KNOWLEDGE, ECONOMIC ORGANIZATION, AND PROPERTY RIGHTS

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Organization, Property Rights, and Knowledge:
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ABSTRACT
This paper is the Introductory chapter to my forthcoming book, *Knowledge, Organization, and Property Rights: Selected Essays of Nicolai J Foss*, to be published by Edward Elgar in 2008. It provides a brief bio-statement and then discusses and places in context the various papers in the collection. The papers in the book are listed in the Appendix.

KEYWORDS: Organizational economics, knowledge management, strategic management.

BACKGROUND

Economics

I have been happily employed for almost two decades at a business school (namely, Copenhagen Business School), I entertain broad and diverse interests (no doubt too many for a serious scholar), and I consider myself to be management scholar. I have never been to a meeting in the American Economic Association. Still, the dominant disciplinary influence on my research has been, and remains, economics. What is published in the better sociology journals is no doubt serious research of considerable relevance to management research, and the same may be said of much research in social psychology. However, psychologists have little idea of the notion of action relative to constraints, and sociologists usually over-emphasize certain constraints over others. In my opinion, one of the main advantage of economics as a social science is that its ability to neatly *frame* problems is superior. To be sure, the framing may often be simplistic (as in the way economists deal with incentives, usually ignoring motivation crowding out effects, Frey and Stutzer, 2007), but it is seldom a bad starting point (those crowding out effects can be modelled as a refinement of the basic model).

I suppose I was from a relatively early age an intuitive economist; at any rate, I remember taking the position in a class in 7th grade in elementary school that, on the whole, organized crime should be preferred relative to un-organized crime, as there would be more crime under the latter situation than under the former. My teacher and classmates reacted with disgust. I remember brief exposures to economics in high school and finding it highly fascinating, not the least because of the moral fervour that my left-wing teachers could mobilize against the latest sinister plots of the sycophants of Anglo-Saxon/US capitalism, that of monetarism (Klein [2008] is really nothing new); predictably, I became a staunch monetarist (and buying into the complementary political positions as well) in University (where I encountered the same attitude, albeit with a bit more sophistication). I had little hesitation that I had made the right choice when I embarked upon the economics program of the University of Copenhagen beginning of September 1983 at the tender age of eighteen.

Copenhagen University

Economics at the University in those days were split between macro-oriented empiricists and mathematical, general equilibrium theorists (luckily, many of the schisms of two decades back have been overcome today). While the former had much standing in the Danish community, the latter had the standing in the scientific community. Econometric model building was also very strong. Only one or two professors did what may be called "applied price theory," so the use of economics as a tool to make sense of the choices people make in the real world and how these choices aggregate to collective outcomes — my main motivation for studying economics — was not stressed. The implications of the missing linkages between these areas were very clear in the teaching that students were exposed to (although it is nothing like the disciplinary diversity that management students are exposed to): Things didn't really seem to connect, so students made choices rather early in the study program to focus on either "micro" or "macro. Most focused on macro; this was where money and power were, that is, positions with the major banks or ministries.

The majority of the teaching in the bachelor part of the economics program centred on macro-issues, and I rapidly developed a strong dislike for the kind of boring macro-drill that we were exposed to. Things began to change when I realized in the second year of my economics study that there were strong alternatives to the Keynesian-macroeconomics-and-macro-econometric-modelling focus of so much of the teaching. The methodological essays of Robert Lucas (1977, 1980) and Milton Friedman's Presidential Address to the American Economic Association (Friedman, 1968) in particular captured my interest, probably because they were bashed by my macro teachers (I do admit to having a querulous tendency in my nature).

At one of my raids at the liberary of the Economics Department, I picked up an old, dusty volume with the appealing title, *The Fallacy of the New Economics* (Hazzlitt, 1959). It turned out to be an energetic (though over-the-top) smashing of the Keynesian revolution (my Keynesian macro instructor eagerly picked it up when he saw it on my desk, expecting it to be an attack on new classical macroeconomics; after flipping through it he put it down again with a disgusted look). When researching the author, I found out that he had been associated with an "Austrian School of Economics." At about the same time I was reading Axel Leijonhufvud's brilliant *On Keynesian Economics and the Economics of Keynes* (1968), a very different attempt (and one with which I felt more sympathetic) to furnish micro-foundations for macro-economics than new classical macroeconomics. That book contained references to a "Friedrich Hayek," and when checking up on that name I again encountered the enigmatic "Austrian School."

This led, of course, to the discovery of the writings of Hayek, as well as Mises, Kirzner, Lachmann, and other modern Austrians, as well as the classical liberal tradition in political philosophy. I did not become a die-hard Austrian (I have usually thought of myself as a fellow-traveller), but I found, and continue to find, the basic Austrian vision highly congenial and inspiring. However, I fundamentally like and admire so-called mainstream economics, and although disequilibrium, process, entrepreneurship, and the subjectivism of knowledge and expectations continue to be soft spots in the mainstream corpus, the mainstream seems to me to increasingly make very serious and sustained attempts at coming to grips with the kind of issues that have been central to the Austrians.[1] It was also about this time (i.e., towards the end of the 1980s) that I discovered new institutional economics, that is, transaction cost economics and property rights economics. I remember struggling with Alchian's famous 1965 property rights paper, and finally experiencing a sort of epiphany: This kind of reasoning simply made (almost perfect) sense of the world! I have continued to be highly, and in fact increasingly, sympathetic to the new institutional economics, and to pursue applications of this body of economics in management research.

However, in spite of this interest in new institutional economics I decided to write my master thesis, completed in late 1988, on the business cycle theory of Hayek (e.g., Hayek, 1931) (parts of the thesis were later published as Foss, 1996). After submitting the thesis, my thesis supervisor, Professor Hector Estrup — an expert in the now unfortunately largely defunct field of the history of economics — managed to get me a 6 months stipend at the Economics Department at the University, but it became clear that my interest in pursuing doctrinal history studies in monetary theory did not resonate well with the current research strategies of that Department, so when I was offered a stipend to pursue graduate studies at the Copenhagen Business School, I happily accepted.

Copenhagen Business School

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[1] Thus, the theme of my 1994 book, *The Austrian School and Modern Economics: Essays in Reassessment*, written as a hobby project while I was working on my PhD thesis, is that although the Austrians, particularly Mises and Hayek, anticipated numerous themes that later became prominent in mainstream economics (e.g., asymmetric information, the agency problem, property rights), they failed to develop these ideas sufficiently and they failed to do concrete theorizing with them. As an example, although the Austrians had many of the necessary ingredients of an economic approach to organization before anyone else, they never thought of piecing them together. Thus, Austrian economics, though sound in its basic vision, seemed to me to constitute an instance of what Lakatos (1970) called a "degenerating research program" (see also Foss, 2000).

I arrived at CBS October 1 1989. CBS in those days was something very far indeed from the present highly entrepreneurial, energetic and research-oriented place: There was rather little emphasis on research, few senior people seemed interested in assuming a mentoring role (perhaps few could), there was no course work to be done, so as a PhD student one was pretty much left to pursue one’s own interests for three years. The only expectation was that one would produce a monograph, typically in Danish, and minimum 250 pages long. Quite predictably, most PhD students were not able to finish in time: without the checks on procrastination provided by courses and teaching, time just slipped through the fingers of the students.

However, a cohort of particularly talented PhD students was hired at roughly that time. These included Kirsten Foss, Torben Pedersen, Bent Petersen, and Steen Thomsen who all since became professors. Also, I was lucky to become associated with some senior people who were quite research active. In particular, my advisor on the thesis project, Christian Knudsen, took a broad interest in economic theories of the firm, an interest that was clearly contagious for I soon developed a similar interest. Since little or no course work was required, it was to a large extent up to the above cohort to imprint itself. There were obvious negative sides to this (it is difficult to learn statistics in this manner!), but also the benefits that come from the feeling of being part of an exciting emerging perspective (we all shared a fascination with new institutional economics), a certain *esprit de corps*. To illustrate, a number of us were literally lined up at the CBS library eagerly waiting for the arrival of the issue of the *Administrative Science Quarterly* that featured Williamson (1991) (obviously, this was before internet access). The conferment of the Nobel Prize to Ronald Coase in 1992 gave rise to celebrations. The discovery of the emerging resource-based view in the beginning of the 1990s was also a major event. We increasingly found that we shared an emerging perspective (for the rest of the story, including various organizational ramifications, see Foss, 2007).

My PhD thesis, submitted in 1992 was essentially an application of capabilities and transaction cost economics to the issue of the organization of technological innovation, an undertaking that was heavily inspired by the work of David Teece (1986) and Richard Langlois (1988). I foolishly wrote the thesis in Danish (very few management PhD students and even economics PhD students wrote their theses in English at that time), but managed to get virtually the whole thesis published as articles translated, of course, from the Danish text.

I was enthused to recognize that many of the ideas on privately held knowledge, dynamics, impediments to exchange and rational agents circumventing those impediments, spontaneous development of institutions, entrepreneurship, and so on that I (perhaps more or less correctly) associated with the Austrian School were part of the core ideas that were taught in business schools. They were also rather closely associated to ideas that were becoming prominent in the business schools in those days, such as insights derived from evolutionary economics (notably the evolutionary theory of the firm and it "capabilities," "dynamic capabilities," etc. offspring) (Nelson and Winter, 1982), and transaction cost economics (Williamson, 1985). I decided that a business school would be an ideal research environment, given the basic outlook I held. Not an unusual decision for a failed economist (or failed sociologist, for that matter)!

I have followed the traditional career path of Assistant (1993), Associate (1996) and Full Professor (1998), all at the Copenhagen Business School, and added adjunct professorships at the Norwegian School of Economics and Business Administration and Agder University. During the twenty years I have spent professionally at CBS, I have witnessed the growth of the school from being a sleepy, provincial and almost wholly teaching-oriented university to become one of the World's largest business schools with 400 faculty, about 17.000 students, marked ambitions, and a strong emerging research culture that increasingly results in publications in the best international management and disciplinary journals. This growth has come about as a result of a fruitful interplay between local initiative and a management approach at CBS that has stressed guidance and orchestration of initiatives rather than piecemeal management. Rather than adopting a uniform model of the organization of a research-based organization (e.g., emulating the typical US business school), or dictating research initiatives from the top, the architect of CBS' growth experience, President Finn Junge-Jensen emphasized a bottom-up approach. In his approach to stimulating school-wide research activities, somewhat reminiscent of the Oticon "spaghetti organization" (treated in "S***elective Intervention and Internal Hybrids: Interpreting and Learning from the Rise and Decline of the Oticon Spaghetti Organization***"), local entrepreneurial research initiative would be supported, sometimes very generously, once it had passed an initial and not very rigorous screening procedure. Typically, such efforts would be organized in a research center, encompassing between five and twenty scholars. Currently, CBS has almost forty such centers. A more serious evaluation would happen later; thus, selection was rather *ex post* than *ex ante*. The whole, largely successful, exercise is a prime example of "guided evolution" (Ghoshal and Løvas, 2000) in an academic context. I have myself been a beneficiary of this approach, as my research has been generously supported, and I am now the director of The Center for Strategic Management and Globalization, a 12 faculty research center devoted to research in international business and strategic management (more on the Center in Foss, 2007).

RESEARCH INTERESTS AND THE PAPERS IN THIS COLLECTION[2]

Early Work

As is usually the case, the research I did for my PhD thesis has very much shaped my subsequent work. My thesis work drew on the industrial economics tradition of post-Marshallians such as Edith Penrose (1959), George Richardson (1972), and Brian Loasby (1976), evolutionary economics in the Nelson and Winter (1982) tradition, and Williamsonian transaction cost economics (Williamson, 1985, 1996). The thesis was very much taken up with meta-issues, specifically issues of theory comparison and integration and the underpinning of such discussions in the theory of science. This interest has never left me, and although much of my work over the last decade or so has been empirical and relatively conventional management research, I keep returning to it. Although it is easy to risk being branded as somebody who does work that is "merely conceptual" or writes "thought pieces" (an academic putdown if there ever was one!), it is my strongly held conviction that in an inherently multi-disciplinary field such as management research, the risk of fundamental confusion is an ever-present danger and the need for a high degree of reflexivity is warranted. Efforts that make an attempt to sort out the fundamental theoretical and methodological issues should therefore be welcomed. Perhaps because this is where my heart (mainly) is, these kinds of papers are over-represented in the present collection, as well as in my paper portfolio in general.

One of the chapters in the thesis was published as Foss (1993a), one of my first and most cited papers (but not included here because I am not happy anymore with the style and message of the paper). Much of my PhD thesis dealt with what foundational and conceptual issues, such as the philosophical doctrine of realism and economics (Foss, 1994c), the

[2] Check www.nicolaifoss.com for a complete listing of publications plus some downloadable papers.

functionalist mode of explanation of transaction cost economics (Foss, 1994d), and the epistemic assumptions of transaction cost economics (Foss, 1993b, 1994d). Most of this work linked up with the evolutionary research programme in economics. The work of Richard Langlois (1992; Langlois and Robertson, 1995) was a huge source of inspiration, as was the work of Brian Loasby (1976, 1991), and the modern semi-formal evolutionary economics associated with Nelson and Winter (1982). Although I have problems with some of the key constructs of this approach (notably, notions of routines and capabilities), in its application to the theory of markets I see it as an interesting and laudable return to and updating of an older and more "realistic" and processual tradition in economics, one that was taken up with the analysis of changes rather than the comparison of equilibrium states. It is not surprising that it has been such a strong source of inspiration for management scholars.

At about the time I did this kind of research I became one of the co-founders of the Danish Research Unit of Industrial Economics (DRUID), which (established in 1995) now has become a highly successful forum for the latest high quality work in industrial dynamics, a sort of European version and blend of the Academy of Management and the Joseph Schumpeter Society. The DRUID was conceived of as a vehicle of disseminating the new evolutionary economics (i.e., post-Nelson and Winter EE), and although this orientation has diminished somewhat it is still strong. Partly inspired by the ethos of DRUID, I published a number of papers throughout the 1990s on the history of economics and management research. Thus, I published a couple of papers on the work of Edith Penrose, taking issue with the conventional interpretation of this work as the main precursor of strategic management's dominant perspective, the resource-based view (the RBV): Whereas the RBV is an equilibrium approach that focuses on the necessary conditions that must obtain for resources to yield rents in equilibrium, Penrose stressed growth, resource-building, and the dynamics of related diversification, and she was not taken up with competitive advantage *per se* (Foss, 1999b). Brian Loasby (1976) directed my attention to the work of the brilliant but neglected economist George B. Richardson (Foss, 1994f, 1995). With Brian Loasby and John Kay I arranged a conference in honour of Richardson (on the occasion of his 70th birthday) at St. John's College, Oxford University in 1995 (Foss and Loasby, 1998). Richardson had left economics in the mid-1970s to become CEO of Oxford University Press in frustration over the lack of interest in his work.

Capabilities, Resources, and Economic Organization

Richardson was, however, the economist who, building from Penrosian foundations, coined the notion of firm-level "capabilities." His thinking on asset specificity and complementarity, particularly in his small monograph, *Information and Investment* (1960), was decades ahead of its time. Richardson was also the first scholar to attempt to explain the boundaries of the firm in terms of capabilities, that is, to give provide an (at least seemingly) alternative explanation to the Coasian approach. The capabilities notion and the associated theorizing seemed to dovetail in various ways with what was going on in the exciting field of strategic management that I was discovering at that time, particularly with various extensions of the resource-based view (Barney, 1991). It provided, in other words, a very fitting bridge for somebody who was in the process of taking the step from economics to management research.

Capabilities have in fact emerged as one of the key constructs in management research. The central notion goes straight back to Penrose and Richardson, namely that firms should be conceptualized as repositories of routines and capabilities (e.g., Nelson and Winter, 1982; Kogut and Zander, 1992) (see also Foss, 1999a; Langlois and Foss, 1999). It is, furthermore, asserted in this stream of research that routines and capabilities cause firm-level outcomes such as financial performance, innovation, and the boundaries of the firm (e.g., Nelson and Winter, 1982; Kogut and Zander, 1992; Teece, Pisano and Shuen, 1997). Thus, it is argued that explaining firm-level outcomes should take place in terms of other firm-level variables. However, the overall problem with such reasoning is that there are no conceivable causal mechanisms in the social world that operate *solely* on the macro level. There are no macro-level entities on the social domain that somehow possess capacities or dispositions to act (Cartwright, 1989) that make them capable of directly producing macro-level outcomes and there are no direct processes of interaction between macro-entities.

Albeit obvious to somebody with a methodological individualist background, this recognition has been extremely slow to develop in the strategic management and related literatures. Presumably because of the strong dominance of a mode of research that is all about building firm-level data bases and engaging in cross-sectional analysis of firm-level data, the problem of accounting for the causal mechanisms that actually mediate between the analyzed macro- or firm-level has too often been neglected; that is, micro-foundations have been black-boxed, whether consciously or unconsciously. In a very brief paper (a note, really), "***Knowledge-Based Approaches to the Theory of the Firm: Some Critical***

Comments;" reprinted as the first paper in this collection, I pointed out that existing accounts of the link between capabilities and economic organization (e.g., the boundaries of the firm) black-boxed or bypassed these micro-foundations so that the link was elusive. The challenge has not, it seems to me, yet been satisfactorily addressed in the capabilities literature.

Micro-foundations and Knowledge Governance

In retrospect, the 1996 paper is the origin of two closely related research streams: Micro-foundations in strategic management and organization research and what I (borrowing from Grandori, 1997) have termed "knowledge governance."

At some point after the 1996 I realized that the problem I had diagnosed in the 1996 paper was a general one that beset this literature, that is, a lack of micro-foundations. The recognition emerged partly as a result of being challenged by my then PhD student Volker Mahnke concerning how I, with my Austrian inclinations and therefore presumed sympathy to methodological individualism, could be supportive of notions of capabilities and the like. However, it took me almost a decade, as well as teaming up with a kindred spirit, namely Teppo Felin (Brigham Young University), before I made the point explicitly. Teppo is a keen thinker with a taste for the radical argument and I have had the pleasure of collaborating with him on quite a number of paper projects. We realized that we shared a commitment to a basic rationalist and realist outlook, and, therefore, a strong aversion to the social constructivist currents that are now rampant in management research (on this, see Felin and Foss, 2008).

In our first collaboration, a brief essay titled "***Strategic Organization: a Field in Search of Microfoundations***," Teppo and I explicitly argue that collective-level constructs in strategic management (and related) fields suffer from a general problem of accounting for their micro-foundations, which makes it hard to understand how they have emerged and how they may be changed.[3] Ultimately, the lack of such micro-foundations means that the managerial relevance of (very prominent) collective level concepts, such as capabilities and absorptive capacity, is unclear: How can managers hope to build capability and absorptive capacity if the way in which these are rooted in individual action and interaction is fundamentally unclear? Somewhat surprisingly, the paper is probably the first explicit

[3] These ideas are pinned out in greater detail in Felin and Foss (2006) and Abell, Felin and Foss (2008) builds a formal model of the micro-foundations of capabilities.

statement of the problem of lacking micro-foundations in management research.[4] However, it seems that strategic management is now embarking on a micro-foundations project somewhat similar to, and perhaps inspired by, economics and rational choice sociology (e.g., Teece, 2007).

My concern with the micro-foundations of knowledge-based ideas led — rather naturally, I believe — to an interest in bounded rationality which I (like no doubt many others) had thought of as the natural individual-level underpinning of notions of routines and capabilities. Indeed, modern writers on routines and capabilities refer (routinely) to Simon, Cyert and March as important precursors of their work (e.g., Nelson and Winter, 1982). In "***Bounded Rationality and Tacit Knowledge in the Organizational Capabilities Approach: an Evaluation and a Stocktaking***," which was published in 2003 in the Nelson and Winter Festschrift issue of *Industrial and Corporate Change*, I argued that the link between the individual level and the organizational level had not been established in moden work, except metaphorically (by the argument in Nelson and Winter that "routines are the skills of an organization") and that, at any rate, routines were really more about tacit knowledge than about bounded rationality. Thus, bounded rationality was invoked in a merely ceremonial manner and not fundamentally inquired into. At the same time I argued that this was a broader characteristic of the theory of the firm or organizational economics literature. "***The Rhetorical Dimensions of Bounded Rationality: Herbert A. Simon and Organizational Economics***" makes the point that bounded rationality has usually been invoked in a rhetorical manner (in a pre-McCloskeyan sense of the word; McCloskey, 1983). Bounded rationality is not in general necessary to drive organizational economics arguments, but is invoked, perhaps partially out of reverence to Simon (or as an appeal to authority?), as a label for a host of things that may make contracts incomplete. I also argued that Simon himself had failed in his attempt to persuade economists that bounded rationality is important: He hammered away at what it was not (namely, maximizing rationality), but failed to truly explain (at least to economics audiences) what it really was.

[4] To be sure, micro-foundations were an emerging theme in management research before our paper, though usually implicitly. Scholars realize that understanding such issues as value appropriation (Coff, 1997; Lippman and Rumelt, 2003a), resource value (Lippman and Rumelt, 2003b), strategy implementation (Barney, 2001), and factor market dynamics (Makadok and Barney, 2001), requires that substantial attention be paid to explanatory mechanisms that are located at the micro-level, that is, the level of individual action and interaction.

My interest in the micro-foundations of knowledge-based views in management also led me to what often passes as “knowledge management.” In particular, I have maintained an interest in issues of managing knowledge in multinational corporations, an interest I have often pursued in collaboration with my CBS colleague, Torben Pedersen, a prominent international business scholar. An example of our collaboration is ”***Sources of Subsidiary Knowledge and Organizational Means of Knowledge Transfer***” which takes its point of departure in the literature on the “differentiated MNC,” that is, the notion that the such firms can be seen as networks of units that each control heterogeneous and potentially complementary knowledge assets, and that they may derive advantages relative to national firms to the extent that they can build complementarities between such knowledge assets (this literature goes back at least to Hedlund, 1986). In the paper we examine the sources of the knowledge that is held in subsidiaries and argue that these sources matter greatly to the costs and benefits of knowledge transfers inside the MNC. We also argue that MNC headquarters can influence subsidiaries’ knowledge sourcing activities. Thus, the paper is an attempt to dig deeper in terms of anteceding and moderating influences on MNC knowledge management activities.

“***The Use of Knowledge in Firms,***” a play on the title of Hayek’s (1945) famous essay, reaches back to my Austrian roots and raises some Austrian concerns relating to the treatment of knowledge in the mainstream economics of organization. It examines the drastic simplifications in the knowledge dimension that organizational economists introduce in their attempt to achieve theoretical traction, illustrating a quip by Brian Loasby that most management problems concern the things that are blackboxed in economics models. “***Theoretical Isolation in Contract Economics”*** is almost like a companion paper, but with a different, theory of science orientation, specifically looking at the kind of “isolations” theorists perform in trying to make their theorizing determinate, and pinning out the specific “on-off” modeling approach employed by contract theorists. The “Use of Knowledge in Firms” paper surely raises many more questions than it gives answers, but I still believe that its basic message is correct, namely that problems of coordination (in the sense of game theory) have been neglected in organizational economics. Notions of capabilities in management and economics have sometimes been presented in the context of examining the coordination of knowledge in the absence of conflicts (see also Foss, 1999), but as already suggested, they do not seem to me to have been successful here.

One of the many profound insights in Hayek's (1945) essay is that societal institutions should first and foremost be assessed against the extent to which they assist in the creation of new knowledge and exploitation of existing knowledge. To be sure this view is at least implicit in the knowledge-based view of the firm as well as in work on organizational learning (March, 1991) and in work on knowledge management. The idea directs attention to how processes of transferring, sharing, integrating, and creating knowledge can be governed by means of choices of governance structures (i.e., markets, hybrids, hierarchies; cf. Williamson, 1996), as well as the governance mechanisms contained in governance structures, such as authority, reward systems, standard operating procedures, ownership, etc. (see also Grandori, 1997, 2001).

In "***The Knowledge Governance Approach***" I argue that although there has been much interest in how organization and knowledge processes relate in management research since the mid-1960s, there has been a surprising lack of systematic investigation of how formal organisation (governance structures and mechanisms) impacts knowledge processes. Organisational governance as a consciously designed effort seems de-emphasized, and the organisation-level antecedents that are, in fact, investigated are predominantly variables such as prior related knowledge, organisational practices, or structural conditions such as network positions, centrality, and the like. However, the relative absence of interest in formal organization in large parts of contemporary writings in management research on knowledge, is somewhat worrying for a number of reasons: Managers can often more directly influence formal organisation than informal organisation; it is usually easier to change job descriptions, reward systems, etc. than to effect changes in, for example, corporate culture. Indeed, the former may drive the latter with a lag. Evidence suggests that managers do change formal organisation in an attempt to influence knowledge processes.

Cutting thematically across the fields of knowledge management, human resource management, organization theory, and strategic management, the knowledge governance approach starts from the hypothesis that knowledge processes can be influenced and directed through the deployment of governance mechanisms, in particular the formal aspects of organization that can be manipulated by management, such as organization structure, job design, reward systems, information systems, standard operating procedures, accounting systems, and other coordination mechanisms. It is a systematic attempt to address these issues, paying explicit attention to micro-issues of motivation and cognition (Grandori, 1997;

Osterloh and Frey, 2000), which is strongly inspired by the basic explanatory approach of transaction cost economics: Knowledge governance may be conceptualized as a sustained attempt to uncover how knowledge transactions — which differ in their characteristics — and governance mechanisms — which differs with respect to how they handle transactional problems —, are matched, using economic efficiency as the explanatory principle (cf. also Nickerson and Zenger, 2004).

Economic Organization

"***Coase vs. Hayek: Economic Organization in the Knowledge Economy***" grapples with knowledge governance issues in a more substantive than methodological way. The context of the paper is all sorts of arguments that the emerging knowledge economy is going to have strong transformative implications for the boundaries of firms as well as for "traditional" authority relations, claims that have flourished in the management literature (as well as in popular discourse) over the last decades. The paper develops a framework for examining such claims, making use of organization economics insights to examine claims that, for example, traditional authority relations and firms boundaries (the "Coasian" dimension) are increasingly breaking down as knowledge workers gain bargaining power and firms increasingly need to source knowledge beyond their boundaries (the "Hayekian" dimension).

It is a sort of programmatic paper that set the stage for a number of my subsequent research efforts. In particular, it points to the need in organizational economic and management research of devoting more effort to conceptualizing authority and to understanding the distinct incentive liabilities of authority. The standard notion of authority is essentially that of Coase (1937) which assumes that the boss can precisely identify the action that an employee should take and command him to take that action. If this is all authority was about, it is easy to see that it cannot survive "knowledge economy conditions," that is, an increased reliance on knowledge workers who may have superior information about their own action sets and about which actions should efficiently be taken. However, authority is also about making delegating decision rights to employees (who may be better informed) and deciding on other aspects of organizational design (this is further developed in Foss and Foss, 2008b). The incentive problem arises once it is recognized that authority in this sense also gives managers the right to renege on, for example, delegation with potentially harmful effects on work motivation. Productivity and overall firm performance may suffer.

An example of this line of reasoning is "***Selective Intervention and Internal Hybrids: Interpreting and Learning from the Rise and Decline of the Oticon Spaghetti Organization***" which discusses the radical organizational changes that took place in Danish hearing aids producer Oticon in the 1990s. In particular, the paper discusses the implementation of the so-called spaghetti organization, which was a radical to install market mechanisms inside the corporate hierarchy of Oticon, and why this organizational form ultimately failed. Although the Spaghetti organization seemingly represented a coherent organizational design, the paper argues that the very flat spaghetti organization fell victim to managerial meddling with the affairs of sub-ordinates, a meddling that proved strongly demotivating and gave rise to a more structured organizational form with more of a distance between employees and management. From the point of view of strategic management and organizational theory the contribution of the paper is to point to the potential incentive costs of strategies of using organizational means (e.g., emulating market organization) to foster dynamic capabilities. From the point of view of organizational economics, the contribution of this paper is to point to the potential importance of the otherwise neglected (or downplayed) phenomenon of managerial opportunism and to suggest that organizational form is an important antecedent of internal incentive problems.[5] In particular, the paper suggests that for property rights reasons — ultimately, managers do keep residual decision rights and credible commitments to not intervene are difficult to establish inside firms — there are limits to how far firms can go in the direction of emulating markets. This theme is also pursued in ***"Tying the Manager's Hands: Credible Commitment and Firm Organization,***" co-authored with Kirsten Foss and Xosé Vázquez, which in contrast to the small (well, 1) N research setting of the Oticon plays out in a large-N setting, namely firms in the Spanish food and electronics industries. The specific contribution of the paper is to look at the factors (reputation effects, informational distance within the hierarchy, unions, etc.) that may constrain the kind of managerial opportunism discussed in the Oticon case.

The delegation theme is also discussed in "***Performance Pay, Delegation, and Multitasking under Uncertainty and Innovativeness: an Empirical Investigation***," which

[5] Baker, Gibbons and Murphy (2001) lends formal support to the reasoning in the paper.

was co-authored with my CBS-colleague, Keld Laursen, a prominent scholar in the economics of innovation (see also Laursen and Foss, 2003). In the paper we employ a data base originally designed for purposes of innovation research to examine one of the key predictions of agency theory, the risk/incentives tradeoff. Finding that support for this relation is tenuous, we argue, relying on Jensen and Meckling (1992) and Prendergast (2002), that this is likely to be caused by delegation: Firms in uncertainty environments are likely to make more use of delegation, but delegation needs to be backed up with relatively high-powered incentives to check to moral hazard problem. Although agency theory is predicated on delegation (i.e., the principal delegates a task to the agent), our analysis suggests that agency theorists need to pay more attention to delegation and how it is linked to uncertain environments and asymmetric information (i.e., the agent being more knowledgeable about which actions should efficiently be taken than the principal).

Property Rights

With Kirsten Foss I have developed a research stream that attempts to apply the economics of property rights (e.g., Coase, 1960; Alchian, 1965; Demsetz, 1967), particularly the work of Barzel (1997) to management issues (see also Foss and Foss, 2008a&b). (Strategic) management scholars have not traditionally been conversant with the economics of property rights so selling these ideas has been a somewhat uphill undertaking. However, economics of property rights seems to become an increasingly strong voice in the conversation of strategy scholars, particularly in the context of the resource-based view of strategy (e.g., Teece, 1986; Oxley, 1999; Kim and Mahoney, 2002, 2005).

The economics of property rights emerged from the recognition that transactions involve the exchange of property rights, rather than the exchange of physical goods *per se* (Coase, 1960). In his pioneering paper, Coase (1960) examines the economic welfare implications of the allocation of legally delineated rights (liability rights) to a subset of the total uses of an asset, namely those that have (non-pecuniary) external effects on the value that agents' can derive from their use rights over assets. Coase argues that these implications can only be grasped if a break is made with the "faulty concept of a factor of production"; the latter should properly be thought of, not as physical entities but as right to perform certain actions (Coase, 1960: 155). These rights are property rights. Coase's paper gave rise to a spate of work on property rights and ownership (e.g., Alchian, 1965; Demsetz, 1967;

Barzel, 1997) that developed more refined categorizations of property rights, for example, introducing distinctions between use rights, income rights, rights to exclude, and rights to alienate resources. Gradually, the economic notion of property rights became dissociated from legal notions. Rights to use, etc. assets may conceivably exist in the absence of the state, legal system, courts, etc. (Umbeck, 1981). Physical force and/or strong social norms may guarantee *de facto* control.

A more recent development is the work of Oliver Hart and his students and colleagues, sometimes called the "new property rights view" to distinguish it from the work prompted by Coase (1960). As in Williamson's work, a central assumption is that because of transaction costs/bounded rationality, contracts must necessarily be incomplete in the sense that the allocation of control (property) rights cannot specified for all future states of the world. Following legal convention, ownership is defined as the possession of residual rights of control, that is, rights to control the uses of assets under contingencies that are not specified in the contract. By control is meant the ability to exclude other agents from deciding on the use of certain assets. These rights determine the boundaries of the firm: a firm is defined as a collection of jointly owned assets.

In "***Assets, Attributes and Ownership,***" Kirsten and I argue that this is a view that implicitly starts from the assumption that asset ownership is fully enforceable. However, recognizing with Barzel that assets have multiple attributes, and that these may be subject to capture in world of positive measurement and enforcement costs, implies that the notion of full asset ownership is problematic. Ownership is a function of the costs of enforcement and capture and of agents' abilities to engage in these activities. New property rights theorists sidestep these issues by implicitly assuming that residual rights of control are perfectly enforced. However, if they are allowed into the analysis, a very different rationale for asset ownership (and for who should own an asset) emerges, one that depends on costly measurement and on agents being of different types, rather than on asset ownership creating threat points under *ex post* bargaining. This is not to say that the new property rights economics is wrong; merely that it is different from the ideas of Coase, Alchian, Demsetz and Barzel, and that their approach should not be forgotten under the impact of enthusiasm over the ability to put property rights ideas in formal garb.

This is certainly also the case of strategic management: As Kirsten and I argue in "***Value and Transaction Costs: How the Economics of Property Rights Furthers the RBV"*** property rights economics brings ideas of transaction costs directly into the picture and thus illuminates the transaction costs dimensions of resource value. If resources have multiple attributes (uses, functionalities, etc.), overall resource value is determined by the benefits of these attributes, net of the costs of protecting property rights to these attributes. Given positive protection costs, some attributes will be left in the public domain. Several implications follow. For example, a "resource" is an endogenous outcome of processes of economizing with transaction costs and the proper unit of analysis may be the resource attribute rather than the resource itself. Moreover, the ability to reduce transaction costs (e.g., internal agency costs or the costs of transacting with suppliers or customers) can be a distinct source of competitive advantage. Indeed, in a broader perspective, the familiar VRIN conditions of sustained competitive advantage specify the conditions under which property rights to resources are secure.[6] In later work we have applied these ideas to understanding entrepreneurship in the context of the RBV (Foss and Foss, 2008a) and to understanding the transaction cost dimensions of competitive strategy (Foss and Foss, 2008b).

One of my most frequent collaborators has been Peter G. Klein (University of Missouri). Peter's background as a Williamson student and an Austrian has turned to fit out very well with my, roughly similar, theoretical preferences. We have co-authored numerous papers, mainly on issues of economic organization and entrepreneurship. "***Original and Derived Judgment: an Entrepreneurial Theory of Economic Organization***" is an example of our collaboration (it also includes Kirsten Foss in the author team). It employs property rights notions and notions of ownership and does so in the context of entrepreneurship inside firms. Traditionally, entrepreneurs have been conceptualized as single persons (or in some recent cases, entrepreneurial teams). This tends to suppress that an entrepreneur can make employees act in an entrepreneurial manner to further the entrepreneur's vision, what we call "proxy" or "derived" entrepreneurship. Building a simple verbal and graphical model we model how much delegation the entrepreneur should optimally choose and we argue (in line with the reasoning in the above-mentioned papers) that an important function of ownership is that it provides the authority to set optimal levels of delegation.

[6] These conditions specify that sustainability of competitive advantage obtains when the resources underlying strategies are valuable, rare and costly to imitate and substitute.

The final paper in this collection “***Economic Freedom and Entrepreneurial Activity: a Cross-Country Analysis***” (with Christian Bjørnskov) also brings together ideas on property rights and entrepreneurship, albeit on a much higher level of analysis. Essentially, the paper is an attempt to explain cross-country variation in entrepreneurial activity by means of the prevailing property rights structure, that is, economic policy and institutions. Although entrepreneurship has been much researched in economics lately, and while the same may be said of the link between economic growth and institutions, surprisingly the paper appears to be one the first papers (and perhaps *the* first) dealing empirically with the link between entrepreneurship and institutions in the economics literature, using cross-national data. The institutions/entrepreneurship link is arguable a link in the overall growth/institutions link, so for that reason exploring the former link seems important. We find that the size of the public sector and the instability of monetary policy are particularly harmful to entrepreneurial activity (another link back to my Austrian pedigree).

CODA

When approached by Matthew Pittman of Edward Elgar to undertake this project, I was initially somewhat hesitant: “Collected papers”-like volumes are usually reliable signals of careers that are approaching sunset — not necessarily a good signal to send when you are in your early fourties! In my opinion (and no doubt in some readers’ opinion!), I just got started! Another reason I was hesitant has to do with my steadfast attempt to deny Smithian economies of specialization in research: Simply put, I have written about what has interested me, which turns out to have been a lot different things, and it is not that easy to see how they are connected. After all, my intellectual heroes include thinkers as different as Yoram Barzel, James Coleman, Frank Knight, Axel Leijonhufvud, Ludwig von Mises, George Richardson, David Teece, and Oliver Williamson. I have benefited from interacting with people as different in their outlook as Geoff Hodgson and Yoram Barzel, or JC Spender and Anna Grandori, or Ulrich Witt and Oliver Williamson (and I am grateful to them all).

However, I decided to approach this as a sort of mid-career research report, and as a snapshot (or rather, brief documentary) of a series of research programmes that I have contributed to get started, programs that surely are still developing. Rather than trying to do a comprehensive tracking of my research by including only well-cited papers, I would include those papers 1) that I liked and 2) which have had a more “programmatic” function in starting some of my research programmes. Accordingly, the emphasis has been on starting points for research programmes rather than very specific manifestations of these. For example, over the last few years I have done extensive empirical work on issues of knowledge governance and micro-foundations, some of it an organizational behaviour vein. However, these research results are not reported here, either because they are still underway in the relevant journals or because they are (too) specific manifestations of an overall agenda.

I am fortunate to have had excellent co-authors, including Peter Abell, Torben Andersen, Christian Bjørnskov, Teppo Felin, Peter Klein, Thorbjørn Knudsen, Yasemin Kor, Richard Langlois, Lasse Lien, Brian Loasby, Joe Mahoney, Peter Møllgaard, Paul Robertson, Keld Laursen, Torben Pedersen, Xosé Vázquez and, last, but most emphatically not least, Kirsten Foss. A number of the papers in this collection represent joint work with a subset of these. I am happy dedicate this collection to my co-authors.

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"Knowledge-Based Approaches to the Theory of the Firm: Some Critical Comments". *Organization Science* 7: 470-476 (1996).

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