

## *Dialogue*

### **Exploring the Minds of Visionary Scholars:**

### **Evolution in Strategy and Strategy in Evolution**

**Authors:** Ekin Ilseven<sup>1</sup>, Sukti Ghosh<sup>2</sup>, Leena Kinger-Hans<sup>3</sup>, Naja Pape<sup>4</sup>, Hoang-Minh Vo<sup>5</sup>, Amy Zhao-Ding<sup>6</sup>, Constance E. Helfat<sup>7</sup>, Daniel A. Levinthal<sup>8</sup>, Gabriel Szulanski<sup>9</sup>, David J. Teece<sup>10</sup>

**Abstract:** In an era where firms face rapid technological and societal changes, the field of strategic management must continually evolve to cater to contemporary challenges. Foundational scholars, Professors David Teece, Daniel Levinthal, and Constance Helfat, offer insights into this necessary evolution. Teece, influenced by his interactions with Silicon Valley, has enriched our understanding of dynamic capabilities and the intricacies of competitive advantage. Levinthal, a key figure in organizational learning, search, and adaptation, provides tools for comprehending the intricacies of organizational learning, spanning areas from technological shifts to intra-organizational dynamics. Helfat illuminates the firm's capacity to change, emphasizing dynamic capabilities and the essential role of managers as capability micro-foundations. A recurring theme across their works is the importance of intellectual cross-pollination and tackling fundamental, big-picture issues. Their collective insights emphasize the importance of adaptability, innovation, and a deep understanding of the evolving landscape, ensuring the continued relevance and impact of strategic management.

Forthcoming in *Strategic Management Review*

---

<sup>1</sup> [eilseven@ucp.pt](mailto:eilseven@ucp.pt), Universidade Católica Portuguesa, Católica Lisbon School of Business and Economics

<sup>2</sup> [sukti.ghosh@insead.edu](mailto:sukti.ghosh@insead.edu), INSEAD Europe Campus

<sup>3</sup> [leenakinger\\_hans@isb.edu](mailto:leenakinger_hans@isb.edu), Indian School of Business

<sup>4</sup> [naja.pape@insead.edu](mailto:naja.pape@insead.edu), INSEAD Europe Campus

<sup>5</sup> [minh.vo-phd@insead.edu](mailto:minh.vo-phd@insead.edu)

<sup>6</sup> [amy.zhaoding@tum.de](mailto:amy.zhaoding@tum.de), TUM School of Management, Technical University of Munich

<sup>7</sup> [constance.e.helfat@tuck.dartmouth.edu](mailto:constance.e.helfat@tuck.dartmouth.edu), Tuck School of Business at Dartmouth

<sup>8</sup> [dlev@wharton.upenn.edu](mailto:dlev@wharton.upenn.edu), Wharton School, University of Pennsylvania

<sup>9</sup> [gabriel.szulanski@insead.edu](mailto:gabriel.szulanski@insead.edu), INSEAD Singapore Campus

<sup>10</sup> [teece@haas.berkeley.edu](mailto:teece@haas.berkeley.edu), University of California, Berkeley

Now is the time for firms to evolve. The strategic landscape is rapidly transforming in a world where technological advancements compel firms to venture beyond their boundaries and the demand for societal betterment places increasing pressure on them. The boundaries of firms have grown more porous, and the unpredictability of their environments is challenging their viability. To help the firm's manager shape its evolution, the field of strategic management must also evolve itself. Indeed, the theme of the 2024 Academy of Management Annual Meeting asserts that "in today's world, the intricacies of effectively leading and managing an organization demand a fresh perspective."<sup>11</sup> Similarly, in July 2023, the Strategic Management Society introduced "The Big Six Questions of Strategy," aiming to inspire scholars and business leaders towards impactful actions. Our article advances this forward-thinking perspective as we reflect on our interviews with three foundational and visionary scholars, Prof. David J. Teece, Prof. Daniel A. Levinthal, and Prof. Constance E. Helfat, who have tremendously shaped the scholarly conversation over the past five decades.

Rooted in evolutionary economics, these scholars have developed their unique perspectives and offered methodologies and research trajectories that enrich our understanding of how firms can excel in a dynamic business environment. Starting in the 70s, Teece was quick to notice that in a context of accelerating globalisation, information flows and innovation, firms with technological prowess were not necessarily always winning. With this observation, he has dedicated much of his academic career to extract an understanding of how firms become and stay great in dynamic contexts, an understanding that took the form of dynamic capabilities. Levinthal's work provided a vocabulary and tools for scholars to conceptualize the prerequisites and consequences of organizational learning: the concept of absorptive capacity, the combination of the processes of selection and adaptation, and the distinction

---

<sup>11</sup> <https://aom.org/events/annual-meeting/future-annual-meetings/2024-innovating-for-the-future-policy-purpose-and-organizations>

between learning and adaptation, just to name a few. Helfat has delved into explaining how firms originate their core and complementary capabilities, how firms revise those capabilities to meet environmental change, and what role the manager plays in such processes. These questions led her to recently explore how a firm can shape its environment instead of merely adapting to it.

Beyond noting their important contributions to theory and practice, these interviews aim to unveil the individuals behind the ideas, revealing the profound emotions behind their convictions of what truly matters. These scholars are not just experts; they are visionaries with deep-seated ideas about the future of the field of strategic management. They share their passion, ambitions, and personal influences that have shaped their journeys. For instance, Teece emphasizes tackling “big hairy problems in business” with ambition and passion, Levinthal values the driving passion of a PhD student, and Helfat highlights the importance of an overarching question guiding a researcher throughout their career. These interviews reveal not only their academic excellence but also their human side, showcasing the passion that propels their research. Each scholar cherishes the cross-pollination of ideas, whether across fields, traditions, or communities.

We believe these interviews with Prof. Teece, Prof. Levinthal, and Prof. Helfat will inspire young scholars and the broader field to embark on innovative research journeys. These interviews, conducted over months, offer a deep dive into their work and the personal and professional influences shaping their trajectories. Join us on this enlightening journey into the minds and hearts of strategic management’s trailblazing thinkers<sup>12</sup>.

---

<sup>12</sup> We present the condensed version of each interview. A series of Foundational Scholar Interviews can be found online at the SMS Knowledge & Innovation IG channel: [https://www.youtube.com/playlist?list=PLzBQuOvAQQcgKo1OWnk1dTF3yy\\_kC7biD](https://www.youtube.com/playlist?list=PLzBQuOvAQQcgKo1OWnk1dTF3yy_kC7biD). Prof. Teece’s online interview can be found at <https://www.youtube.com/watch?v=JR2eJjYqXH0>.

## **Getting to Know the Person in Pursuit of Fundamental Questions in Strategy:**

### **A Conversation with Professor David J. Teece**

*By Naja Pape and Sukti Ghosh*

#### **Influences: The Carnegie School and early days of Silicon Valley**

***Q: Introducing your career's work and the journey undertaken as a scholar, what perspective would you like to give to younger researchers today?***

I think the biggest takeaway is that it's important to focus on fundamental issues. In my case, I'm trying to figure out a framework that explains why firms exist, why they're all different, and how in very competitive circumstances some firms achieve competitive advantage. Of course, the theme of innovation and change runs through all of my work. Alfred Chandler, the famous business historian, said that you can't have wealthy nations unless you've prosperous and productive firms. And if you can figure out how firms get to be great and stay great, you're part way towards understanding both shareholder wealth creation and the wealth of nations. Because I'm an economist, I've got those bigger questions in the back of my mind all the time. In a sense through my work, I'm asking the same question as Adam Smith did 250 years ago. Posing the right questions is more important than exactly how well you execute on the answer. A good answer to a great question is more praiseworthy than an overly precise answer to an uninteresting question.

***Q: What have been some of the major influences on your work over the years?***

Edward Mansfield (my advisor) and Oliver Williamson (and Almarin Phillips, Herbert Levine and Irving Kravis) were all University of Pennsylvania faculty and each was influential on my career. Mansfield drew me to the topic of innovation. I knew how important it was, but back then the economics of technical change was not "cool". It was all about macro modeling and welfare theorems. Mansfield was the first scholar to study industrial research in a serious way. He was also disaffected with advanced economic theory and made no bones about it. He was a

strong statistician too. He knew that small data sets that directly measured the phenomenon of interest could be a better platform for insights than large databases collected for other reasons. Oliver was likewise disrespectful of mainstream theory and was breaking out of the standard neoclassical paradigm, but he was much more subtle about it. There was a buzz among graduate students about “Markets and Hierarchies” (Williamson, 1975). Most people thought that his writing was incomprehensible, but I found it very readable. My early career was about explaining and testing what Williamson was trying to say because he loved to use archaic vocabulary (e.g. Teece, 1980, 1982, 1986b).

Both Mansfield and Williamson had spent time at Carnegie Mellon where Lee Bach was the Dean (Richard Cyert, James March, Herbert Simon, Richard Nelson, and Sidney Winter were all there too.) They were all true social scientists and very interdisciplinary. At Penn, they invited their friends from the Carnegie School to do workshops. I offered myself to go to the airport or train station and pick the visitors up and bring them to campus. As a diligent graduate student, I had the fortune to chat about ideas with these absolutely brilliant people. It’s important who you have access to; it’s the very fact that you get to meet them that makes you excited by their work. Ninety percent of what I’ve learnt from these people was not from reading their work but flipping through the pages they’d written became more meaningful than any other dry and dusty book because I had met them. That close, personal contact was part of what shaped what I was able to subsequently do and also gave me the courage to do it. That’s the great benefit I had: I’m a child of the Carnegie School of the 50’s and 60’s. Nelson and Winter came to be great mentors and friends. Lucky me!

The students in the classroom at Stanford and then Berkeley were great as well. At Stanford, I was teaching applied microeconomic theory and I was enthusiastic about it. But I came to realize that the apparatus of microeconomic theory didn’t provide insights of practical use to managers. The students were 5-10 years older than me and they were more worldly.

They were excited by Silicon Valley. They posed good questions about real businesses. I had good answers, but the answers weren't informed by textbook analysis. I came to the view that the weakness was the textbooks, not the students' questions; and there was a lot of work that needed to be done to create new theories of the firm and new frameworks about business and management. That led to my early work on economies of scope, the scope of the business enterprise and diversification (Teece, 1980; 1982).

***Q: How did being proximate to Silicon Valley influence your research on dynamic environments and uncertainty and your subsequent interest in technology and innovation?***

Living proximate to Silicon Valley has sure made a difference although Silicon Valley was still relatively young when I was shaping my research agenda in the 80s. The San Francisco Bay Area is full of creative people. I had the good fortune to meet and know some of the pioneers of Silicon Valley like Gordon Moore and Bob Noyce from Intel. I met Steve Jobs. I knew the folks at IBM labs and Xerox Parc (e.g. Will Spencer, Vice President of Xerox Corp) is a good friend. Eugene Kleiner (pioneering venture capitalist) was around. Wozniak (co-founder of Apple) would come to campus to talk. David Packard and Bill Hewlett (founders of Hewlett Packard) were acquaintances. Once you understand these founders, you've a natural way to think about the firms they founded.

***Q: You've mentioned that you consider yourself an amateur business historian. How does it factor into your research?***

I had the good fortune of knowing Alfred Chandler. I think there's always a benefit from a historical perspective in two ways: First, there are lessons to be learned from the past (I still assign a chapter of Alfred Sloan's book "My Years at General Motors" to my students). Managers today keep making the same stupid management mistakes and I always say to myself: "If you'd only read Alfred Chandler, you wouldn't do this." Second, these narratives

also give you credibility when you talk to executives. They'll listen more eagerly if you mention a real-world case study rather than some theoretical proposition.

**Scholarly trajectory: Genesis of the theory of the innovating firm**

***Q: Could you tell us more on what influenced you to take a stance against mainstream economics and how your work addresses the failures of neoclassical economics?***

I remember as a graduate student in New Zealand we were trained to think critically. I came to understand that good research wasn't just about rigor. Relevance mattered too. Our job as scholars is to help solve complex problems, not just impress each other with elegant theories that don't have empirical support in the real world. Personally, at Stanford, I knew that I was taking on great personal and professional risk as an economist and junior faculty member to step outside the box. The more cynical answer might be that I didn't have a chance to play inside the box, so it was "play outside or don't play at all."

My early work was on diversification, the scope of the enterprise, profiting from innovation and then eventually dynamic capabilities. All of these were efforts to try to come up with a better foundation of the theory of the firm. In the field of strategic management, people are looking at it from a managerial lens but I looked at it from three different lenses: (1) the managerial implications, (2) public policy implications and (3) the implications for theory itself. Dynamic capabilities is really an effort at one level to create a theory of the innovating firm – a theory of the firm that is constantly improving, maintaining and strengthening its competitive advantage.

***Q: You are a proponent of managerial agency as opposed to just luck and the role of individuals in creating dynamic capabilities. Can you elaborate?***

Luck plays a role, but as is often said, "Luck favors the prepared." Being in the right place at the right time is critical. But there are usually quite a few people standing in the right place at the right time. It's not only that a few are prepared to act; it's also that only a few are able to

sense the opportunity and know what to do. Dynamic capabilities are needed both at the individual and organizational levels to make things happen. Of course, it's not just talent. It's also networks, which can provide access to resources if you're a credible player. Managerial agency matters a lot. The top management team and the board of directors hold the destiny of the most organizations in their hands. I've gone into this a little bit in "Dynamic Capabilities: Routines vs. Entrepreneurial Action" (Teece, 2012). Mainstream economics has no place for the entrepreneur, and very little room for the manager. That extreme oversimplification has impaired the field a good deal but does leave room for strategic management to slip in and fill the gap! I have my own life experiences as an entrepreneur, manager and board member.

***Q: Could you describe the ideation process behind your 1997 article on dynamic capabilities (Teece, Pisano & Shuen, 1997)?***

It got going in the late 80's and early 90's. I was trying to understand the foundations of competitive advantage. Porter's "5 Forces" (Porter, 1980) said nothing about this. He wasn't trying to explain the outcomes and the performance of firms; yet the "5 Forces" framework was taking the world by storm. Of course, in the 1980's, globalization was increasing rapidly, and the speed of information flows and innovation was accelerating, which is the reason I started focusing on capturing value from innovation. But the bigger question is how do you create value? How do you figure out the next big thing and place the right bets? That was a mystery from a theoretical perspective.

Teaching the PhD students at Berkeley was really where the ideation process got concrete. Gary (Pisano) and Amy (Shuen) were both doctoral students. We were very familiar with the work of Nelson and Winter (1982), the innovation literature and the work of Williamson (1975). We somehow started combining this together into this dynamic capabilities paper. Complicated problems require a multidisciplinary perspective, and that's what the paper was all about. The big insights were that firms with a lot of technological prowess were not



always winning, that history mattered, that firms couldn't just do anything... they needed capabilities. I appealed to innovation processes to help explain how competitive advantage was eroded. Competitive advantage flowed from the firm's capabilities, not just market positioning.

***Q: Why did it take so long (six-seven years from working paper to publication in SMJ) for this particular paper?***

I submitted the paper, and then I forgot about it. I didn't necessarily think the paper would be widely cited, as to me it seemed fairly obvious what we were saying. I was busy building a company, The Law & Economics Consulting Group, and then one day Dan Schendel (then editor of Strategic Management Journal) called and said "David, you only have to do minor revisions, please get it published". Besides the fact that I was distracted by building companies and consulting, deep down, I thought the paper was highly incomplete and unsatisfactory. I feel the same way today. I wasn't sure I wanted the paper published. The 2007 paper "Explicating dynamic capabilities" is an effort to tidy it up a bit. Dynamic capabilities is hard because you have a multidisciplinary exercise going on where you're trying to pull together threads from decision theory, economics, organizations, behavioural theory, marketing, and business strategy. There is hardly a subfield of business and economics that is not in some way implicated and finding the right way of doing that in 30-35 pages is quite hard.

What I subsequently came to realize is not only that it was an ambitious paper, but that it was in fact attempting to create a workable systems theory. That's very hard to do. I overcame my reluctance to publish the paper because I thought Gary and Amy might benefit from it career-wise. They were important contributors to the enterprise. With hindsight, it's an important paper. It attracted a lot of attention. However, the literature it has spawned is messy because, as with any systems theory, different people are attracted to different aspects, whereas what matters the most is the holistic perspective.

***Q: Your work on dynamic capabilities has been controversial. How would you like to see the framework evolve?***

I keep thinking about how to improve the paper and relate it better to the work of others. In that regard, I'm planning to write a book entitled "Dynamic Capabilities and Related Paradigms: Implications for Management and Policy" to try and bring some order to the literature.

In terms of how I'd like the framework to evolve, I think it's important for young researchers to first of all equip themselves so as to be able to truly understand the framework. To do so, you need a good understanding of several literatures including evolutionary economics, resource-based theory, technology management, organisational change, cognitive psychology, decision theory, behavioural economics, and of course, strategy. I'd like to see the framework evolve by building on what has been built, and not trying to take it off course... Let's mine executive quotes to illustrate the key points. Let's support it with more empirical studies, with in-depth studies of the elements, and with better ways of integrating the elements and with more case studies. Maybe we can create a "Dynamic Capabilities' Society", agree on certain standards and manage the research ecosystem.

**Contributions: The field of strategy found me**

***Q: What do you consider your greatest academic achievement?***

It's getting recognized as the leading scholar in business and management in 2020, based on a peer-reviewed methodology applied across multiple disciplines. I also take pride in the honorary doctorates that I've received. I like giving graduation addresses. It's hard to impart wisdom in 15 minutes!

In terms of my best work, there is a sort of an elegance-relevance trade-off and, to many academics, the best work is the most elegant work. For me, it's either "Profiting from Innovation" (Teece, 1986); or the twin papers "Economics of Scope and The Scope of the Enterprise" (Teece, 1980) and "Towards an Economic Theory of the Multiproduct Firm"

(Teece, 1982). These papers are narrower than some others, but quite elegant. By that I mean there is a clear straight forward logic and the development of testable propositions. For an empirical posture, the JIE paper “Internal Organization and Economic Performance” (Teece, 1981) is quite simple and elegant and uses nonparametric statistics to test a simple hypothesis. Those four papers aren’t only a bit more elegant than most; they also provide considerable insight into big questions and give you some tools for thinking about how or why pioneers often lose and what they need to do to win commercially in the marketplace.

Central to my research was putting innovation center-stage. In the field of strategic management, I hope to have brought the innovation piece and competition policy into the literature, an area I actively work in and have been pushing hard for 30 years with limited success. Public policy should be animated not by static competition, which is what you get from standard microeconomics, but by innovation-based competition which is the Schumpeterian perspective. Suddenly, with a focus on big tech, there is at least now an audience. Whether it affects policy is another story. I’m proud of the fact that I made some of these streams of research more interdisciplinary than they otherwise might have been.

Dynamic capabilities, obviously, is a different type of contribution that doesn’t come from its elegance but from its ambition to answer the most fundamental question as I see it in the field of strategic management: *What explains how you build and maintain durable competitive advantage over time?* Porter’s “5 Forces” (Porter, 1980) doesn’t even attempt to address that question. Resource-based theory of the firm (Barney, 1986) is a static version of this, but to me, dynamic capabilities takes that interest and makes it dynamic. We’re interested in renewing, expanding and scaling success. If I can make it more elegant, dynamic capabilities will probably be my best work. It’s a very central set of ideas not just for understanding how to build and maintain the advantage of the firm level but as an individual level analogue for that as well: how do we as individuals have dynamic capabilities and how does it play out at

the nation/state level? When you think about it, it's really about the learning process and so its strength and weakness is that it's a universal tool. Assessing the situation, sizing it out, making a decision and then there's always going to be a reaction to the decision, responding to that and transforming from the decision to the next stage. It ties in with lean start-ups, evolutionary economics (Winter & Nelson, 1982), leadership, ambidexterity (Tushman & O'Reilly, 1996) and knowledge-based firm (Kogut & Zander, 1992). If we can make these connections a bit more explicit, I think we'll have a better dialogue with not just academics across disciplines but better decision making in industry and in governments.

***Q: In your roles as a businessman as well as a scholar, how was your work shaped by opportunities to cross-fertilise ideas between practice and academia?***

I spent three quarters of my life actually building companies. This has worked for me because I see the connections between the two; my decisions in building businesses that I have managed in a way have been very much informed by the literature. The greatest privilege in life is to be a professor; to have society support you and let you think, write, teach, create and experiment. A great university is a precious institution, certainly more precious than most businesses. But business is not mainly about making money, it's about learning. My practical activities have informed my research. It affords me confidence around decision-making if you've seen a hundred case studies on something and read articles on it and have contributed to that literature yourself. When the decision comes up in the neighborhood of that, you already have a compass. It's not to say that the literature is always right, but you can frame the problem and you know what the conventional wisdom is. I've been able to leverage the literature to help me make managerial decisions and formulate strategy in running my current companies. But it's also going the other way - I'm dealing with client problems all the time and you're meeting with people who look at the documents and data from companies, so it becomes a wonderful filter through which to reflect upon. So, I think my writing has been assisted by the experience I've

had and the phenomenon I've had the privilege to look at. There are economies of scope or synergies between the two.

***Q: What would you like to be remembered for, by the academic as well as the practitioner community?***

It would be nice if those two communities fused more seamlessly to begin with. By that I mean I wish for the appetite in business for academic ideas because a lot of learning is missed, since businesses don't take in very fast the learning from the academic world and vice versa. Keeping innovation at the forefront, helps to understand that it isn't just a driver of individual firm prosperity and longevity but also of economic growth. Whichever nations control the technology of the future – 5G, robotics, biotech – political implications of this are of absolutely massive proportions.

I never considered myself a strategic management scholar until mid-career. The field of strategic management found me, not the other way around. Dan Schendel, in the early days of Strategic Management Society, invited me to the SMS conference in Singapore; thereafter I started to take the field seriously and I'm glad I did. I got to meet Dick Rumelt and loved his perspective and trenchant insights. Much to my surprise, and delight, the field has taken me seriously too. I've said many times that the field owns one of the greatest questions in economics; how do firms create and capture value, and sustain that condition over the long run? Business firms bring benefits to society by doing so, particularly if they are innovative, because there are spillovers. The social returns from innovation are often orders of magnitude greater than the private returns.

I'd like to be remembered as someone who helped craft a (dynamic) capabilities theory of the business enterprise that helped managers do their jobs better, while also helping policy makers understand the importance of well-run business enterprises to the wealth of nations, the survival of democracy and the continuation of a liberal economic order globally. The Panmure

declaration which I helped craft at the New Enlightenment Conference in Edinburgh, 2019 summarizes my aspiration for the world<sup>13</sup>. I like to think that we could keep Adam Smith relevant. He wrote two books “Wealth of Nations” (Smith, 1937) and particularly “Theory of Moral Sentiments” (Smith, 1822) which are useful guides for today. We haven’t talked much about it in the field of strategic management, but I think that the business enterprise and actuality is focused because of so-called shareholder pressure and short-term results. Innovation requires a long-term focus. Strategic management needs to engage with questions in governance. I think the challenge for the field is to put some leadership out there and have some impact on public policy as well as on managerial decisions. To the extent that my work can contribute to that, I’ll be happy; I hope people see these cross-connections and provide a research pathway to do so. It’s time for us to show that we’re real scholars and can embed our research in these broader sets of global issues that are now confronting us.

#### **Strategic management: Dynamics that shaped the field and the future of the discipline**

***Q: How is the profiting from innovation and the dynamic capabilities framework relevant for understanding ecosystems and digital platforms?***

The dynamic capabilities framework is relevant in several ways. First of all, it brings us back to the role of managerial agency, so much of the literature on platforms proposes that there is no role for managerial agency but my sense when I look at big tech is that if you don’t have strong dynamic capabilities, getting ahead doesn’t mean staying ahead at all. The sort of notion of tipping points that are mathematically determined is pure nonsense. I think any good scholar in strategic management should question the tipping points as being inexorably set by scale

---

<sup>13</sup> It “....urges international leaders to base their policies and decision-making on a set of common principles, as espoused and formulated by Adam Smith, which cherish the required values of an ethically-based liberal democratic system, a moral commitment to the well-being of our communities and affirm responsibility to protect economic, political and social freedoms, use resources wisely, avoid unintentional consequences, follow the rule of law, favour markets and prices as guides to resource allocation and take a long term view of private and public investments, to support inclusive economic growth and prosperity for all.” (Panmure declaration, New Enlightenment Conference in Edinburgh, 2019)

and network effects. Certainly, they've a role. But every good platform has to face competition from another good platform and Andy Grove at Intel wrote this book "Only the Paranoid Survive" (Grove, 1997). Only if you have very strong dynamic capabilities and constantly sense, seize and transform it sensibly, will you have a chance to survive.

From the outside, policy makers looking in, think there's a natural advantage that is inexorable. It certainly isn't the case. Maybe you get a second chance if you have a strong platform and a big user base but managerial agency... the capturing value, "Profiting from Innovation" framework, I think it's relevant and it ties together into business models. Choices to which side of the market... and in some cases you give away something free in one place and charge for it elsewhere which turns a lot of traditional economics on its head. In ongoing public policy debate, there is an absence of professional competition policy. Policy makers typically put very little weight on managerial agency and are probably going to craft policy which is not ideal.

***Q: How do you think your work has influenced the evolution of the field of strategy and is likely to, going forward?***

The field of strategy goes to the beat of its own drum; that said, in the 1990's some economists put out the view that strategy and game theory would become one and the same. I thought that was unlikely, so long as one continued to define the strategy field by big questions such as "what are the foundations of competitive advantage?" The 1990 Napa Valley strategy conference was organised to give the game theorists a chance to show their relevance to the strategy field, but they couldn't provide compelling insights into foundational questions. There is certainly a place for game theory. Indeed, strategic thinking is core to the field, and will always remain so. But the best strategists are not the best game theorists and vice versa. Also, we have come to understand that most game theory models are non-robust... change one assumption and the results collapse. That's a severe limitation. It would have been nice had

game theory been able to deliver on its promise; it has in certain areas such as auction design but in other areas it has not contributed at all.

I'd like to think I've been influential through my work, and that of my students and co-authors, by making innovation central to the field of strategy. Strategic management is by necessity an eclectic field and the study of innovation requires one to have a wide aperture lens... so there's a natural fit. I also like to think that I've contributed in helping others understand the role of the business enterprise and management in advancing both value creation and value capture through innovation. As we go forward, we have to figure out how to better integrate strategic management and public policy. To be successful, the business enterprise must fit comfortably into society. In Europe, the Americas, and in the rest of Asia state regulation of private enterprise will need to change. Industrial policy, technology policy, and competition policy can no longer be siloed. Management has to become much better attuned to the new world we live in, and scholarship in strategic management can aid that process.

### **Two Faces of Scholarship: A Conversation with Professor Daniel A. Levinthal**

*By Amy Zhao-Ding and Leena Kinger Hans*

#### **Early Influences**

***Q: As a scholar, you have been working on different streams of research: learning and search, adaptation and selection, technological change, etc. Would you say there were some underlying threads that were sort of common across these works?***

I guess if you want to go way, way back to the origin story, I am a child of Silicon Valley. My dad was a faculty at Stanford and I grew up in the 60s and 70s during the technological transition in the valley. In some sense my dad was involved in it and the concept of Schumpeterian dynamics was imprinted; thinking about adaptation and technological change was kind of a natural calling from that starting point.



***Q: Speaking of path dependence, you actually started out as an economist—you got an undergraduate degree in Economics at Harvard in 1979 and immediately went on to do a PhD at Stanford Graduate School of Business. How did you make the switch?***

You know, [as a] slightly informed naïve young person interested in social sciences, I liked both formal modeling and social science questions—the answer to those two propositions seemed to be economics. And then in my junior year as an undergraduate, I wandered into a sociology class taught by a freshly minted new assistant professor named John Padgett, a course in organizational sociology with an amazing reading list. He had just come from Michigan, we were reading working papers by Nelson and Winter, Axelrod and Cohen, as well as the Carnegie iconic works (Cyert & March, 1963; March & Simon, 1958). As an economics student, I thought wow this sounds interesting. I almost went to Yale as PhD student in economics to study with Nelson and Winter, inspired what I had read in Padgett's class and my starting point in economics. And then I had this basically foolish idea of going to a business school, thinking it might be easier to integrate those different strands of my interest there. At the time I started at Stanford Business School in the economics program there, agency theory was really taking hold the econ program. I then moonlight with Jim March and began to do some work with him. So as a legacy effect—I have a PhD in economics. The broader interest in thinking formally about interesting social science questions has endured, even as I have wandered fairly far from economics as a paradigm.

### **Absorptive Capacity**

***Q: But clearly, starting out as an outsider has its perks—did the idea of absorptive capacity come from reading an economics article?***

Indeed. You two [referring to the interviewers] did an amazing job! They did a great job of sense-making of my work, and as I teased them, it was kind of like an academic therapy session: “Oh now, we understand where these ideas come from” and actually prove that—“look, Dan,

you are once again the outsider; you kind of liked being the outsider.” To show my incredible career foolishness and build a little on your question, in addition to this odd PhD training, I accepted a job at Carnegie (immediately after graduation). In itself, that was not a foolish decision, it’s a wonderful place. The foolish part was I accepted a position that was joint with economics and organizational behavior. So, in the fall I taught microeconomics and in the spring I taught organizational behavior. In my third year they needed a strategy person—an opportunity that I took as it both made my teaching load simpler and suggested a “category” in which my diverse interests could lie.

But back to your specific question, I had this background (in economics) and slightly paying attention to my students as I was proctoring an exam, I read a back issue of *Econometrica*. There is this lovely article by Michael Spence on spillovers (Spence, 1984). It’s a nice model and he ends the model with this open question: “Given the effect of spillover, why are we seeing in the semiconductor industry all this R&D happening?” I was thinking, well, “I know!” From my perspective as someone interested in learning, it struck me that if you came to me and told me some deep dark secret about Intel’s processes, it will still be a deep dark secret, I would have no idea what you were talking about. So, the issue of absorptive capacity seemed so self-evident to me by being conversant or being able to engage in their conversation, but not being a part of it. It was pretty clear to me just putting down the article, that if I introduced—what Wes and I would term absorptive capacity—another consideration, I could qualitatively flip the results Spence was talking about. I went out for lunch with my friend Wes, and Wes got excited; and that particular journey went forth.

***Q: The article on “Innovation and learning: The two faces of R&D”, originally published in 1989, was republished in 2015 by the Economic Journal to celebrate its 125<sup>th</sup> year anniversary. What do you think is the future of absorptive capacity?***

I have to say that that reprinting was a truly lovely occurrences. To be listed on the same Table of Contents with John Maynard Keynes? That is a humbling (and daunting) experience. Clearly, it has been very gratifying to see the influence that it has had. But looking forward as to its future influence, I see a few opportunities. There is a lot of interest and excitement about open innovation in the world and in our literature for quite appropriate and sensible reasons. But it strikes me that in our enthusiasm around these issues, that we may be re-committing perhaps the sin of neoclassical economists when they were thinking about innovation that Wes and I were critiquing. Can a potential buyer of the IP contextualize it? How do I articulate its potential value as a seller, and so on? Clearly there are things like Netflix running their prize competition for a well-defined engineering problem, asking a “crowd” of potential inventors if they can come up with an algorithm that does better predicting and matching than Netflix’s current one. But I think maybe there is a new opportunity (for absorptive capacity) to express itself in considering how firms realize the latent potential of open innovation. In a similar vein, at the time when Wes and I did our work, there was a lot of excitement and enthusiasm around joint ventures and alliances. The implication from our work was that often managers should not frame these initiatives as a complete substitute for their internal capabilities. Clearly, the absorptive capacity argumentation suggests complementarity. So, in this interesting new brave world of open innovation and so on, what’s the viscosity of knowledge, how does it move, how do people understand and represent these opportunities and possibilities, are still very much important. In this same spirit, there is a large and growing literature on how network structures influence the flows of knowledge. However, much of this work treats knowledge as if it is akin to an infectious disease—the only issue is one of exposure, and not the actor, or node’s, susceptibility (i.e., its absorptive capacity).

**Learning, adaptation, selection, industry evolution**

***Q: You have been quite specific about the distinction between learning and adaptation. Yours and Jim's work on myopia of learning showed the traps of learning and how it may not lead to successful adaptation (Levinthal & March, 1993). Have you been interested in adaptation and interaction with environment all along?***

At the time I was a doctoral student, the population ecology research program was starting to really take off. So, I was very much aware of this emerging line of work, at the same time I was focused on issues of organizational learning with Jim. So, I was sensitive to the interplay of these processes early on. Also, Michael Cohen happened to spend some time at Stanford on a sabbatical when I was there and, among other "gifts" gave me a winter break reading "assignment" of John Holland's *Adaptation and Natural Artificial System*. It wasn't the easiest way to spend the December break, but the book quite squarely put these ideas of multi-levels of adaptation central in my thinking. Further, core to Holland's (1975)'s argument was the notion of the exploration and exploitation tradeoff (something Jim acknowledges in his opening discussion in his 1991 piece). So the joint consideration of learning, adaptation, and selection were important to my thinking. In particular, as you suggest in your question, learning can be a change in these response patterns, but we should not view it as necessarily a normative inevitability that learning is putting us onto a better path. So to distinguish between learning, a change in behavior or belief, and adaptation, an enhanced fitness to one's environment.

***Q: Would you then say that selection and adaptation are two sides of the same coin? Also, what do you think are the micro-foundations of industry evolution? Do you think your thoughts about selection and adaptation offer a different lens on industry evolution?***

Taking the first part of that, clearly there are two kinds of interplay. One is, selection at one level may suggest adaptation at a higher level. That's what population ecology will tell you. Level of analysis matters. Furthermore, if you are going to have a selection process, underneath

that, you need some fairly stable basis of heterogeneity (Levinthal, 1991). And the learning, the path dependence associated with that is really the fodder over which selection might occur. With regard to the broad question about (industry evolution), I was also imprinted early on by the work of Nelson and Winter on evolutionary economics which is very much about taking adaptive system sensibility to the issues of industry dynamics—which seems to me a very generative, promising approach. We don't reach equilibrium in the blink of an eye, indeed the context keeps on changing, so focus on dynamics and embrace of disequilibrium offered by evolutionary economics seems like a useful structure. More generally, we have the issue of what is the underlying micro-unit of analysis and what's the base of the path dependence. I think there are still a lot of opportunities for theorizing and for empirical work on that front, I don't think we really have resolved that to a particularly satisfying degree.

#### **Formal modeling and NK model in organizational studies**

***Q: In your 1997 piece, “Adaptation on Rugged Landscapes”, you used the Kauffman NK model from the work in complex adaptive system and applied it to the study of organizations.***

***How did that happen?***

The story both reflects some of my ongoing research instances and fortunate happenstance. In terms of the happenstance, at some point in the mid 90s Michael Cohen organized a really wonderful workshop with the Santa Fe Institute. It was an interesting mix of the Santa Fe folks, the physicists, chemists and so on, and a few organizational theorists. The clever design Michael used was ‘mix and match’: the Santa Fe people would present an organizational paper and vice versa. So a physicist presented Jim's exploration and exploitation paper (March, 1991). His take was that the model represented an interesting variant on a spin-glass model, and that there might be something to it. Jim politely sat and listened to that characterization. I was a still untenured faculty and Kauffman this MacArthur award winner major figure. Michael Cohen as organizer told me, regarding Kaufman's work on NK models: “Dan, here's a pile of

papers why don't you talk about it". I took a little time processing the work and thought, "Wow, this is an interesting model structure! Organizations are fundamentally about interdependent actions, and here's a nice framework; the Nelson and Winter idea of routines would be a natural implication of some dynamics here." So, that was my initial exposure to Kauffman's work. However, Kauffman had been thinking about how proteins adapt, not people or organizations. In addition, he didn't incorporate a role for selection. So, I saw the NK structure as a really great basic engine, and (on top of it) I could introduce selection. In some sense the 1997 paper allowed me to, among other things, consider in a much more rigorous and clear way what I was talking about in my 1991 piece (Levinthal, 1991). In the model, we see the path dependence and stable heterogeneity over which the forces of selection are going to operate. We've been talking in the organization's literature about the idea of local and distant search, and here was a way for me to put some additional clarity on that distinction. So, a lovely happenstance and as we joked, another illustration of fortune favoring the prepared mind.

***Q: What do you think was the key message of that paper in the field?***

I had my use of it, as I just noted, but I think why it took on greater energy in the field is that for a modeling platform you are looking for a canonical representation of important phenomenon. For instance, consider the prisoner's dilemma. There are likely hundreds of papers, some in psychology, some in economics—for which the prisoner's dilemma is used as a canonical representation to think about some issues around conflict. The multi-armed bandit is a canonical representation to think about exploration and exploitation. Not that you cannot think about other ways to represent that tension, but the bandit model offers streamlined, simple way to examine that tradeoff. I think people saw the NK as this simple abstract vehicle to talk about organizations as interdependent systems. After I had done some work using the NK framework, it occurred to me, that the NK as set out by Kauffman is an interesting degenerate case of an interdependent system. If you think about Simon's *The Science of the*

*Artificial*, systems are likely to have some inherent structures. Central to Simon's argument is the idea of near-decomposability—interdependencies are not randomly distributed across a system. In the subsequent work I did with Sendil Ethiraj on modularity, and related work by others, we began to tweak the NK platform in some way to speak to these issues. Having that possibility as a basic platform made it able to speak to a number of important organizational questions, quite apart from the specific contributions of the 1997 piece.

***Q: So the NK model was one of the examples where you use formal modeling and simulation. What do you think is the role of formal modeling in organizational studies and what are the benefits and challenges of it?***

I think formal modeling can be a useful way to engage in theorizing. We have some primitives, we have some non-trivial interactions, let's understand better these phenomena and processes. I think with formal modeling though, in that same spirit, it is incumbent on the modeler to make the connection to the underlying phenomenon. Models that just speak to other modelers, are not going to be so well cited and shouldn't be so well cited. My presumption is that readers of my work in most cases won't go beyond section 2. So yes, I have done this formal model, I've used that to help sharpen my insight and be able to generate some insights about the world, now it's incumbent upon me to then express these insights in a manner that is evocative for a broader audience. I think that's part of the modeler's responsibility. That is in contrast to thinking "oh these non-modelers didn't read my work carefully". No. You should be speaking to their phenomenon and making those potential linkages.

***Q: How much has intuition played in theorizing and bringing together these disparate areas you put together?***

Quite a bit! I am not going to start a model or theory building process without having the intuition first. In my head there's something interesting that might work. You may then ask me why would you bother to do a model and not just lay out the verbal theory? Two elements to

that: one is that it may be easier in the journal processes, but more importantly, I tend to be partly right, partly incomplete, and maybe a little bit wrong. Back to your question, research is not a blind discovery process. It is not, let me make these three assumptions and put these into a soup can and (see) what would happen. No, that seems to me not the case. The ones who are close to that, wind up working hard and doing mediocre stuff. It's not that my intuition is correct, but it's a guide to action—a lot like the managerial vision in the strategy process. Partly good but incomplete intuition is a suggestive guide to action, the initial trigger for a research project.

### **Working with Ph.D. students**

***Q: At Wharton, you worked a lot with PhD students. What do you look for when you are asked to be an advisor? Do you generally work with students who aspire to become theorist/modeler like you?***

What do I look for... so Jim would probably say “they find me, I don’t find them”. I am in my office doing whatever I do with my distracted life, and they knock on the door. Oftentimes in that knocking on the door is a pitch of the sort “Oh Dan, let me pitch how I am interested in what you are doing”. After about two sentences of that I generally stop them and say, ok, tell me what *you* are interested in. You made this irrational choice to become a PhD student. You have some underlying passion that made you go that way. Let me understand and hear a little about that. Yes, we need to find some common ground, otherwise there’s not too much point of having a conversation. But I generally put a brake on the isomorphic pleasing inclination to (ask them to) tell me about their deep interest and (see if they can) articulate that. If in the fourth or fifth sentence it’s about how they are going to be able to generate a paper about this, that’s a turn-off—I feel this is a dating kind of question. So no, I want you to talk about what is animating you about ideas and how we are going to explore that.



Specifically, Sendil Ethiraj is a lovely example of walking into my office. He was energized about modularity, a hot topic at that time. I expressed my puzzlement about the question of how actors identify what constitutes the modular structure and why do we presume that they have gotten it right, or (essentially) where is the design element. Our initial conversations around that question lead to our paper in *Management Science* and then some years later to our piece in ASQ where we consider the question of the search for and potential discovery of an underlying modular structure when that structure is not apparent to actors *a priori*.

Giovanni Gavetti came to Wharton having been pre-socialized and trained by Franco Malerba at Bocconi. He's already pre-exposed to an evolutionary economics perspective and Franco gave Sid (Winter) and I a heads-up that this wonderful guy is coming. He (Giovanni) marched into my office early on saying "evolutionary theory lacks cognition and that should happen". I am like "OK, you know, that's actually a pretty good point!" Giovanni has that wonderful passion and engaging around the issue of cognition was something I was quite happy to join him in pursuing—yes that was an enormous void and maybe we can begin to make some progress. Those conversations become our ASQ paper (Gavetti & Levinthal, 2000). With his great fieldwork about Polaroid with Mary Tripsas (Tripsas & Gavetti, 2000), he examined those issues empirically as well. So, it's a nice example of a strong distinctive perspective that I got seduced by and luckily was able to co-work with talented doctoral students.

#### **Future direction of the field**

***Q: You've been a true descendent of the behavioral school of strategy. In the world today, 20 years into 21st century, where organizations and environment change faster, when open information and innovation is widely spread, do you think the assumptions of the behavioral school need to adapt? What are the new questions we as scholars could ask?***

In some sense all this flux and change stuff is wonderful news for the behavioral school. These issues of learning and adaptation are always there, but the context is giving them extra energy. Partly also we are in this world of big data and how does that change things. It strikes me as interesting if you look at how data analytics is currently being applied, it is at a fairly operational level and being used by the Amazons and Googles of the world doing machine learning. These organizations are being behavioral. They are not solving for some optimum. Rather, they are getting rich data, quick feedback and being able to run experiments and figure out what ads to throw at you and so on. I think it's an interesting open question whether these approaches that have been successful at the tactical level might scale to more strategic questions. It is an interesting open question, but again very consonant with the behavioral approach. Actually, if you think about the early work going on at Carnegie, Simon left the study of organizations to study cognitive psychology and AI. The interplay has always been there. But, I think at the same time, it is important for organizational scholars to understand organizations as multi-level systems, not a unitary actors bootstrapping through big data. So an expert system is a different issue than the representation of organizations. In sum. it is a good time for the behavioral tradition, and there are many opportunities!

***Q: You once mentioned that becoming a strategy professor is a decision to resolve the economics vs. OB identity issue. You yourself have brought different worlds together. Right now what would you reckon are the current debates and what do you think are important to be resolved to take the field forward?***

I am fairly eclectic and I guess a pragmatist. I am not a big ideologue. Maybe with my continued outsider sensibility, I don't resonate with that part of the question. If I see an issue, it's less about contestation of theoretical premises. Frankly, what I am more troubled or concerned about is a kind of sensibility, well, essentially the publication fetish. All the energy around building the vita, all our tutorials about how to manage that process and so on. So in this sense

I get impassionate ideologically, but my concern is more about the process and the values and goals of researcher.

For me, back to the irrational commitment to the journey of the Marchian sensibility—you do good work by asking interesting questions and try to gain traction on them. The article is a byproduct of that effort. If the article is the goal in view, I don't think the odds of doing something interesting are all that high. You may actually get an article. But bear in mind that the citation to the modal article in our field is rather modest. So my concerns are less about the game theorists banging at the gates of my fortress, than more of the general “what is the nature of the intellectual engagement in the community”, (such as) the near term selection pressure and feedback of our system. My concerns are more of that sort than the specific contestation about intellectual tribal wars.

***Q: Just a direct follow up to what you were saying... you founded a strategy journal Strategy Science. I am just curious if in that journal there are things that you are doing to speak to this curiosity?***

I took part of Arie Lewin's design for *Organization Science*. I'm an old enough scholar to remember when *Org Science* was a start-up, and the discussion about it at the time as to whether the world needed another journal. It turned out that there was a role for journal that cared about the phenomena of organizations, was willing to take some risk and lead by senior scholars who could make judicious use of that risk taking opportunity. I took a similar approach in setting up *Strategy Science*, both in terms of the values (identifying interesting work that might influence the conversation in the field) and in the design of the Journal's structure. *Strategy Science* has a flat power structure and we started with ten outstanding Senior Editors who were all capable of making the tough calls regarding the merit of the manuscripts they were evaluating. Also having 10 intellectually diverse Senior Editors allowed me to make a pretty good match between the manuscript and their expertise. That high quality match and the stature

of the Senior Editors allows them to be editors, not aggregating polling data and respond in the form, “Oh you’ve got three reviews, that’s interesting. Some like it and some don’t. Make them happy.” My pitch is, reviews are really important, but they are important to the editorial judgment. You can disagree about editorial judgment, but providing a role for that judgment is a really critical element to allow for the consideration of interesting, possibly non-standard work. Maybe I will elaborate more and add another element to that consideration. Broadly, there is insight and “do we believe in it” element in judging a manuscript. There’s technical correctness and the interestingness. In my *Organization Science* role, I found reviewers are much more energized on technical correctness. Senior Editors are more like “OK, but does anyone care?” I am trying to further reinforce that “do we care” sensibility at *Strategy Science*. As a boutique journal with a flat structure, I think we will be able to do that.

***Q: You spoke about those contrasting sensibilities, you used the term quite a bit, how can we in life use the strong sensibilities to inspire us in the ways you described, as opposed to the institutional and other pressures that seem to bring us down?***

That’s a really hard question. We as senior scholars when making personnel decisions, are we really counting the ideas and impact and are we truly embracing those sensibilities? Surely the easiest thing is counting bullet points on the vita. As a senior scholar, a letter writer, and journal creator, I have tried to create vehicles, exert influence in individual instances and on contexts (departments, journals) to support those values. Obviously, a scholar cannot neglect the instrumental entirely. However, as you lean further on the instrumental, where does that lead you? McKinsey pays better than being a professor, so unless you seize on and enjoy the freedom and flexibility that an academic position offers as to the ideas you pursue, McKinsey will offer a better value proposition. I guess my pitch is thinking about yourself as having a career rather than a job at a particular institution. Yes, you are in a particular institution at any given point and you want to be part of it, you want to contribute and do not want to feel like a

short-timer. In fact, you may want to be there for a long duration. But I think sometimes the energy around the possible persistence in a particular role may cause you to make choices you are ultimately not all that happy with. Much like the exploration and exploitation tradeoff, you need a paycheck to survive in the near term while you pursue these broader ambitions. Also I think in pursuing that ambition it is important not to be self-indulgent. Do the hard work—can you operationalize these ideas, can you actually get to the empirics and can you get to an articulated theory, not just a tease. But lovely question with an incomplete answer!

***Q: We are here to celebrate the (research) trajectory, not the result of it. So there were obviously some moments with drama, especially in the publication process, as you tried to communicate the ideas and so on. Can you share with us perhaps what you would consider to be the most dramatic moment, perhaps triumph, going forward?***

I made a very early on choice that had consequence. I was in the economics PhD program within the Business School that had a substantial focus on agency theory at that time. I was a research assistant to Bob Wilson and Kenneth Arrow and did this survey of agency models. In the dissertation itself was an early, possibly first, model of a “two-sided” moral hazard problem. But that was not quite me. I had done work with Jim March while at Stanford and after arriving at Carnegie I started talking with Wes Cohen and Steve Klepper—people stepped in the evolutionary economics tradition. Wes and I started talking about absorptive capacity (see Cohen & Levinthal, 1989, 1990, 1994), Mark Fichman and I were talking about the dynamics of interorganizational relationships (see Fichman & Levinthal, 1991; Levinthal & Fichman, 1988), and I did the total career suicide, stupid move. I said, “You know what? I’m not going back to the dissertation topic” and associated papers. I left my dissertation in the hard drive except for the survey piece. I was going to follow what I was excited about at that time. I think it was probably off-putting to some people at Carnegie—“*I thought we hired an economist, what happened to him?*” But you know, maybe my own foolishness and naïveté and outsider

identity is kind of liberating. I care what people think because that is feedback: I want to do better work and learn. But that is different than normative isomorphism. Whatever stubborn and foolish career disruptive choices I made, were not obvious choices and may not have made a lot of sense at the time. The first version of absorptive capacity was just the model. We sent it out to RAND, and they say it is interesting, but it really needs a bit more game theory for them to like it. But then Wes realized that we could link our theoretical ideas to the empirical data he and David Mowery had built on R&D investment at the line-of-business level linked to the Yale Survey of R&D managers. We could actually test our theoretical model with this data. So Round Two of that project became the *Economic Journal* article. Recently the *Economic Journal* celebrated its first 125 years and identified 13 articles during that time span to recognize, and our piece was one of those—a wonderful and humbling honor. Perhaps this anecdote illustrates some of the virtues of slow learning and obstinance. But at the same time, you don't want to be delusional. If the world is telling you something, there is a message. If I get an article rejected, I essentially treat that as akin to an R&R—just without the long boring memo to the reviewers. It's a learning opportunity. You could be annoyed at the reviewers if they did not appreciate your wisdom and insight, but ultimately you have to realize you did not express the work in a way that was sufficiently compelling for them to see the insight. That's your bad. You ultimately have to express your ideas with sufficient clarity and power that other people will see it. So I'm comfortable being hard on myself and perhaps well positioned for our profession that frequently offers up these opportunities.

***Q: We've seen your work throughout and see how the "core" of Dan Levinthal has always been there, accompanied by inspirations from other exciting colleagues and students. So as a closing, I wonder if you could give any advice to the junior scholars who are still exploring now, who may later become source of your inspiration?***

We already discussed about my encouragement to follow ones intellectual interests and passion in a way that connects to real life outcomes. But one additional important advice is to be a bit ambitious. Yes, you are drawing on prior work, but prior work is always incomplete. I remember an early doctoral student who would go to examine the literature and come back saying “Oh, X kind of already said this.” Yes, you don’t want to ignore the past, you are building on it. Acknowledge it, but then you extend and go from there. So be ambitious and not be intimidated. Actually, we are really blessed—what a wonderful job! Yes, grading bluebooks is not so exciting, but this other piece of what we get to do, this incredible freedom and opportunity—take advantage of that!

### **From Dynamic Capabilities to Microfoundations:**

#### **A Conversation with Constance E. Helfat**

*By Ekin Ilseven and Minh Vo*

#### **Early Times: Initial Path and Influences**

***Q: If you look at your pre-tenure period, when you were graduating from Yale Economics in '85. What was going through your mind? What were your plans back then, and what is academia for you?***

I used to hide the fact that I had an Economics degree when I came into this field, because that didn’t have a very good connotation. Now, I tell people I’m a reformed economist [laugh]... I [was] interested in firms. That’s what my dissertation was about. Economists weren’t interested in firms and therefore, they didn’t know anything about firms. I came out of Yale where Sid Winter and Dick Nelson were; they were renegades. They did evolutionary economics; they actually were interested in firms. [...] So, I did get exposed to that at Yale. In fact, I’m very proud to say that I proofread the first chapter of their 1982 book. The way you proofread is one person reads out loud to another...I got the full dose. This is imprinting; it really does work...

Then, when I was coming out, one of my dissertation advisors Rick Levin, said, “You’re going out to Davis. I’ll tell you what, I’ll send David Teece a note. He’s at Berkeley.” I knew David Teece’s work. David likes to have co-authors. I ended up working with him and he ended up educating me. He was running a seminar series. He was bringing out all these big names, phenomenal researchers who were on the borderline between economics and strategy.

He pulled me along with a couple of his grad students, who were Will Mitchell and Gary Pisano, ...to these seminars every couple of weeks. David Teece was a huge influence. Not only that, he made me come to one of his sessions at the AEA. That’s where I met Cynthia Montgomery and Birger Wernerfelt and Raffi Amit and actually one of my good co-authors, Ruth Raubitschek, a long time ago. Then David said, “Northwestern’s got a job.” I’m like, they’re not going to hire me...[David said:] ”You call Cynthia.”

I ended up at Northwestern [where] I’m still trying to figure out how do I have a conversation and talk to people in strategy. Cynthia and Birger and Raffi started telling me all about the resource-based view. I thought, “Oh! That might work! It sounds like I can think about it more from an evolutionary point of view.” I would say in my early years, I was around some big influences.

### **Bridging Evolutionary Economics to Capability Evolution**

***Q: Following early influences, we would like to focus on the first 15 years of your career. What influence did your dissertation in economics have on your early work and, more specifically, which papers of yours are connected to it?***

My dissertation spanned really, three areas. What I was looking at was, how firms adapted to a major shock. In this case it was the oil price shocks of the ’70s...So, I’ve always had this interest in strategic change. I went to Jim Tobin, who was one of my thesis advisors, and said, “Hey, I want to track how changes in the macro-economy affect firms and how they adapt”, which was a very atypical economics question of course; for strategy, it’s perfect.



He said, “Why don’t you look at the oil shocks?” That’s how I ended up doing oil. When you do that, you start looking at the R&D, and the scope of what they [firms] do. All the investments they’re making. There’s the scope of the firm. Then, who’s making these decisions? A lot of these decisions are made from the top, that’s how I got interested in managers. Then, I started tracing some of these questions through. As I got exposed, I started thinking about my R&D data because I saw patterns in them that I didn’t write about in my dissertation. I started augmenting the data, investigating them more and I realized that what I saw were persistent differences between firms in their R&D data. Which is, again, perfect to understand the heterogeneity [of firms].

Then I got exposed to David Teece and complementary assets. I realized this also could explain some of the adaptation in firms. Where you saw them placing some of their R&D bets had to do with what other assets they had. Did they have, for example, coal assets that they could use for some of their synthetic fuels R&D?

### **Research on Different Levels of Analysis**

***Q: Until 2015, the level of analysis seems to be diverse and it seems to have a linear progression down to the level of the firm’s manager, as you were combining many levels of analysis. Can you walk us through that to get an intuition of how these are related?***

My dissertation was about how managers made choices about where to invest in their assets, including R&D, in response to oil prices. This is strategic change. I’ve got the managers on one hand, I’ve got the firms on the other, and I’m thinking about capabilities and evolution... It’s not an easy match. That’s the first thing because nobody [who] was doing research on managers...[was] thinking this way. It wasn’t until later, when we get to dynamic managerial capabilities in 2003, is where I managed to put them together. That took me a long time to figure out how to put them together. I’d go back and forth, trying to understand questions.

I started out with trying to understand the evolution of R&D within the firm [and] then went to complementary assets. Then, I went down to the co-evolution of knowledge, capabilities and products, because again, I'm trying to figure out how do you put together evolution more broadly, not just within R&D, but as firms change. What are they building out? How do they diversify over time from an evolutionary perspective? What choices are they making up and down the value chains? Diversifying across the value chains. I started building that out.

I also started...trying to figure out, if we think firms evolve and we think there's capability evolution, can we say with any regularity...how these evolve. It was a thought piece really, [the] capability life cycle is one way of thinking about this. It's just an analogy to the product life cycle.

Then I start[ed] with the corporate effects, and dynamic managerial capabilities, and start[ed] building on my managerial rents and capabilities stuff, but taking it in a more evolutionary way and pulling it together with understanding how firm capabilities evolve. And then I kept building it out.

It's not linear, but it is really informed by the same set of questions the entire time. Just trying to push further and see what can I understand and what new questions do they bring up. For example, [for] managerial cognitive capabilities, I'd been watching all the work on cognition percolate and thinking "This has got to be important. How do I understand this?"...[For] intertemporal economies of scope. Again, let's try and understand over time, can we think of how firms change. So, it's all related.

## **Getting Tenure**

***Q: We're curious about is what did getting tenure meant for you personally?***

For my career, coming from a background that wasn't in the field, it took me quite a while to figure out where I could contribute to the field of strategy with a really strong interest in technology and knowledge. It took a while. One thing you might not notice is I think my first

publication in SMJ was actually '98, '99...It took me a while to figure out how to speak to that audience...That's one issue.

The other thing is you start building out everything you've been thinking about. In terms of the citations, some of the works that actually got published after tenure were things I was working on before tenure. Some of that big spike [in citations] you see was, in hindsight, the reaction to freedom..."I can do what I want!" It's very invigorating, you get all kinds of new ideas that way. I feel like I was in the right place at the right time.

What that spike is, those are my ideas on capability evolution, on dynamic managerial capabilities, intertemporal economies of scope...you have no idea what people are going to find interesting of your own work. You really have no idea. Even the things people do find interesting, it takes a while until it percolates through.

### **Cross-Pollinating through Places, People and Subjects**

***Q: There's this simple notion that, the people that you start around with help shape what you do. You started with Yale economics, you got to U.C. Davis, Kellogg, Wharton, and now you are with Tuck for a while. Did this journey affect your research?***

It's not about the places, it's more about the people. If you think of the people there: Berkeley was David Teece, U.C. Davis was Rick Castanias, who I wrote the Managerial Rents pieces with; Rick's actually a finance guy, incredibly eclectic and broad-minded. Kellogg was Dawn Harris, again an interest in managers, and Cynthia, Birger, and Raffi who were just famous [for the resource-based view]. If you think about those places that I was, these are places ...where my worldview made sense to them. I could learn from the people there, connect with the people there, then take that to the next place. I learned from all these people. Then I ended up at Tuck, where Tuck's very eclectic in their worldview on strategy...This is part of the freedom. Also, I'm so lucky because I have all these people in my history...I've been very lucky just to have been at Tuck for 20 years. That's the longest I've ever lived in one place.

***Q: We observe that you maintain your connections and have a diverse co-authorship network. How did your co-authorship network influence your research?***

First of all, I would say my coauthors are also my friends. Either we became friends and then were co-authors or we became friends because we were co-authors. I find that enriches my life. Like I said, I think the cross-pollination is really helpful. My coauthors have different backgrounds and ways of thinking about the world that are consistent with mine but with different perspectives...different expertise. Sometimes people approach me, sometimes I end up talking with people. Christian [Stadler] came up to me at one SMS conference years ago and said, "...You and I are two of the five people who work on oil in this field. I'd be interested in talking to you more. By the way, I have data."

I look for patterns across areas. Like I said, the cross-pollination. That's how I managed to put managers together with evolution and capabilities, because I'm trying to figure out how these work together. I think that co-authors are really helpful for that. They push you to think. Oliver Schilke came to me interested in dynamic capabilities, [and] had a really good idea for a paper. I thought I'd learn a lot. "Okay, great let's go for it!" This is an expanding network. Many other people will co-author with their doctoral students. [...] I happened to be at schools that by and large did not have doctoral students. Davis didn't have them. Northwestern, at the time, my department didn't have them. I was still an assistant professor by the time I got to Wharton; the doctoral students were not banging on my door. The opportunity to work with doctoral students was somewhat limited. I went to Tuck and we don't have a Doctoral Program...That's part of another explanation for my network.

[An] exception would be Ron Adner. Ron was a Ph.D student at Wharton. I taught a methods class one year; they actually let me do it. The Ph.D students and I, like Ron, we got to be friends. I said, "want to help me with a paper here? It'll be fun." I also had some work with Aija Leiponen whom I met when she was a doctoral student [in Finland] through a

connection of someone who was a graduate student with me at Yale, who was Finnish [an economist] who out of the blue got in touch with me and said, “Hey, I’ve got this doctoral student in strategy, I could really use some help.” Those are the only two people I met when they were doctoral students who I ended up co-authoring with.

### **Three Streams of Inter-Linked Research**

***Q: What’s surprising to us is that the work that you published out of your dissertation fit into three streams at the same time. All the time you seem to be working simultaneously on so-called different interests. As young scholars, we’ve both been to doctoral workshops and were told to have an identity, do one thing at a time and then maybe switch to something else. Was that your approach and how did you do it?***

You see, I go back and forth and I triangulate. Scope of the firm is vertical integration with David (Helfat and Teece, 1987). That was early on. I started thinking about managers with my colleague there [at Davis]. I started building out some of the technological change and capability evolution [stream]. The only other one [article] that I thought was important to put in two categories [of research streams] was product sequencing. It really was simultaneously about a heavy role for technological change and the evolution of firms. It was about going up and down the value chain with new products and diversifying across value chains. They are both, scope of the firm and technological change. If you were to take my work and go paper by paper, you’d have to cross code most of them, many of them. They would be technological change and scope of the firm, or top manager and technological change/capability evolution. The most disparate category is the managers. For a while, I stopped working on it. [...] 2003 was dynamic managerial capabilities: “These things bothered me in the back of my head. I’ve got to figure this out. What’s the role of the managers in the way I think about the world? What I’d been seeing other people write?” The light bulb went off. “Oh! Managers have a dynamic of capability. Bingo. Now I can bring these things together.” I would say over time my main

stream really did evolve into technological change, scope of the firm, capability evolution...[and] when I could fold in the managers, I would do that. I would say that's how I would characterize my work...

This is, like I said before, really non-linear. If and when somebody wants my advice about building a career, I answer *don't do what I did*. It's intellectually satisfying but it's an incredibly difficult way to make progress, because you're trying to triangulate between different things. It's really hard for everyone to know your work because there are disparate audiences for these sorts of things. There is a subset of people who are interested in managers, technological change, capability evolution, but it's a hard way to do this.

The only way I managed to do it was to go back and forth. Look at capability life cycles. I was trying to understand whether I can think of something regular about how capabilities evolve inside firms. It's got a time dimension to it, at the same time I'm thinking about how does the product space evolve inside of firms. What implication does this have for the scope of the firm? That's product sequencing (Helfat and Raubitschek, 2000). I merged dynamic managerial capabilities with an interest in corporate effects. Ron Adner and I were trying to figure out how to put in the manager effects. We got this idea, "What if we looked at the big decisions managers make and see if there's an impact from the top of the firm?" We looked at restructuring and said "This has got to come from the top." [...] All these pieces fit together even if it's not super obvious. If you go from product sequencing, you start thinking more generally "How do firms transition?". Intertemporal economies of scope (Helfat and Eisenhardt, 2004) was how firms redeployed resources from one time period to the next rather than having synergies at a point in time. You get these economies of scope over time. It's a different way to see firms evolve.

To tell you the story of how I got that idea; this is again, about the cross-pollination and the triangulation, and other people who are really important. There's Kathy Eisenhardt. Kathy

does really different work than I do. She does it with a really different method than I do. She's qualitative, she's highly inductive. She came and presented this paper with Charlie Galunic on Omni! She's talking about how Omni is doing things and the light bulb went off: "I don't understand what is going on at Omni, how can I think about it from what I know?" I said, "Oh I have a completely different and complementary way of possibly explaining what's going on at Omni and how they make choices to move from one business to the next." [...] That's how I got to be colleagues with Kathy. That's how I got the idea I never would have thought of [intertemporal economies of scope], if I hadn't been paying attention to someone who thinks differently than I do, but has the application to firms that I'm interested in. Understanding some of the core questions but from a completely different method, perspective... It's still one of my favorite papers, honestly. You always have your favorite papers.

#### **Directions as an editor and a researcher**

***Q: As a co-editor of SMJ, where do you think the literature is going or should go? Where would you be going and what do you plan for research?***

This is fun. First of all, the methods pieces... So, the great thing about being an editor is [that] you can say what you've been wanting to say for a long time and you have a platform. It's perfect. These are actually things I've been thinking about for a long time. The first one I wrote was in *Strategic Organization* (Helfat, 2007)...about stylized facts because we need empirical regularities. Otherwise, what do you have to explain? It's great to have one-off stuff but we like to build knowledge here. When I became an editor, I started talking to Rich Bettis about replication, and he'd seen my article on stylized facts and we started talking. Then he said, "Why don't we do a special issue?" I'm like, "Oh, Bingo!"

We started writing other essays on what do we think are some important ways to understand quantitative research. For a few years now we have a new policy at SMJ about quantitative work, which is - my translation for this policy is - don't throw out the baby with

the bath water, meaning: Just because you haven't gotten everything nailed down, we will still publish it if you do a good faith effort of trying to explain your data as well as you can, especially if it's useful and interesting for people to try and understand the research.

Where I see the field going. I don't have a crystal ball any more than anybody else. There are hot topics. I remember way back when being at some SMS or Academy session where Dick Rumelt got up and said, "Strategy is a faddish field." He didn't mean it in a bad way. He meant look, there's things going on in the real world that managers are really concerned with and strategy wants to know about this stuff. These things go through cycles. We still do that in our field. We keep the core interest but are also getting a lot of CSR papers. Broadly defined, non-market strategy, CSR... At first I thought, this is just what the young people are really into. But actually, I don't think that's entirely right, because you're listening to CEOs, agonizing about this over and over again. I think maybe we were just a little ahead of the curve.

There was a session last year at the SMS where people talked about the fragmentation of the field. We're all bifurcating. My answer is that we still have core questions in our field. How do firms differ? Why are they different? And over time? Innovation is a core area. Diversification is a core area, including the role of headquarters. International... Global... These are core areas. My hope is... people working in these newer areas...start connecting those newer areas to our core questions. If you do that, the field doesn't fragment so much. I'm starting to see a few people doing what I would call CSR, but they're starting to look at divestment and diversification. "Yes! Let's bring some of this back." We do see some new topics, but I think that one solution to the worry about fragmentation is to always start bringing it back to the core questions.

About where I'm going: I'm doing two things that I'm very excited about, in addition to other stuff. I'm still going to do dynamic capabilities. I do think it's important even if some



people differ. What I am doing though, is trying to bring in the managers, think of new ways to bring managers and organizations together in one single analysis. I honestly wish I thought of the term micro-foundations. Not for the “angst” that it’s generated - maybe it’s good to have this debate -, but because it brought the individual back into the [rest of the] mainstream...If you think about the core questions, managers are hugely important as the individual. The good thing that the micro-foundations stream has done for the field is to say “We’ve got to connect these [the individual and the organization].” I’m doing a couple of empirical pieces on that. This is an empirical question. Who, how, when, how much? You need to also contrast with things that are going on at the organization level of analysis. I have two empirical papers. One is with Christian [Stadler and Gianmario Verona] where we are trying to do this, relying on some of his data. The other thing I’m super excited about is...something that I’ve been thinking about for 25 years. I finally found a route in by talking to one of my wonderful colleagues, Giovanni Gavetti. There was all this conversation, when I was at Wharton about...shaping, but was very diffused. People say, “Well, firms co-evolve with their environments...” and I thought “This is not helping me. I need some precision. I need some mechanisms. What are they shaping, what does shaping mean anyway?” Giovanni and I have a theory piece [on shaping]; we’re going to do more. I think it’s an important piece.

### **Reflections with Audience**

**Q (Audience):** *I have a question about shaping, when you’re talking about shaping, how do you envision the boundary conditions of this idea. For example, are you talking about shaping institutions or cognitive frames or others?*

You could think of it in any number of ways. My question which is still a large question is about shaping the environment that’s external to the firm. You could think about shaping institutions. The question that we had to work out in this paper (Gavetti, Helfat, and Marengo, 2017), it took quite a while, I’m happy with the answer we came up with (but you might have

a different view), which is that we already know that firms react. Take game theory: ...[firms] play a strategic game...there's a payoff structure... There's work in economics on [firms] trying to shape the regulators, how they react. That's not really what we mean. In those settings, the payoffs are exogenous to the firm. All the firm is doing is taking what's already given to them. If we take this approach, we can affect the regulator in this way. If we take this new product introduction, we think our other competitors react in another way. Maybe there's some uncertainty about the payoffs, but you're not shaping what the actual payoffs are. What we mean is shaping the actual payoff. We're going to shape what the regulators' choices are, for example, or what the other firms' choices are. So, if you ask me, this [Apple] shaped Samsung's handset choices... That's how I've come to think about shaping.

**Q (Audience):** *My only concern with your presentation was the recommendation to doctoral students, don't do what I did. I think that's actually quite not right. I think what marks your work is you had one question. You really hammered that. The beauty of this, you have these modules. You have some modules of the market level, some modules of the firm level, some modules of the manager level. The phenomena are not just at one level. You have focused on that phenomena from a modular perspective; you went really deep within that. Stay very focused, but have an overarching question, that will take you from that dissertation to something bigger, some big research program.*

It's good advice: The importance of having an overarching question that will motivate you for your entire career, it's great, it just keeps you going. You're never going to answer it to your satisfaction. It makes you read and it makes you pay attention to other people; it makes you talk to other people. It's great for the field. It's great personally. That is right and you are correct and I'm still trying to figure out the same question.

Personally, the difficulty with this is that you have these disparate people who don't know your other work and I wasn't willing to choose... There was plenty to do in the one stream

but I wasn't willing to choose. Maybe it would have been easier if I had chosen. For me personally... It worked out great in the end. I'm really happy. I love my research. I love all the people I've met. All the ideas I've been exposed to... I'm looking forward to many more years. Thank you guys for listening.

## **Conclusion**

This interview series provides unique insights into the profound contributions of three eminent management scholars: Professors David Teece, Daniel Levinthal, and Constance Helfat. Through candid conversations, we have delved into the depths of their intellectual journeys, revealing not only the academic impact of their work but also the personal sensibilities, struggles, inspirations, and collaborations that underpin their scholarship.

Professor David Teece's interview highlights his pioneering perspectives on competitive strategy, economic development, and dynamic capabilities. His relentless pursuit of fundamental questions in the realm of strategy has not only earned him international acclaim but has also positioned him as a dynamic force in bridging theory and practice. Teece's journey serves as a compelling call to scholars, urging them to undertake interdisciplinary research and embrace intellectual risk-taking.

Professor Daniel Levinthal's narratives unveil the inner workings of his groundbreaking ideas in management, such as absorptive capacity and adaptation in rugged landscapes. This interview sheds light on the essence of scholarship—the intellectual and ethical mentorship that defines a true scholar. Levinthal's dedication to nurturing the next generation of scholars serves as a beacon of inspiration for aspiring academics in the management field.

Professor Constance Helfat's conversation offers a glimpse into her transformation from an economist to a visionary strategist. Her contributions to the fields of the scope of the firm, strategic change, and the role of managers reflect a harmonious blend of broad perspective

and empirical rigor. Helfat's approach, characterized by the delicate balance between open-mindedness and focused inquiry, serves as a testament to the vitality of research.

In essence, these interviews collectively illuminate the multifaceted nature of scholarly pursuits, transcending mere academic recognition to encompass the heart and soul of the research process. These scholars stand as beacons of inspiration, guiding both aspiring and seasoned researchers toward a deeper understanding of management's fundamental questions, advancing the field in the aspired directions (Leiblein and Reuer, 2020). Their enduring legacies will continue to shape and enrich the field of management research as we continue on a path of soul-searching to better address the needs of academia, industry, and society at large.

## References

- Barney, J. B. (1986). Strategic factor markets: Expectations, luck, and business strategy. *Management science*, 32(10), 1231-1241.
- Cohen, W. M., & Levinthal, D. A. (1989). Innovation and Learning: The Two Faces of R & D. *The Economic Journal*, 99(397), 569–596. JOUR. <http://doi.org/10.2307/2233763>
- Cohen, W. M., & Levinthal, D. A. (1990). Absorptive Capacity: A New Perspective on Learning and Innovation. *Administrative Science Quarterly*, 35(1), 128–152. JOUR. <http://doi.org/10.2307/2393553>
- Cohen, W. M., & Levinthal, D. A. (1994). Fortune Favors the Prepared Firm. *Management Science*, 40(2), 227–251. JOUR. Retrieved from <http://www.jstor.org/stable/2632763>
- Cyert, R. M., & March, J. G. (1963). *Behavioral Theory of the Firm*. Englewood Cliffs, NJ: Prentice-Hall.
- Fichman, M., & Levinthal, D. A. (1991). Honeymoons and the Liability of Adolescence: A New Perspective on Duration Dependence in Social and Organizational Relationships. *Academy of Management Review*, 16(2), 442–468. <http://doi.org/10.5465/AMR.1991.4278962>
- Gavetti, G., Helfat, C.E. and Marengo, L., 2017. Searching, shaping, and the quest for superior performance. *Strategy Science*, 2(3), pp.194-209.
- Gavetti, G., & Levinthal, D. (2000). Looking Forward and Looking Backward: Cognitive and Experiential Search. *Administrative Science Quarterly*, 45(1), 113–137. JOUR. <http://doi.org/10.2307/2666981>
- Grove, A. (1997). Only the paranoid survive: How to identify and exploit the crisis points that challenge every business.
- Helfat, C.E., 2007. Stylized facts, empirical research and theory development in management. *Strategic Organization*, 5(2), pp.185-192.
- Helfat, C.E. and Eisenhardt, K.M., 2004. Inter-temporal economies of scope, organizational modularity, and the dynamics of diversification. *Strategic Management Journal*, 25(13), pp.1217-1232.
- Helfat, C.E. and Raubitschek, R.S., 2000. Product sequencing: co-evolution of knowledge, capabilities and products. *Strategic management journal*, 21(10-11), pp.961-979.
- Helfat, C.E. and Teece, D.J., 1987. Vertical integration and risk reduction. *JL Econ. & Org.*, 3, p.47.

- Kogut, B., & Zander, U. (1992). Knowledge of the firm, combinative capabilities, and the replication of technology. *Organization science*, 3(3), 383-397.
- Leiblein, M.J. and Reuer, J.J. (2020). Foundations and futures of strategic management. *Strategic Management Review*, 1(1).
- Levinthal, D. A. (1991). Organizational Adaptation and Environmental Selection-Interrelated Processes of Change. *Organization Science*, 2(1), 140–145. JOUR.  
<http://doi.org/10.1287/orsc.2.1.140>
- Levinthal, D. A. (1997). Adaptation on rugged landscapes. *Management Science*, 43(7), 934–950. JOUR.
- Levinthal, D. A., & Fichman, M. A. (1988). Dynamics of interorganizational attachments: Auditor-client relationships. *Administrative Science Quarterly*, 33(3), 345–369.  
<http://doi.org/10.2307/2392713>
- Levinthal, D. A., & March, J. G. (1993). The myopia of learning. *Strategic Management Journal*, 14(S2), 95–112. <http://doi.org/10.1002/smj.4250141009>
- Levinthal, D., & Posen, H. E. (2007). Myopia of Selection: Does Organizational Adaptation Limit the Efficacy of Population Selection? *Administrative Science Quarterly*, 52(4), 586–620. JOUR.  
<http://doi.org/10.2189/asqu.52.4.586>
- Nelson, R.R. & Winter, S.G. (1982). *An Evolutionary Theory of Economic Change*. Cambridge, MA: Belknap Press
- March, J. G. (1991). Exploration and Exploitation in Organizational Learning. *Organization Science*, 2(1), 71–87. JOUR. Retrieved from <http://www.jstor.org/stable/2634940>
- March, J. G., & Simon, H. A. (1958). *Organizations*. New York: Wiley.
- Porter, M. (1980). *Competitive Strategy*, New York: Free Press.
- Smith, A. (1822). *The theory of moral sentiments* (Vol. 1). J. Richardson.
- Smith, A. (1937). *The wealth of nations [1776]* (Vol. 11937). Na.
- Spence, M. (1984). Cost Reduction, Competition, and Industry Performance'. *Econometrica*, 52(1), 101–122. Retrieved from <http://www.jstor.org/stable/1911463>
- Teece, D. J. (1980). Economies of scope and the scope of the enterprise. *Journal of economic behavior & organization*, 1(3), 223-247.
- Teece, D. J. (1981). Internal organization and economic performance: An empirical analysis of the profitability of principal firms. *The Journal of Industrial Economics*, 173-199.
- Teece, D. J. (1982). Towards an economic theory of the multiproduct firm. *Journal of Economic Behavior & Organization*, 3(1), 39-63.
- Teece, D. J. (1986a). Profiting from technological innovation: Implications for integration, collaboration, licensing and public policy. *Research policy*, 15(6), 285-305.
- Teece, D. J. (1986b). Transaction Cost Economics and the Multinational Enterprise: An Assessment" *Journal of Economic Behavior and Organization* 7:1 (March 1986), 21–45.
- Teece, D. J. (2006). Reflections on “profiting from innovation”. *Research Policy*, 35(8), 1131-1146.
- Teece, D. J. (2012). Dynamic capabilities: Routines versus entrepreneurial action. *Journal of management studies*, 49(8), 1395-1401.
- Teece, D. J., Pisano, G. & Shuen, A. (1997). Dynamic Capabilities and Strategic Management”. *Strategic Management Journal* 18:7 (August 1997), 509–533.
- Tushman, M. L., & O'Reilly III, C. A. (1996). Ambidextrous organizations: Managing evolutionary and revolutionary change. *California management review*, 38(4), 8-29.
- Tripsas, M., & Gavetti, G. (2000). Capabilities, cognition, and intertia: Evidence from digital imaging. *Strategic Management Journal*, 21(10), 1147–1161.
- Williamson. O.E. (1975). *Markets and Hierarchies*. New York, NY: Free Press