayek had noted the importance of tacit knowledge for economics in the 1950s. While Michael Polanyi is often cited as the philosophical source, it is Gilbert Ryle's (1949) Concept of Mind that directly and powerfully observes the 'category error' that what we think and how we think and do are very different. In the 1960s, Fritz Machlup (1980) in his studies on knowledge also starts with a discussion of tacitness.

These ideas of kinds of knowing ('know-who', 'know-what', 'know-why') are deeply reflected in the work on technology transfer that was so important to the field of international business, especially in regard to developing countries. Farok Contractor (1980), Paul Streeten (1974), and Sanjay Lall (1978) discuss the importance of 'absorptive capacity' (borrowing its use by Ekhaus in development economics) and of kinds of knowing. The Rand Corporation was engaged by the Department of Defense to help them understand how to transfer technology to military allies. The classic study by Hall and Johnson (1970) on the transfer of aeronautic technology to Mitsubishi no doubt influenced two young Rand economists, Nelson and Winter (1982). Though not picking up on Hall's and Johnson's important discussion of three levels of transfer (including the compatibility of the institutional context), Nelson and Winter make implicitly the Ryle distinction of know-what and know-how in their discussion of skills in their seminal book an Evolutionary Theory of Economic Development. As always in touch with 'the latest', David Teece played with these ideas in his publications on technology transfer. He also sought, unsatisfactorily in our judgment, to combine tacit knowledge with transaction costs to explain the diverse firm and the multinational corporation.

It is impossible to avoid the pivotal role of technology transfer when confronting the business history of multinational investments or in the accounts made by managers. Even much of the discussion of cultural differences reflects the problems of transferring technology to different countries. Why worry about cultural differences unless the home office felt it wanted to transfer practices and technologies across borders?

The 1987 chapter by Sid Winter transformed this issue by the following observation: if technology transfer concerns knowledge, then a firm must be 'knowledgeable'. Winter's treatment of the firm remained largely in the strategy tradition of trying to figure out to what factor we attribute the rents. He called this the 'imputation' problem: imputing economic value to factors. In his analysis, he noted that valuable factors must be those difficult to transfer and to imitate, and he stated (somewhat along the lines of Everett Roger's taxonomy of impediments to diffusion) characteristics that would inhibit imitation. Winter's chapter represented a far more nuanced and micro-economic treatment of uncertain imitability than found elsewhere at that time.

Criticisms of our contribution

From the circulation of the first draft of our JIBS paper, we have had the good fortune of direct and outright criticism forcing us to develop and refine our arguments. The following quote from a letter to a reviewer gives you a feel for the situation:

> Because your comments are rather general objections to our enterprise, we are basically forced to respond by way of counter-argument. We acknowledge the importance of your objections, and feel that they are deserving of public discussion, as are the points we raised in this article. It would be of great interest to migrate this discussion out from the exchange of reviewer and submitter into the public arena.

Our contribution that followed this effort was to make the argument that these ideas were sufficient to explain the firm; one did not need to invoke contractual hazards. We did not claim that contractual hazards are not present. To a great extent, whether knowledge or hazard is the better theory is not the question. Our claim, then and now, is that

the question is what their relative importance is. In some contexts, contractual hazards will be the more powerful explanation. As a first order effect, for explaining 'heterogeneity' or why firms specialize (the central boxes in Williamson's figure in his 1981 AJS paper), knowledge has proven to be the more powerful explanation. However, there is surely a second-order effect in how incentives, coupled with a division of labor, molds the evolution and sharing of knowledge.

Central to our theory is the idea of the firm as a social community. We placed, consequently, emphasis upon the cognitive properties of individuals and the routinization of individual behavior through 'higher-order organizing principles'. From the initial formulation, we emphasized that knowledge exists in networks and in institutionalized contexts. After all, in those case studies cut by Organizational Science (though they appear at length in Udo's thesis and in summary in our Zander and Kogut (1995) OS article), we described how knowledge transfer differed by country.

A major objective of our firm was to bring business history into the center of research on knowledge. As any student of international business history knows, multinational firms evolved by very different trajectories depending upon their national origins. The interplay of national institutions and entrepreneurship forged the organization of knowledge by firms, and these national variations resulted in distinct capabilities and organizational forms. We had hoped that business history would find a degree of 'liberation' from the ahistorical theory of transaction costs and would focus on understanding national patterns in the evolution of firm capabilities.

We were vulnerable, however, to two criticisms: our conceptualization of knowledge and our incomplete explanation of the boundary of the firm. The first criticism argued that our treatment of knowledge was mechanical, 'objective', and ignored that knowledge is learned and produced interactively. We accepted this criticism, though we would still claim that our measures of tacitness capture the difficulty of learning knowledge (read the questionnaire items attached to our JIBS article for your own assessment). In our 1992 discussion of knowledge, we reflected at length how the encapsulation of knowledge depends upon the language and the shared knowledge of the language. Still, the importance of context and the distinction between knowledge and knowing were not developed in our initial articles.

These points were very effectively developed by others, many of whom are European researchers trained in a *hermeneutic* tradition in which knowledge, including scientific, is essentially shared interpretation. The work of Lave and Wenger (1991) on communities of practice, and its innovative development by Brown and Duguid (1998), has had a very significant impact in the United States and particularly in Europe. This strand of work is also echoed in articles and books by Nonaka and Takeuchi (1995), who cite heavily European philosophers and who made an innovative contribution to understanding creativity. Nonaka spent a few months at the IIB. Gunnar Hedlund (Udo's thesis adviser) and he produced an early article comparing Japanese and Western firms regarding knowledge accumulation. This research made no reference to our working papers under review, though it is not our claim to review precedence. It would have been desirable for subsequent research in international business to have integrated these various perspectives – especially given the overlap in institutional identifications. But integration, even when willed, is often hard to achieve.

These similar streams of research have often justified the claims that the knowledge literature is not cohesive and, thus, can be dismissed. This observation is usually made in the start of an article that justifies yet another attempt to define knowledge, hence confirming its initial premise that there is no cohesion. Since academics are in the knowledge disciplines, there has been an overwhelming variety that frequently has a 'me too' flavor. There has been a resistance to deciding what questions are to be answered and, more importantly, which ones have been answered. We have read the criticism that there is a proliferation of terms for similar constructs, and there is not much that individual scholars can do. For example we have largely tried to avoid the need to explain how 'combinative capabilities' differ from 'dynamic capabilities' and which has had the greater impact in strategy.

Instead, we tend towards the view that it is more useful to state the program and mark the progress. As we have two fine tributes by Steve Tallman and Alain Verbeke accompanying our comments here, we will simply give an example in Morten Hansen's (1999) excellent articles in the Administrative Science Quarterly and Organization Science on knowledge transfer in multinational corporations. Hansen validated his constructs against the constructs used by Szulanski (1996) and Kogut and

Zander (1993) and he also integrated the constructs of knowledge with the firm as a network (associated in the international literature with Bartlett, Ghoshal, and Nohria). This work provides insights into the control of multinational corporations and the limits of geographic dispersion that was not simply possible prior to knowledge based views of the firm.

The second criticism was that having explained why firms are social communities, we did not explain why social communities have boundaries and why there would be 'more knowledge inside'. This essentially was the criticism of Nicholas Foss published in Organizational Science and to which we responded with the article awkwardly entitled 'What Firms Do? Coordination, Identity, and Learning.' We did not like the core of Foss's criticism – though we had admired the corpus of his research, because we felt it was useful for research to expend the effort deciding if a theory of the firm could exist in absence of incentive conflicts. His effort to say that a theory of knowledge could be combined with incentives struck us similar to the excellent analysis of Jean Francois Hennart made 15 years ago at that time. But he was right to say we had omitted something.

We made the simple observation that the knowledge view of the firm would require a different behavioral foundation than individual utility maximization. We posited that a critical property of people is that they 'identify'. Historically, they can identify with the guild, with their class or profession, or with many other 'reified' social entities. For us, the economically important implication is that a division of labor precedes such identification: categories serve as both the definitions of types of knowledge and as entities to which people identify. Coordination often fails because people fail to communicate because their identities simply get in the way, not because they are acting by guile. We had made essentially this observation in our response to the criticisms published in JIBS by choosing a somewhat sarcastic example that all academics would recognize:

> One of the common and cyclical cries of academic life is for more inter-disciplinary research. It is possible that such research is scarce because of market failure among contracting parties. Devious physicists are outwitted by the topological guile of complex mathematicians. David Lodge and god know how prone academics are to cooperative failure (1995, p. 423).

In other words, we did not say that people are programmed to identify with their firms, or must have a single identity. Rather, we argued that it is an outcome of a historical process that work is organized not only by skill but also by firm. What gives firms an advantage over a market (or a network for those that feel better with this term) is that identification *correlated* with a firm's boundaries enhances coordination, communication, and learning. Such identification also limits the diversification of the firm (auto engineers don't make fashion clothes but they might make textile machinery or software) and consequently the evolution of future competences. We ended the 1996 article analysis on an empirical note in using a Boolean methodology to show firms compete on heterogeneous complementarities. This analysis was one of the first empirical studies to provide an empirical methodology to identify *complements* and to show how they vary by firm.

Foss's (1996) response to this article was to dismiss it as essentially non-economic. As he had the last word in that issue, we would like to make three quick observations. First, it is easy to convert identification into a 'price' and to show that boundary is determined by the tradeoff between variety and specialization (see Kogut, 2000). Second, identity is no less economic than the behavioral claim people maximize their utilities. Third, identity is so pervasive that it may well represent an integrating foundation to many social phenomena and disciplines (see Akerlof and Kranton, 2000; Tilly, 2000, for examples.)

A primary advantage to the concept of identity is that it permits an understanding of the evolution of a firm's knowledge, and its extension across borders, in reference to institutional contexts. For this reason, we conclude that identity, as a foundation of knowledge, is of utmost importance to researchers in the area of international business and multinational management who are interested in such questions as convergence, economic development, and technology transfer. At the end of the day, we forecast, a principal explanation for why knowledge does not flow across borders lies in the differences in professional categories (and hence in the division of labor) and the cognitive claims and status attributions attached to such categories.[2]

A culmination of our work was our article published in the American Sociological Review comparing the two Zeiss companies. We kept the ideas of knowledge in the distant background and focused on the interplay of institutions, markets,

and technology. Zeiss West Germany had begun after World War II with some of the refugee staff (managers, scientists, and engineers) from the original company in the East, and claims to the patent portfolio. Zeiss East Germany kept the buildings and most of the factory workers, but its capital equipment had been evacuated to the Soviet Union as part of reparations.[3] Many of its best scientists had migrated to the West under American policy. It too was largely a new venture, with claim also to the same patents. The natural experiment was too compelling to avoid: how did two firms, starting with identical patent portfolios, develop their technological trajectories over time?

The answer was that the trajectories were highly correlated. The East German Zeiss in Jena was not a technological disaster; it had distinct capabilities and a moderately successful export record. A primary default was that the East German state demanded too much of this body of expertise, forcing it to diversify into semiconductors and other areas. Jena resisted, largely due to the entrepreneurial efforts of its director, Biermann, who insisted on the independence of their research and development laboratories.

This story enraged many of our colleagues, especially those in economics who were deeply hostile to the socialist legacy. In seminars, many refused to admit that any technological achievements were made under communism, as if the Soviet rockets were shot in space by huge rubber bands stretched across the Steppe. Their arguments were riddled with paradoxes, viewing socialist firms as valueless, while believing their privatization would result in sudden efficiencies due to incentives. All of this is sadly amusing, when we read the history of socialism and the massive efforts to develop finely tuned incentives to promote effort and then innovation. Total lack of knowledge and incentives were not the problem under socialism.

After interviews and long hours in the West German company archives, we drove to visit Jena – the first time from Poland. Udo's father was born in West Prussia in a village outside Torun, and Bruce's grandfather was born in a small town about a hundred kilometers from Kielce. We visited this rural town. It was poor, people worked the fields with horses, and older women tended their one cow along the roads where they could feed. We had come from Warsaw and still wore our ties and jackets. The town people treated us coldly, as we asked in English and then in German after information. The year was 1993 and they were afraid we were Germans coming to buy their land, or to reclaim it.

As we made our way to Jena, waiting a whole night until morning at the Polish–German border, we talked often about history, our families, and the aftermath of the collapse of communism. The town of Jena was no longer especially impressive. Zeiss was located in a tall 'skyscraper', but the archives were located near the 16th century University. Jena, the city where Napoleon defeated the Prussians and liberated the under-cast, was a shadow of its past. Yet, in the archives, we meet the 'archivist' who spoke with tears in her eyes of the achievements of Zeiss and her gratitude that someone from the West came to write a history of her company. We thanked her in our acknowledgements to the paper, this keeper of knowledge and of an identity desperately preserved at the twilight of its perish.

Conclusions

We have offered these personal comments as a recognition of a teaching we learned from Larry Prusak: knowledge is often best understood and remembered via stories. Over the years, we have come to appreciate better this metaphorical aspect of knowledge generation and transfer. At the same time, we feel that a lot is explored regarding the concept of complementarities and how incentives and perception molds their evolution. We sketched these ideas and provided an empirical start on their testing in our Kogut and Zander (1996) Organization Science article, and they remain teasingly present as worthy of further research.

We would like to again thank our colleagues, the AIB, and JIBS for this award. It recognizes an article that appeared at a fragile moment in our careers. It reminds us of the personal journey we both took and the fun we had in our research. If our research has a primitive belief, it is that knowledge grows out of healthy social communities. In many ways, AIB and its members have made powerful impressions on us and have encouraged us in our work. We would in receiving this award like to acknowledge their contributions and their importance to us.

Notes

[1]See Zander (1991).

[2]Udo, in his fieldwork on the management of international R&D found many examples of national engineering cultures. Sometimes the introduction of hardware like CAD/CAM systems was

the only way to standardize action: a French engineer could accept adapting to a 'stupid machine', but would never conform to a 'Swedish' way of engineering.

[3]Steve Kobrin related to us in a message sent after the publication of the article that the Soviets in fact produced a camera, the Kiev, that advertised its manufacture used the old Zeiss machinery.

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Appendix A: Quotes from the review process

To the editor: 'We can't see anything in them [the reviews] which should prevent publication, though we have incorporated by and large all comments other than those just objecting to the story.'

To the editor: 'Reviewer 1 took the high ground, citing everything but the Bible to us.'

To the editor: 'The last reviewer made many good points. Frankly we want to write this paper as an essay, not hypothesis one, two, etc. It is hard to interpret the comments on jargon; there has to be some common vocabulary specific to our field and

the cited words strike us to be precisely this vocabulary.'

To reviewer 2: 'Thank you for your supportive comments. It is very unusual that a reviewer states a disagreement, while fully supporting the efforts of an article.'

To reviewer 4: 'We are aware that we have written this paper in a different style than the usual article, and we would like to stay with it. We have tried to present the intellectual heritage, if you will, of the field so that we might all have a chance to reflect on it and move forward. For this reason we have included the quotes, and whereas you may be right that they are disturbing, they should also mean something to all of us educated in this line of thinking. Curiously enough, from the selfish perspective of the authors, this practice will make the article less accessible to people outside the field. But we are trying here to address the central readership of JIBS.'