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The Evolving Dynamics of Organizational Capabilities: An Interview with David J. Teece

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Papers in Organizations – Editor’s Foreword

The purpose of the series *Papers in Organization* is to work as a stepping-stone towards final publication in scientific journals. As such, PIO is a working-paper series, yet with a distinct position in the process towards final publication. The aim of PIO is to be the final stepping-stone in that process:

- For the *author* PIO should add value to the work in progress through the editorial process. A publication in PIO is thus also a measure of the quality of the work – it is no longer simply a draft or an informal contribution to debates, but a work close to final publication.

- For the *reader* PIO should be a good place to be if one wants to keep track of contemporary research within the international field of organization studies. Indeed, many of the papers are manuscripts, which have been submitted to social science journals and as such appear in a rather final stage of completion. Others may contribute with empirical results from ongoing research projects or may in a more theoretical sense contribute to current academic disputes.

In this paper, Mie Augier provides a rich description of the intellectual traditions, the significant people and academic institutions that in some way or another made a difference to Davis Teece’s own intellectual development. In this sense, it is a dynamic account of the emerging career of a distinguished scholar - but not only that. It is also a description of the co-development of three major disciplinary fields; organization theory, economics and strategic management during three decades or so. David Teece has made several important contributions, perhaps most notably to economics (on the theory of the firm and transaction cost economics) and strategic management (on dynamic capabilities) while drawing upon organization theory and notions such as organizational routines and bounded rationality. In addition, Augier also provides an interview with David Teece, a true scholar still unsettled with what has been achieved so far - in all three fields: “Maybe I’m wrong; and maybe technology is a special case and maybe technology and organization do not belong at the core of the theory of the firm. My intuition tells me otherwise.” (David Teece, quoted in this issue).

Kjell Tryggestad/Søren Christensen
Editors

Introduction

David J. Teece (born 1948) received his Ph.D. in economics from the University of Pennsylvania and was on the faculty at Stanford University before going to UC Berkeley where he is currently a chaired Professor at the Haas School of Business, and the director of the Institute of Management, Innovation and Organization.

Teece has made key contributions to the theory of the firm and strategic management, the economics of technological change, knowledge management, technology transfer, antitrust economics and several other areas. His early work focused on issues relating to the internal organization of business firms and their boundaries and diversification, extending and pioneering the statistical testing of transaction cost economics (TCE) framework originally developed by Ronald Coase and Oliver Williamson. He imported to TCE ideas from evolutionary economics, and from Edith Penrose. Later work introduced the ideas of complementary assets and appropriability regimes (1986) in building a conceptual framework for understanding which factors influence who profits from innovation (the innovating firm, the follower, or firms owning related assets).

Teece is one of the founding fathers of strategic management as we know it today; he pioneered research on both the resource based approach and especially dynamic capabilities, thereby helping to establish the competence based perspective on economic organization. He has also contributed to related areas such as technology transfer, organization theory, intellectual property rights, and general management. At the core of his work (in particularly in the theory of the firm and strategic management area) is enhancing, testing, then synthesizing different intellectual traditions – in particular transaction cost economics, evolutionary economics and the so-called capability approach. His overarching ambition is to build a coherent and robust understanding of the central issues in economic organization and wealth creation, particularly at the level of the firm.

Particularly important to Teece's work is his deep understanding of the *dynamics* of business organization. Educated in economics, Teece draws on economic concepts; but he also uses insights from organization theory and management in developing an understanding of the dynamics of the modern business enterprise (Teece, 1984; Teece and Winter, 1984). In his later work, he provides much of the intellectual foundation for understanding dynamic corporate strategy, which has relevance for economists, strategy scholars, and managers. Thus, the evolving dynamics of

Teece's own ideas and work have significantly influenced the evolving dynamics of the fields to which he is contributing. Therefore, in trying to understand the development of his ideas we might also come to understand developments in the fields to which he has contributed, in particular the field of strategic management, a complex field grappling with many important issues.

When young scholars today begin to study strategic management, they will immediately observe that the field is in serious disarray. Like other relatively new fields (such as organization theory), there has been a tendency to encourage fragmentation and to favor new ideas rather than integration and consolidation of old ones; thus favoring what Thomas Kuhn referred to as 'revolutionary' instead of 'normal' science. This is only superficially appealing, as it does not lead to cumulative learning. What Jeffery Pfeffer (1993) observed about organization theory is therefore true for strategic management too: "there are [in strategic management] thousands of flowers blooming but nobody does any manicuring or tending" (also see March, 199, 1996). We see this manifested in the diverse approaches ranging from rational choice theory and game theory, to institutional and evolutionary theory, to post feminism and social constructivism, to name just a few. Fragmentation is encouraged. There exists a plethora of academic journals and professional societies within the field of strategy. Awards and best paper prizes are given for formulating "new concepts" but not for testing and rejecting concepts already invented. As Teece mentions in the interview below, there is too much pluralism for strategic management to be a 'research program' in the Kuhnian sense. Ultimately, this lack of integration and strong disciplinary foundations leads to a situation where the historical dimensions of the field gets lost and evolutionary opportunities missed.

James March (1991) developed these Kuhnian ideas into an organizational learning framework, which can be applied to the evolution of ideas/research programs:

"[Research programs, such as those prevailing in strategic management] that engage in exploration to the exclusion of exploitation are likely to find that they suffer the cost of experimentation without gaining many of its benefits. They exhibit too many undeveloped new ideas and too little distinctive competence. Conversely, [research programs] that engage in exploitation to the exclusion of exploration are likely to find themselves trapped in suboptimal stable equilibria" (March, 1991, p. 71).

One implication of this is that a situation of too much exploitation and too little exploration can lead (and is already leading) to a 'competency trap' situation where older theories will not be replaced by new ones, but just forgotten. The creation of an independent quasi-discipline of strategic management has many valuable consequences, but it risks separating the field from the discipline of disciplines. This separation from the disciplines has implications for the balancing of exploration and exploitation required for the long run adaptation of the field (March, 1991). It makes the field more "open" (exploration) but less efficiently rigorous (exploitation). And indeed, one of the insights from Jim March (1991, 1996) is that research programs and scholars must engage in researching the past in order to maintain a balance between the exploration and the exploitation of ideas. David Teece's 1997 paper on Strategic Management (first circulated in 1990) did exactly that. In providing a conceptual framework for mapping out and then advancing the different traditions in strategic management, he also provided us a framework for understanding the field's history and contributions to it.

Teece's ideas are considerably more interdisciplinary than those of his fellow economists. Many of his contributions stand out because they embrace ideas from several disciplines; and his research covers various aspects and levels of the modern organization. For instance, in his important contributions to the theory of corporate diversification (1980, 1982), Teece used transaction cost economics to understand diversification, building a theory of diversification around the problem of technology transfer (Teece, 1980); and trying to extend the transaction cost framework of the firm by introducing more evolutionary insights (1982). In addition, his paper on 'Organizational Structure and Economic Performance" (Armour and Teece, 1978), was (remarkably) the first empirical study to demonstrate a statistically significant link between organizational structure and performance. His paper with Monteverde (1982) was the first to establish a statistically significant link between asset specificity and organizational structure, thereby helping to transform transaction cost economics into an empirically relevant paradigm. [1]

In addition to his academic accomplishments, Teece is also an intellectual entrepreneur, an academic entrepreneur, and an institution builder. Among his activities, his has built a very successful research In-

[1] Monteverde and Teece (1982) contribution was the first study which showed statistical support for the transaction cost framework. The empirical support for transaction cost economic has since been growing. Oliver Williamson often refers to these empirical studies as indications that transaction cost economics is "an empirical success story" (Williamson, 2002).

stitute (IMIO) and a global publicly traded (NASDAQ listed) expert services company, LECG Corporation. Today LECG has over 650 employees and more than 20 offices worldwide. In building LECG, Teece has drawn on his academic ideas and experience. He has created a unique business model which (as with his writings) integrates elements of evolutionary theory, organization theory, leadership, competence based theory and transaction cost issues.[2] He also co-founded a successful private equity firm of considerable size (i-cap) and effectuated the takeover and turnaround of a publicly traded sports apparel firm. Ever since graduate school he has been a successful and very active consultant on decisions and disputes. His clients have been corporations and governments around the world.

How can we understand his ideas and contributions in the face of such diversity, spanning across disciplines and traditional boundaries between theory and the empirical world of business?

[2] Elements of LECG's highly differentiated business model are explicated in Teece (2003) and in A Stanford Business School case titled LECG and the Leveraging of Intellectual Capital: From Private to IPO to Acquisition to Private MBO. Teece has pioneered and applied an entirely new model of business organization (see below).

To be sure, Teece's work in the real world has not compromised his academic activities; on the contrary, his unusual ability to work in both worlds brings insights from business to bear in his academic research. Another man might have handled the possible tension between working in business, and in academia, by compartmentalization; but Teece has managed to channel the tension into an unusual productive program of research. In effect, Teece's interest in extending his scholarly ideas to business activities tied him to the mast, like Ulysses, and enabled him to actually enjoy the siren songs of academic disciplines (in particularly economics) without losing the critical distance so vital for interdisciplinary and empirical inquiry. Like Herbert Simon, James March and others who have been developing an 'empirically relevant' theory of the firm (see, for instance, Simon, 1997), Teece's work advances our understanding of the modern business enterprise which (unlike traditional econonomics) can accommodate ideas such as market disequilibrium, firm behavior, and the interaction of firms in markets.[3]

[3] In keeping with the Ulysses metaphor and the comparison with Simon: Considering Simon's over 1000 publications in (very) different disciplinary circles, Simon could appear to be always leaving and never finding home; always embracing a new discipline with passion and intensity, but at the same time always appearing to be moving away. Simon never really joined an established disciplinary community, preferring instead to establish his own domains (such as behavioral science, cognitive psy-

Teece has recently written about how his experience from building a world class expert services firm can contribute to further understanding of issues relating to economic organization (see in particular, Teece 2003). The business model he designed and implemented (at LECG) challenges accepted notions in human resource management and compensation theory. He has successfully designed and implemented an entirely new organizational and compensation model for professional service firms. In doing so, he has perhaps pioneered key elements of the modern (talent oriented) corporation.

chology, artificial intelligence). Domains, which were close enough to mainstream disciplines to allow recognition, yet different enough to not expecting Simon to be its follower. For, how can you follow, when your instinct is to lead? Ulysses restlessness (which we also see in the poem by Alfred Lord Tennyson, 1842) represents the restlessness of dedicated scholars as well as their fierce work ethic and interest in productivity. In the poem, Ulysses, Tennyson reveals the horror of retirement, of ease and relaxation. Ulysses (and similar spirits, such as Simon, March and Teece) wants to be productive, always rushing forward, just as in the poem "Ulysses,"(where Ulysses says: "For ever and for ever when I move. How dull it is to pause, to make an end, to rust unburnish'd, not to shine in use!"). Ulysses can't bear to stay at home and enjoy his leisure after a lifetime of adventure. He lives for knowledge, and his search for intelligence and knowledge is unending ("To follow knowledge like a sinking star, Beyond the utmost bound of human thought."). For this reason, he sets off again on new voyages; just like scholars set off on intellectual journeys, traveling into new disciplines, or new areas ("To strive, to seek, to find, and not to yield"). It is this instinct to be independent and to be different, in addition to an enormous intellectual curiosity, which allowed Simon to be so broad in the scope of his research, and which allows Teece to be able to so successfully use his academic ideas in domains beyond traditional academic adventures.

Furthermore, Teece co-founded the UC Berkeley management of technology program and has built and obtained funding for the Institute of Management, Innovation and Organization (an interdisciplinary research center). He is a co-founder and co-editor of the journal, *Industrial and Corporate Change*.

This is a remarkable portfolio of accomplishments, at least for a serious academic, and demonstrates the Ulyssian urge to strive, rather than sit still; the capacity for aspiration, and a determination for knowledge and intellectual adventures, rather than peaceful dullness. The exploratory way of the intellectual adventurer is manifested, for instance, in the search for the ideas and reflections on human rationality and decision making in different disciplines (Simon); in the attempts to develop ideas to understand organizational and individual intelligence in the face of numerous biases, imperfections and contradictions (March); and in the drawing upon knowledge and experiences in business activities in developing a coherent understanding of the dynamics of the business firm (Teece) – all aspirations that will contribute to the growth of consciousness and knowledge. Seen this way, research (be it in economics, organization theory, or strategic management) is driven by a process by which the inner quest for looking beyond the present state of knowledge fuels the outward journey and, in turn, the outward journey illuminates inward realization (this is consistent with March's theme of Don Quixote, see for instance March 1996). The cycle is endless; because the possibilities of growth of knowledge are endless.[4] The mind of dedicated scholars is always searching for knowledge of what always surrounds and extends well beyond our current reach.[5] Yet, as we continue to penetrate, we leave behind us the skins of our former ideas and theories, and embrace an increasingly greater potential. As we choose to do so, we join in spirit with Ulysses, and push science forward.[6]

As an introduction to the interview below, I will summarize some of Teece's most significant ideas and contributions, which will be discussed in the interview.

[4] Tennyson's Ulysses invites us:

"Come, my friends,
'Tis not too late to seek a newer world.
Push off, and sitting well in order smite
The sounding furrows; for my purpose holds
To sail beyond the sunset, and the baths
Of all the western stars, until I die."

[5] This also is the theme in March's 'A Scholar's Quest' where March talks about man seeking knowledge and desiring "for its own sake, the conformity of his own character to his standard of excellence, without hope of good or evil from other source than his own inward consciousness".

[6] In more poetic words (Tennyson's), this is when we become:
"One equal temper of heroic hearts,
Made weak by time and fate, but strong in will
To strive, to seek, to find, and not to yield."

Intellectual Formation and Early Work

Teece began studying economics in 1967 at Canterbury University in New Zealand, before going to graduate school in the US. He acquired very early an interest in understanding the organization of business firms; and was also interested in international economics. From his father, who was a business man who had founded a trucking company, he gained insight into some elementary issues in management. This planted the seeds for the ideas he has later developed in his contributions to business strategy and the theory of economic organization.

Studying economics at the University of Pennsylvania, Teece learned under very respected faculty such as Steven Ross, Almarin Philips and Edwin Mansfield. He did his dissertation with Ed Mansfield on international technology transfer. He was not a student of Oliver Williamson as many assume, but he was clearly influenced by Oliver Williamson's pathbreaking work in transaction cost economics.

Multiproduct Organization

While Teece was already as a student interested in the theory of the business enterprise, his interest in this subject was further stimulated when during his graduate years he read a manuscript version of Oliver Williamson's first major work in transaction cost economics (Williamson, 1975). Williamson followed Coase in viewing markets and hierarchies as alternative structures for organizing transaction, and introduced the idea of transaction costs as the major determinant of the boundaries of the firm. This was a major inspiration; and Teece felt that this helped solve puzzles economics had been unable to explain such as the existence (and scope) of the firm (Teece, 1984). Since neoclassical economics was unable to deal with firms (other than as production functions), the transaction cost framework expanded the explanatory power of economics by integrating ideas such as bounded rationality, incomplete information, and small numbers bargaining. It was natural for Teece to begin to apply transaction cost ideas to the study of vertical integration in specific industries (Teece, 1976) and diversification (1980, 1982).

In building a theory of the multiproduct firm, Teece used ideas from Edith Penrose (1959) to build an economic theory of the firm which could accommodate the multiproduct character of the modern firm (Teece, 1982). Until his work, economic theory could not explain diversification well. Until Teece's work, market power and managerial explanations were in vogue. These could explain little. Indeed, Panzar and Willig's work (for instance, 1981) on economies of scope did not have strong organizational implications at all. Thus, Teece argued that while those theories could explain aspects of joint production, they couldn't explain why firms adopted multiproduct structures over outsourcing or joint venture arrangements. In his paper "An Economic Theory of the Multiproduct Firm", Teece introduced ideas from evolutionary economics (particularly Nelson and Winter, 1982), and from Edith Penrose.[7]

As recently argued (Rugman and Verberke, 2002), Penrose's legacy in strategic management is a curious one. Much cited, but little read, her work is recognized as one of the main intellectual foundations for modern resource based theories of the firm and strategy. However, Penrose wasn't much interested in contributing to the field of strategy per se; and numerous misinterpretations (or misreadings) of her work do not seem to acknowledge that her main contribution was to understanding the nature of the firm and its growth (not strategy); and that firms can be viewed as a collection of resources. Teece's paper on the multiproduct firm was the first to apply Penrose's ideas to strategic management issues, and

[7] Edith Penrose is now widely recognized as a precursor for ideas in the competence based theory of the firm and strategic management (Rugman and Verberke, 2002); but in 1982, she wasn't cited at all. In fact, Teece's work brought her to relevance for modern theories and he was the first to use her insights in developing the resource and competence based perspectives on the firm and strategy.

he focused on her observation that human capital in firms is usually not entirely 'specialized' and can therefore be (re)deployed to allow the firm's diversification into new products and services. He also used Penrose's view that firms possess excess resources which can be used for diversification (1982). Later, Wernerfelt (1984) cites Penrose for "the idea of looking at firms as a broader set of resources … [and] the optimal growth of the firm involves a balance between exploitation of existing resources and development of new ones". And this basic idea is now the foundation for much of the research by scholars working on the resource based theory of the firm (see for instance, Prahalad and Hamel, 1990).

When Teece first introduced this perspective in 1982, it was built directly on ideas from transaction cost economics, evolutionary economics and from Edith Penrose; but at a deeper level, we may also see Teece as a 'grandchild' of the behavioral theory of the firm – tradition emerging out of Carnegie Tech in the 1950s and 1960s.[8] Like Herbert Simon, Teece builds on ideas on bounded rationality and is interested in developing a more realistic and empirically relevant theory of economic organization (see, e.g. Teece 1982, 1986, 1996). Like Richard Cyert and James March, he uses ideas of "conflict" and firm heterogeneity (Teece, 2003; Teece et al, 2002). In addition, he has recently carried insights from the behavioral theory of the firm into the tradition of strategic management (see in particular Teece et al, 2002; and Teece, Rumelt and Schendel, 1991).

His paper, 'Profiting from Technological Innovation' (Teece, 1986) also integrated insights (such as the tactiness of knowledge and the nature of innovation) not traditionally on the radar screen of economists. The paper (which is the most cited paper ever published in 'Research Policy') developed a framework for understanding why (and under which conditions) innovating firms may fail to obtain significant economic returns from an innovation while customers, imitators and other industry players benefit, focusing in particular on the role of (the ownership of) complementary assets, regimes of appropriability, and the evolution (and paradigmatic character) of industry development (in particular the role of dominant designs).

Strategic Management

At a time where the field of strategic management was, at best, very scattered Teece began extending his ideas on the theory of the firm to business strategy. He was

[8] The direct intellectual link goes through Oliver Williamson, who was a graduate student at Carnegie in the early 1960s; and through Sid Winter and Dick Nelson who were closely connected to the Carnegie group in the late 50s and early 60s.

among the first to argue that a theory of strategic management can build on insights from economics, while realizing that many shortcomings of the neoclassical program made it necessary to include alternative approaches (Teece, 1984; Teece and Winter, 1984).

That strategic management can build on theories of economic organization in general and theories of the firm in particular is now well established. But this was not always so. When Rumelt (1984) talked about "a strategic theory of the firm", arguing that the study of business strategy must take off from economic theories of the firm, the dialogue between economic theories of the firm and strategic management was largely absent. The tensions between neoclassical theory of the firm and strategic management included the treatment of know-how, the emphasis on dynamics vs. statics, and the differences in behavioral assumptions (Teece, 1984; Winter and Teece, 1984).

It wasn't until the mid 1980s that strategy scholars began to realize the usefulness of recent developments in organizational economics (in particular transaction cost theory and evolutionary economics) (Teece, 1984). Teece (1984) thus indicated how capability considerations may be further integrated with transaction cost arguments in order to enrich strategic management research, an approach that was started with Teece (1982). Another argument that developed (and is now well established) was that integrating economic theories with strategic management could address issues, theoretical as well as practical, with regard to questions of firm boundaries and organizational design (1988).

In the paper "Dynamic Capabilities and Strategic Management" (Teece et al, 1997), Teece developed a framework of understanding the different intellectual traditions which can be classified as strategy research: The competitive forces theory espoused by Michael Porter (which focused on the structure of markets and the nature of competition in different industries); the game theoretical approach to strategic management (which argued that firms could gain competitive advantage by a series of strategic moves); and the resource based perspective. In addition to those three major traditions, Teece suggested a perspective which focused on the kind of capabilities that firms must acquire to establish competitive advantage in industries with rapid technological change (also see Teece and Pisano, 1994). This provided a conceptual framework for mapping out the field of strategic management, which is now widely accepted. It helped facilitate a dialogue between the different traditions, and organized the conceptual content in the field.

This is important not only as an aid to our understanding of fundamental issues in the management of organizations but also in terms of it's potential for future research in strategy.

Perhaps a more well known outcome of this paper was the introduction of the term 'dynamic capabilities', an idea which is currently enthusiastically employed by strategic management scholars. In fact, a quick look at the citations demonstrates that dynamic capability was popularized before the publication of the original paper; it had circulated for seven years as a working paper in multiple drafts before it appeared in the *Strategic Management Journal*.

The dynamic capability approach builds on both the resource based, transaction cost and evolutionary and behavioral theories of the firm, and seeks to explain how firms achieve and sustain competitive advantage in an ever changing environment. High performance of internal processes (or, in Cyert and March's terminology, 'standard operating procedures') are critical. So is the ability to sense and seize market and technological opportunities. Routines define the tasks of the organization; how the organization solves problems, and how tacit knowledge translates into learning.

The idea of routines traveled from Cyert and March to the work of Nelson and Winter and, from there, into strategic management (Teece et al, 2002; Winter, 2000). Moreover, building on the idea of standard operating procedures, Teece has developed the idea of dividing a firm's competence into allocative, administrative and transactional elements. The dynamic capability view of strategy emphasizes that the "key role of strategic management in appropriately adapting, integrating, and re-configuring internal and external organizational skills, resources and functional competencies toward a changing environment" (Teece and Pisano, 1994, p. 57), thereby building on behavioral ideas of adaptation and the dynamic character of expectations and goals. It also follows the behavioral view in seeing learning as an organizational process; "[w]hile individual skills and knowledge can contribute critically to the organization, learning processes are intrinsically social and collective" (Teece et al, 2002, p. 90). Moreover, "[a] more specific application of [behavioral ideas] in the dynamic capabilities literature is the importance of routines in identifying and exploring opportunities" (p. 91). Through mechanisms such as uncertainty avoidance and problemestic search influencing the standard operating procedures of the firm, a firm's organization and performance is uniquely influenced by the nature of decision making; as is the firm's strategic behavior. As emphasized by Cyert and March (1963) and Simon (1955, 1993), firm decision making and strategy depends on the firm's ability to identify decision opportunities, create them and to act on them facing bounded rationality and uncertainty; (cf. Simon, 1993: "strategic decisions is a chapter in the topic of decision making under uncertainty"). In keeping with this perspective, the dynamic capability view emphasizes that dynamic capabilities of a firm depends on both its ability to identify strategic opportunities and its ability to change the structure of the firm to better exploit those opportunities (Teece et al, 2002, p. 92; Teece, 2004).

The future relevance of strategic management will depend on whether future developments in the field will bring us closer to an empirically relevant paradigm, which can accommodate and address issues relating to the dynamics of the business enterprise. This in turn will depend on the ability of the scholars and ideas within strategic management to work together and for the research program to accommodate an interdisciplinary vision, and to be disciplined (March, 1996). As Teece points out in the interview below, such a (interdisciplinary, yet disciplined) vision is the first step toward realizing a coherent program in strategic management; and we may see the dynamic capability program as taking the first important steps toward establishing a coherent and rigorous research program in strategic management. The dynamic capability program integrates ideas from Teece's previous work, and sets a research agenda for future studies in strategic management.

The following conversation took place March 25-May 28, 2003, at UCB between David J. Teece (DT) and Mie Augier (MA).

***MA**: First a few background questions. How and why did you become interested in economics in the first place?*

DT: Well, I guess I was lucky in the sense that while economics wasn't taught in secondary school in New Zealand, I had the good fortune of having a brother who was studying economics and he brought home a copy of Lee Bach's introductory textbook (Bach, 1954). I picked it up and started reading it and there were ideas that I found very interesting and so I told myself that this was the subject that I wanted to study.

***MA**: Did you already then have an interest in the firm and in management?*

DT: Yes, my father was a manager and a director of, what I thought at the time was a sizable company. He had started his own company when he was 18; a small trucking company and actually pioneered a thrice weekly freight service between the west coast of the Southern Island of New Zealand and the northern part of the Island. He went overseas (to North Africa and then Italy) during the second World War as part of the New Zealand Expeditionary Force. Before being deployed to fight alongside the British in North Africa, he became a founding shareholder in Transport (Nelson) Ltd.

He never talked about management but I knew viscerally that he was a respected manager, known for his fairness, objectivity, operational skills, and bottom line focus. My father never said 'oh you should study economics or management'. But merely observing him got me interested in some of the issues. Even in high school I was very much interested in public policy and the competitiveness of nations. I was also interested in international trade. Growing up in a small country there is very much an interest in external economic forces. New Zealand was and remains highly dependent on international trade. The two areas of specialization I chose in graduate school in the United States were international economics and industrial organization.

***MA**: Why did you choose to go overseas and study at Wharton?*

DT: I was very fortunate to grow up in New Zealand. As an undergraduate, I discovered that I wasn't a good athlete, but I did do well academically. One has to focus on what one is good at. I loved to learn. It was an escape. So it was natural for me to want to do graduate study. I needed a stepping stone to embrace the larger world. Living in a small country was lovely but a bit limiting. I was very much provoked by things external to my daily life.

A key question one might ask is, how did I ever get to a world class place? It's a long story. As I was growing up in New Zealand, I was living, in a virtual sense, in the rest of the world. I listened every night to short wave radio. It was one way communication (listening) but I felt connected to the external world. Today, kids are chatting on the Internet with other kids from the rest of the world. So, even though we didn't have chat rooms and all the technology of the Internet, we had radios and I had several short waves radios and a hugh antenna. I tuned into the BBC, Voice of America, and Radio Beijing almost every night. The Chinese were broadcasting lots of revolutionary garbage. Talking about the American "bandits" in Indochina. Some of it was comical. Voice of America was a bit dreary but had good coverage of the space program and foreign policy speeches of the Secretary of State.

When I was considering graduate school, the comfortable thing to do would have been to stay in New Zealand, but I didn't want that. It was a challenge to go oversees; and to go to the United States was a special challenge. Students in New Zealand wanting graduate study abroad in economics would traditionally go to the London School of Economics, or to Cambridge, or to Oxford. That was also where most scholarships were set up. But I got advice – which was good advice – that there were good possibilities in the US and that the future of the field of economics was in North America. Canada actually had much greater visibility in New Zealand, and many would go to UBC. Indeed, I had never heard of Wharton until 1970. Wharton didn't actually require a GRE for students coming from New Zealand. That's one reason I applied! I'm sure it was possible to take a GRE in New Zealand but I had never heard of the exam at the time. So I didn't apply to the schools where the test was required - - - which was almost everywhere. Somehow I got into Wharton. And I got a fellowship. I was really lucky. I had no idea how good a place it was until much later.

***MA**: What can you tell about your early graduate years? Your advisor was Mansfield but you also worked with Oliver Williamson?*

DT: I liked Penn from the beginning. Philadelphia took a little getting used to. I found it easy studying micro theory and macro theory because, unlike many other students, I had already had four years of economics.

At that time, the economics department at Wharton took in many students and only about half would make it through the first year. The first year's exams were designed to sort the wheat from the chaff. Even though I wasn't strong in mathematics, I got by. And then I ran into Olly. I never took a class from him; many people assume that I was a student of his, but I never was a direct student. I took IO with Almarin Philips, and I was interested in theories of market failures and the appropriate role for government. In fact, that was one of the reasons I liked Olly's work. He built on the ideas of market failures, which I knew from the study of welfare economics.

I also took Edwin Mansfield's class on the economics of technological change and he encouraged me to study international technology transfer. Because of my background in international trade and finance and economic development, I was interested. But no one, not even Mansfield, knew anything at all about the topic.

I ended up with Mansfield as my advisor. He had decided at that time that he didn't like economics and had a strong distain for theory so it was actually hard doing a thesis with him. But I did it. It was an empirical study of the costs of technology transfer, and was completed before the modern literature on the nature of technology existed. I was writing on a blank slate. This was hard to do as a student.

Mansfield was a pragmatic and brilliant scholar. However, I think his own work would have been more powerful if it has been linked to a broader framework, such as the theory of the firm or the evolution of technology. But that was not how he worked. I did however learn a lot from Mansfield. He had a good nose for data. My thesis was published as a book with a couple of papers being spun off into journals - - - one paper in the *Economic Journal* and the other in *Management Science*.

***MA**: One of the books that seem to have influenced your early work is Markets and Hierarchies (Williamson, 1975). Do you remember when you came across that in the first place?*

DT: Yes. It was that book that made me feel that here was a chance to build a half-way decent theory of the firm. I had been thinking about the international firm. I was aware of Hymer's work. Olly's manuscript provided a brilliant conceptual apparatus for organizing my thinking about firms. It didn't help much in terms of understanding innovation, but the framework was rich and deep. Having had Mansfield the pure empiricist as my advisor I needed some conceptual apparatus to understand business and the multinational firm.

So, I was in my final year and I was waiting for Mansfield to read my thesis because he was always a little bit slow in getting to it – just as I might be a bit slow in getting back to my students. I had nothing to do for a short period and I went by Oliver's office and volunteered my time as a research assistant and he gave me a manuscript version of Markets and Hierarchies. He said 'come back when you have read this'.

This was in 1974, I think. He gave me the manuscript, I read it and I immediately appreciated and understood its importance, even though I had not taken a course from him. I knew the foundations - - - market failures, Akerlof's market for "lemons", Simons' bounded rationality. I was familiar with the bits and pieces and I thought *Markets and Hierarchies* was magnificent.

I went back to Olly and said, "this is a great book. This is the framework we need to understand firms." He didn't disagree. It provided powerful new insights into what firms were all about. I told him so and I feel good about that today. I think I still know it when I read something that is a major contribution!

I jumped on Olly's bandwagon. I was able to give him a little bit of assistance on the two chapters on innovation … those two chapters sort of hang out on a limb and are not fully integrated into Markets and Hierarchies. Note that when you come to the *Economic Institutions of Capitalism* the topic of innovation disappears. Oliver kind of solved the problem of intellectual coherence by throwing out the innovation chapters... that is why I often say to people that I think *Markets and Hierarchies* is the richer book. The *Economic Institutions of Capitalism* [Williamson, 1985] is tidied up so that economists would find it more palatable. And indeed, Grossman and Hart and many others have jumped onto the idea of specialized assets, and built upon Olly's work. However, what they have is not a theory of the firm to me. *Markets and Hierarchies* has this richness; it has a Carnegie flavor to it. All the essential elements of the economic institutions of capitalism are there, and more. I think that book was transformational.

***MA**: Were there other scholars who inspired you at the time?*

DT: Yes, many. Mansfield, Kuznets, Rosenberg, Kindleberger, Koopmans, Akerlof, Arrow, Baumol, Leontief. But by the time I had graduated, it was Olly who I admired the most. By now my field was Industrial Organization. Standard industrial organization – built around the structure conduct performance paradigm – was very sketchy with respect to what firms were all about. I hadn't read much of Chandler yet – I read Chandler subsequent to reading *Markets and Hierarchies* – so for me *Markets and Hierarchies* was a great *entre* into the theory of the firm. I sort of knew at some level that this was critical to economics and business studies. I still believe that one of the big shortcomings of economics is the theory of the firm, despite its recent progress. Of course, there's been considerable progress in the last 15 years on the theory of the firm. But it has slowed, and there is a lot more distance to travel.

***MA**: You didn't read any Simon or March when you were in graduate school?*

DT: Well, I sort of backed into it through Williamson's books and references. You know, you kind of got the flavor of it from reading Williamson. So I didn't read them very carefully in graduate school. I did so subsequently. The year that I was doing industrial organization Oliver was visiting in the UK. The IO course was taught by Al Philips who was much more interested in classical antitrust than Olly was. And I'm sure if I were to find Al's original reading list from that course, there would be nothing of March and Simon on it. But I came to them from Williamson and I followed up and read more but never really had any instruction on it – I do remember buying 'A Behavioral Theory of the Firm' and 'Organizations' and reading them. It could be when I just came to Stanford. Lee Bach had been Dean at Carnegie and he wanted someone at the business school at Stanford who was interested in opening up the "black box" of the firm. He knew I had been influenced by Mansfield and Williamson. I'm sure my interest in the firm and the esteem with which he held my mentors was one reason I got hired.

MA*: I'd like to know how and why you came across Penrose's work. You're the first one to use her in theories of the firm/strategy so I was wondering how you picked her up?*

DT: Yes, no one had read her. If I remember it right, it might have been Sidney Winter who told me I should read Penrose. It must have been around 1978 or something like that. I think it was after Sid read my paper on 'The Economics of Scope and the Scope of the Enterprise' (Teece, 1980). And no one knew anything about Edith Penrose or her work. It was difficult to find the book. She had zero visibility in economics or management. It is hardly a claim to fame but I am quite sure that you find no references in the research by faculty at business schools to Edith Penrose until the publication of my paper 'Towards an Economic theory of the Multiproduct firm' [Teece, 1982]. Birger Wernerfelt then picked up on it I think and he was perhaps the first one in strategy to use her ideas [Wernerfelt, 1984]. I didn't consider myself to be in strategy back then. I was just in industrial organization and business economics.

The other person I was talking to in those days was Dick Caves at Harvard who was working with Michael Porter. I was slow to figure out early on why there was all this excitement around Porter's work. I didn't know enough about strategic management at the time. The contribution was not to economics; the contribution was making ideas from industrial organization really useful to managers. Porter was excellent at translating ideas from old school industrial organization and marketing and converting them into a form so they were useful in a management context. He provided a wonderful framework for doing industry analysis. However, I believe that stuff is a bit overblown in terms of its importance to the history of ideas. It's unquestionably very utilitarian.

***MA**: We'll get back to strategy later. I want to go back to your dissertation. How did you find a topic?*

DT: Well, you see I had two majors; you were required to major in two fields back then – so I had industrial organization and international economics. In fact, when I applied to Penn – they had this regional economics program, so I was going to do regional economics [laughs]. Remember as an undergraduate I had also studied geography. But I always had a good grounding in international economics and Mansfield picked that up and he was going to some conferences I think in Europe where technology transfer issues had surfaced in the defense-contracting context. He wanted to get someone working on technology transfer. Ed always had a good nose for what was important.

One of my term papers was on the theory of direct foreign investment. Technology transfer was implicated so that's how I indirectly got into the theory of the firm, coming in through trying to explain direct foreign investment and technology transfer. Stephen Hymers work was a break from tradition. He helped bring industrial organization and international economics together in a theory of direct foreign investment and multinational enterprise. That was revolutionary. However, Hymer got all muddled up in ideas of monopoly "exploitation" and confused the theory of direct foreign investment as much as he illuminated it.

Anyway, there's now a whole literature in international business and the theory of the multinational firm. That's one place where the study of technology transfer belongs. Back then, my thesis didn't really make the link between technology transfer and the theory of the firm because I was too 'Mansfieldian'; I missed an opportunity. I'm still working on it. And indeed, when you look at 'Economics of Scope and the Scope of the Enterprise' (Teece, 1980) and 'Towards an Economic Theory of the Multiproduct Firm' (Teece, 1982), they are early efforts to bring technology and know how into the theory of the firm.

If *Markets and Hierachies* had been written a couple of years earlier… if that had been published before I did my thesis on technology transfer, I think my thesis would have been much more interesting. But it went the other way. I've subsequently tried to bring technology and know how into transaction cost economics and the theory of the firm.

But anyway, I picked up on Penrose and worked her in as well, and she suddenly became popular. Few people in strategy actually read her; they seem to just guess what she said. Unfortunately, you have many derivative scholars in the field of strategy who never go back and understand the original stuff. This is a major weakness in training in the field. There are too many wannabe scholars not quite up to it. They need to read the literature more carefully and build deep disciplinary roots.

***MA**: When you were done with your thesis, what did you think you would study next?*

DT: By the time I got my thesis done I had this interest and knowledge in technology transfer but I did not know a single soul, not a single person, in the economics profession who was interested in technology transfer. I did subsequently meet Nathan Rosenberg - - - the only other one. I didn't know Dick Nelson and Sid Winter at the time. However, I shouldn't complain because I did get my thesis published as a book and the key article published in the *Economic Journal* so there was a constituency interested, but it was outside mainstream economics.

It wasn't very clear where to go next with my research because I didn't have a conceptual framework. I had results but no theory to motivate them. And it left me at a dead-end. Actually, I did do another paper coauthored with Mansfield; he wanted to look at R&D activities in the multinational firm so that was another empirical paper. But I needed to backfill.

Where Williamson's ideas came in is that they provided me with a conceptual framework. With the lens of transaction cost economics, I dove into Carnegie School ideas with encouragement from Sid Winter and Dick Nelson and I found a way to bring the technology story into the theory of the firm. There wasn't in fact any literature with know how at the core of the theory of the firm. Neither Cyert nor March or Wiliamson were oriented that way. Nelson and Winter weren't really working on a theory of the firm. There was an opportunity to put technology and know how into the theory of the firm. That's what I tried to do. In fact, most economists still leave it out. If you read Groseman and Hart there is no references to technology, other than as a special case of hold-up.

So, Williamson doesn't have innovation and knowledge issues at the core .. nor for that matter do March and Simon. Maybe I'm wrong; and maybe technology is a special case and maybe technology and organization do not belong at the core of the theory of the firm. My intuition tells me otherwise. Without having technology and know how at the core, I sometime wonder whether one really has a theory of the firm. One has a theory of the boundaries of the firm. But, I'm not sure one has a theory of the firm. Oliver certainly has a theory of vertical integration; but a theory of vertical integration is not a theory of the firm. A robust theory of the firm must be able to explain business competence, behavior behavior, and business strategy too.

***MA**: Nelson and Winter – what did they add to your Penrose-Williamsonian frame?*

DT: How did I get to it …. Well, Sid came and gave a workshop at Penn when I was visiting there. Brilliant guy. I got to talk to him and he started moving me towards evolutionary ideas. He and Dick latched onto me as a potential fellow traveler. Dick and Sid helped give a new direction and energy to some of the things I was playing with. I gave them the manuscript version of 'The Economics of Scope and the Scope of the Enterprise' (Teece, 1980). The core idea is about finding failures in the market for know how and using that to explain diversification. When they got that they said 'gee, this is interesting'. I had innovation in there but it was very Williamsonian. They encouraged me to extend the paper, which I did. I did get some very useful ideas from them.

'Toward an Economic Theory of the Multiproduct Firm', is much richer as a result of their comments. It brings in Penrose, and it is also much more in the Carnegie spirit. I introduced organizational slack into the diversification story. I see it as sort of a dynamic version of 'Economics of Scope and the Scope of the Enterprise'.

***MA**: You went to Stanford; why didn't you go to MIT? One thinks of MIT when you think technology.*

DT: I almost did. That was very interesting actually because my two key offers were Stanford and MIT. MIT was joint – half in economics department and half in the Business school. At Stanford it was in the Business school with a courtesy appointment in economics. And when I asked my advisors, they would all say I should go to MIT. Because if you asked any economist back then, and possibly even now, MIT is the stronger economics department. It was certainly the number one economics department in the country at the time. So, everyone at Penn said 'you must go to MIT'. And in fact, I accepted the job at MIT and called up Stanford and said I was going to MIT. I got hold of Lee Bach. He said ‘would you mind telling me the reasons’ so I did. He said ‘would you mind me telling you why those reasons are wrong’ and I said “no”. So he told me why they were wrong reasons, and he was right, so I said ‘ok, I’m coming to Stanford’ so I called up MIT – this was all within an hour – and said I had changed my mind. That was it.

***MA**: Did you ever regret?*

DT: No, it was the right decision for me. Really, I think the reason I wanted to go to Stanford was because it was a 100% business school appointment; MIT was sort of a business school appointment but they thought it would be a more attractive deal to me given where my head was at the time if they threw in a half appointment in economics. The chances that I would have ever gotten tenure at MIT economics were low, I believe. In hindsight, I think they were just using the economics offer to induce me to come to MIT. I would have had great colleagues in Joskow and McAvoy. But I really wanted to study business, not just public policy, or regulation.

Also, at Stanford, I was free to do my own stuff and at least initially I wasn’t thinking about other people’s research agendas or what other people wanted me to do. There was really no one there with my interests and I didn’t mind at first. I’ve always been a self starter. At that point the school was trying to figure out what to do with business economics so some people would say that was not a good place to start a career because there weren’t any strong senior faculty. At MIT there was strong senior faculty but no one was critically interested in technology and the firm. Now, it is conceivable that I could have latched onto Paul Joskow –but Paul would probably have enticed me to go deeper in regulatory economics. So, I think the Stanford thing gave me the chance to have the plusses and minuses of not having any senior faculty. And I had good colleagues in the profession – Olly, Sid, Dick, and Joe Stiglitz and Nate Rosenberg. I related to them much more because there was no one at the Stanford Business School with my interests. Lee Bach was always most encouraging and understood the importance of building a faculty interested in getting inside the “black box” of the firm. But his interests were fundamentally in macro economics.

***MA**: What did you teach at Stanford?*

DT: I taught an MBA class on economics which was very interesting because Stanford had been using a principles of economics-book in the MBA core. I used Mansfield’s applied micro book instead. I thought I was going to teach all these smart

MBA's all about business and firms with an intermediate micro theory book. Now, I was 26 or 27; the students were on average 30, they had business experience and they asked me all those great questions about firms. And there were no answers to those questions in the microeconomics text book. They were all good, legitimate questions and so I quickly became disillusioned with intermediate micro theory primarily from finding its inadequacies around the firm. I then offered an elective course called 'the economics of the enterprise'. In fact I have a book proposal with that title accepted by Harvard University Press. It's 20 years old now! I never really got it done. It is a book I should still write and I'll send it to Harvard. They gave me a $3000 advance for it [laughs]. It is still a good title, and it is a book worth writing. You know, 'managing intellectual capital' is not that book; so I should come back to do that over the next ten years. If I don't, I'll send them back the $3,000.

***MA**: Why didn't you stay at Stanford?*

DT: I think the issue was crystallized when I tried to get Olly Williamson to Stanford. I tried very, very hard and succeeded in persuading the GSB to make an offer to Olly. And here's what was going on: the business economics group in the business school was rudderless. There were no senior faculty. There was decision sciences group, a business and the environment group, but no strategy group. Lee Bach was there. He was a macro person. He had been the Dean at Carnegie during the great years and really wanted to build competence around organizational economics. So they started a search process and there were very, very different views in the school about what economics in a business school should be.

I had one very clear view which was it should be around Williamson and Nelson and Winter; and Bach was supportive of that but the guys in finance and the guys in decision theory were not enthusiastic about that at all and they really wanted the group to be more of an applied game theory group. I nevertheless managed to persuade people that Oliver was sufficiently good that we should make him an offer and they did. It wasn't a particular good offer, but it was an offer. And Olly was seriously considering it. He came back and tried to bargain for some extra things and they said no and so Olly didn't come. So, a chance to really shape Stanford by having Olly accept disappeared. What happened is that the finance and the decision sciences guys said 'well, Bach and Teece, you clearly can't build a group – so let us do it'. So they built a great group of applied game theorists and really missed an opportunity to create strength around the firm, around technology, and around strategy. They still struggle with the ramifications of those decisions today.

For a while, I was a high-flyer at Stanford. David Kreps and me were the first two to get accelerated promotions to associate professors without tenure. I was there for only two years I think before getting this accelerated promotion. I was on fast track! Then I started to see, ok, this group is going in a quite different direction. My chances of getting tenure were going down not up. So I asked for an early tenure review, given that I had been advanced to associate so early. I figured that if I was going to stay there I needed tenure because they were recruiting faculty who were not sympathetic to my research agenda. I was also very cocky because I had offers from Wharton; I had soft offers from all over the place. It wasn't worth fighting against a constituency that wasn't interested in organizational economics and technology. Meanwhile, Berkeley came up with an offer. And then a most wonderful thing happened to me. I applied for an associate professor with tenure position. The offer came and I accepted. But within 30 days of my arrival at Berkeley I opened the mail, and there was a letter from the Chancellor, saying that I was being immediately promoted to full professor! The university review committee decided (without me or my school asking) that I ought to be a full professor. A complete bluebird. Everyone deserves one of those somewhere in their career.

***MA**: That's wonderful. After how long?*

DT: Effective from the date that I arrived.

And when I went to Berkeley I immediately started working on getting Olly Williamson hired which I did achieve, with the help of others of course. We have a great group now in organizational economics. The community includes Pablo Spiller, George Akerlof, Richard Gilbert, David Mowery, Howard Shelanski, Bob Merges and many others.

***MA**: You wrote up Oliver's case when he got hired?*

DA: Yes I said in there that he was going to get the Nobel Prize, long before it was common talk. … he should get it. He will get it.

So, I was very happy to get an accelerated promotion. I was advanced ahead of my own expectations. It made me an extremely loyal citizen of the University of California.

***MA**: Your 86 paper is the most cited paper in Research Policy. Can you tell a bit about the pre-history of that paper?*

DT: I don't remember how I came up with the idea but I do remember presenting it at a Stanford OB conference at the Asilomar conference center. I was at Berkeley at the time. I presented an early version of the paper (this must have been the early 80s) and it made no impression on the audience whatsoever. I think it was the wrong audience. I continued to work on the paper and then I presented it in Venice at a conference Giovanni [Dosi] organized. The audience there was a technology policy audience. After I gave the paper Dick Nelson stood up, looked around, and said 'we've just heard a very important paper'. That surprised me; but it made me happy! Keith Pavitt was there too and asked me to submit it to *Research Policy*, which I subsequently did. Dick said that it was a conceptually important paper. I had long been aware of the British success at invention coupled with failure at achieving subsequent commercial success. Historically this was always explained by reference to macro factors like access to capital and public policy. In the later 80s the same thing started to happen in the US. You know, the US firms were very good with the early stages of innovation but the Japanese were winning in the global market place. That was the story. No one could explain it well. There were all kinds of stories around macro economic issues and the costs of capital; but I wasn't satisfied with these explanations so I sought to find another framework. To me, the question of understanding innovation really required a much more sophisticated firm level theory, and that's why I developed the framework. At that time, everyone was interested in "competitiveness". But there was a lot of hot air and few frameworks to help organize peoples' thinking.

There were (at least) four sets of ideas in my head that formed the basis of my paper. One set of ideas around the innovation cycle which came from Abernatny and Utterback and so forth. A whole other story about tacit knowledge, intellectual property, and immitability, and how it impacts strategy. Third, a set of ideas around transaction costs. And then I also was thinking about complementary assets which until that point weren't recognized; no one was thinking about them. And I thought about the nature of knowledge too. The paper really integrates many of my early ideas.

After Venice I became smarter in figuring out the natural audience for the paper. I started to realize that the paper had legs. I remember one entrepreneurship conference where I presented it to 2000 venture capital/entrepreneur types. Afterwards I got mobbed. About 100 people left me with cards because they wanted copies. I almost felt like a rock star for the first (and last!) time. It was great. Anyway, I'm glad to see the ideas being used widely today. I think I could do a slightly improved version today. There's still not really a competing paper out there. It's coming up on its 20th anniversary. It still generates tons of cites.

***MA**: When did you begin IMIO?*

DT: Well, I got to Berkeley in 1982. In 1983 there was this research unit – center for research on management – and they were looking for a new director. The director for many years had been a fellow by the name of West Churchman. Churchman was the father of the field of systems thinking. He and Fred Balderstom before him had created and built a great research center in the 1950s and 1960s. It had been one of the first centers for the application of computing to business. They had big NSF grants supporting their research. But by the mid 70's it was completely run down. There was no activity or money left in it. So the University gave me this research center shell and asked me to rebuild it. I started new initiatives around innovation and organization theory. At one point I had several big grants from the Sloan foundation. Before I started LECG I put an enormous amount of energy into the institute. We created and funded a lot of programs. We supported research by students and faculty all over campus and at other campuses too. I was essentially using the study of competitiveness as a way to get funding to work on innovation and organizational change and things like that. I wrote this very long research proposal. I sent it to The Sloan Foundation and got several grants. We shared some of the money with Stanford (Nate Rosenberg) and Columbia (Dick Nelson) and Harvard (Dick Rosenbloom).

It was a very productive and important period for research in business schools. In fact, I think we helped trigger changes in business education. I'm quite sure of it. Many business schools had become too theoretical. You couldn't tell the difference between a business school professor and an economics department professor. These grants encouraged empirical work and really changed the focus and got business school academics to understand the computer industry, the steel industry, and so on. Much research followed the specific industry studies. The consortium on competitiveness and cooperation was the lead program. I got it started and David Mowery took it over. We raised 2 or 3 million dollars. David did a great job. Over the years I began putting more and more of my efforts into LECG. Putting efforts into university work can sometimes be a thankless task. Jim March captures the dilemmas well in 'A Scholars Quest' [March, 1996] with his statement that 'research is not an investment, it is a testament'. However, I've continued to raise money for IMIO. We now have $10M in endowment and are in line for some other significant gifts.

***MA**: You have also worked in the real world. What do you see as the main differences between business men and academics, and can the two worlds learn from each other?*

DT: When we as academics think about an issue, we know the literature so when we hear a problem; we have many ideas on how to approach the problem. The problem with managers is that they don't read and they therefore have the benefit of leveraging business history. They typically don't even leverage their own corporate history very well. The challenge we face is to take some of the ideas from deep thinkers like Jim March, Ken Arrow, Sid Winter and William Baumol, and Herbert Simon, and package them so that managers can understand them better.

Managers tend to be generalists. The American ones don't read more than a couple of pages. European ones do read a bit more I think. One reason I like lawyers as professionals is that they read .. there's a lot of talent in that profession. It's sometimes easier for an academic to have a conversation with a great lawyer than a great manager.

I feel most at home in academia. In part because I'm not really a business man. When I go into the business world I don't go in because I'm executive chairman of LECG. My calling card is more often than not my academic work. I'm not a good glad hander. I like intellectual executives like Marion and Herb Sandler at World Savings. I admire creative ones like Steve Jobs. I recently had lunch with George Soros - - - a brilliant investor who reads and writes.

***MA**: How did you get interested in strategic management?*

DT: I think I got a call from Cynthia Montgomery welcoming me to the field! She had read my paper on the multiproduct firm. Until then I didn't think of my field as including management. I have a lot to thank her for. Then I got invited to a strategic management society conference in the mid 80's.

The first real strategy paper I wrote was on dynamic capabilities. I had written the initial paper and presented it at a number of workshops and it had really taken off. And the working paper was frequently cited long before it was published. At first I thought that since the working paper was out there and cited it didn't need to be published. Gary Pisano who is a co-author once told me that the paper is the most frequently cited working paper in the field of strategy. Eventually, seven years later, the journal [Strategic Management Journal] requested it because they wanted to publish it. Then I trimmed it down; and now everyone cites it as 1997. So when you see the literature, people have stopped citing the working paper from the early 1990's because now the published paper is there. But that whole dynamic capabilities business, there's no doubt that Gary and my work started that. No doubt. Amy [Shuen] chipped in too.

At some point I want you to talk to Gary Pisano because he was more aware of the fortunes of that paper; my career didn't depend on the paper so I let the working paper languish and didn't put effort into publishing it. I once assumed a much more perfect market for academic ideas. I used to think 'the smart people will know where these ideas come from, it doesn't matter if the paper is published or not". But in the field of strategy, there are very few careful scholars. Many seem in a big hurry. Authors sometimes cite without going back to the source and actually reading the original. Unfortunately some strategy scholars don't seem to exercise the same care and scholarship as you see in the social sciences.

***MA**: Another part of your work deals with knowledge management. How did that begin?*

DT: That is an interesting history. Jiro Nonaka came to Haas as a visitor and attended my class in the management of technology. It was great having him there. He always had a unique perspective. He took on the literature, mixed in his own experiences and philosophy, and produced the knowledge-creating firm book (with Takeuchi) which has been very influential with senior executives.

***MA**: Did you get the idea of tacit knowledge from Nelson and Winter; from Hayek or from Polyani?*

DT: Polyani. My paper in the annals of academy of political and social sciences [Teece, 1981] is the first time I talk about that. That's before Nelson and Winter 1982. So you asked me, how did I get into knowledge management? My doctoral dissertation was on technology transfer and of course that is a core concept in knowledge management.

***MA**: What do you see as your three most important papers?*

DT: Hmm .. The 1982 paper, the 1986 paper. Well, maybe also the 1978 Bell Journal paper (with Armour). That was the first time anyone showed a statistically significant relationship between organizational structure and economic performance. Then the paper with Monteverde (1982). Until those results came through transaction cost economics was having a hard time. There was no compelling evidence. This paper started the empirical tradition in transaction cost economics. Others followed. I'm not sure about the 1997 dynamic capabilities paper. It is getting a lot of citations[9]. The other papers are reasonable elegant in the sense that I wouldn't change them now. But with respect to the 1997 paper, I'd like to rewrite it or say things a little differently[10]. The core ideas would stay the same. But those other paper I wouldn't want to rewrite them. I'm quite happy with how they are and always will be whereas the 1997 paper I think about how I might say it differently.

***MA**: What do you see as the future of the idea of dynamic capability? Do you plan to work more in this area?*

DT: Well, there isn't really a future unless some more rigorous work is done in the area; but I hope there will be. Really, dynamic capability was intended in the beginning as just a set of ideas around flexibility, adaptability, integration, disintegration, complementary assets etc. And until we start laying out some testable propositions, get some organizational performance data together, and so on, it won't become a real paradigm. It is still 'pre-paradigmatic', to use Thomas Kuhn's terminology. Fortunately, there are quite a few people who are using the framework. I think there will be time for me to contribute again. I do plan to work more on it some day soon [see Teece, 2004]. I believe there are fundamental issues in strategic management which the paper can help illuminate.

***MA**: Thanks very much for your time.*

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[9] This paper received (in 2003) the Strategic Management Journal's Best Paper Award.

[10] Teece's Viipuri lecture titled "Explicating Dynamic Capabilities" (2004) is the author's effort to extend the paper.

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