Journal of Management
Vol. 47 No. 7, September 2021 1663–1676
DOI: 10.1177/01492063211015272

The Emergence of Resource-Based Theory: A Personal Journey

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I have recently been encouraged to share my personal reflections on the emergence of resource-based theory. In many ways, I have been reluctant to do so, at least in print, since any such effort would necessarily reflect my idiosyncratic view of this history. A complete discussion of both the people involved in the development of resource-based theory and the context within which this theory developed in the field of management would, I suspect, require the objective eye of a third party. In this way, I certainly do not qualify to write such a history. However, when the 30th anniversary of the publication of the Special Theory Forum on Resource-Based Theory in the Journal of Management *came around, I thought it might be time to put down on paper—a quaintly old-fashioned phrase—my own recollections of this history. In doing so, I decided to make no pretense that this is an objective or rigorous historical effort. Rather, these are the reflections of a strategic management scholar, coming toward the end of his career, about a time, now over 30 years ago, when resource-based theory did not yet exist. I have not tried to verify my reflections by appeal to historical documents, except for any papers I and others have published. I did pass this essay by many of the people mentioned in it—to see if my memories were consistent with their memories—but that is as far as I have gone in verifying the "facts" I share in this essay.*

Keywords: *resource-based view; stakeholder theory; resource dependence*

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Acknowledgments: Comments and suggestions from Connie Helfat, Bill Hesterly, and Anita McGahan were helpful in developing this essay, as were comments from Kim Barney, who, after all, was there.

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PhD Program

I graduated from Brigham Young University with a bachelor of science degree in sociology in December of 1975. As an undergraduate, I learned two things about college: I liked it and I was good at it. So, abandoning my original ambition to become a lawyer, I applied to sociology PhD programs at several universities, with the goal of becoming a sociology professor. I was accepted at the University of Washington and the University of Chicago but accepted the offer from Yale University because they gave me the most money. After a semester off—making money driving delivery trucks—my wife, our 3-month-old baby daughter, and I drove from Phoenix, Arizona, to New Haven, Connecticut, to begin a new educational adventure at Yale University.

I quickly fell into the rhythms of PhD student life: taking seminars, writing papers, engaging in debates with fellow students. After arriving in New Haven, I decided that I would focus my studies on the more technical aspects of the field of sociology: statistics, network sociology, and mathematical sociology. In this context, my work with Professor Scott Boorman was extremely important and has influenced my thinking the rest of my career. I still remember how he led our seminar through his formal model of the effects of weak social ties published in what was then known as the *Bell Journal of Economics* (Boorman, 1975). Since that experience, I have always appreciated the clarity and precision that can be associated with formal theorizing. While my own theorizing has been mostly verbal in character, I have always striven to make my verbal theories as carefully articulated as formal theories but more accessible to a broader audience.

In my second year of the sociology PhD program, I had to choose a substantive area of work in sociology, in addition to mathematical sociology/network sociology, to emphasize for my preliminary exams. It happened that that semester, none of the seminars that were offered in the Sociology Department were particularly interesting to me. Back then, sociology was a relatively fragmented field, and seminars focused on very specific topics, for example, the sociology of medicine, the sociology of sport, the sociology of crime and criminality, and so forth. It so happened that, back then, the Yale School of Organization and Management (SOM) was right across the street from the Sociology Department. So, I walked across the street and found a seminar that I thought might be interesting, Organization Theory, taught by Professor Bob Miles. A quick perusal of the syllabus made it clear that this seminar was about the "sociology of organizations."

Thus, I took my first class in a business school.

I rapidly became more interested in the seminars being taught at SOM than those being taught in the Sociology Department. In a short period of time, I took an additional seminar in organization theory from Professor John Kimberly and seminars in organizational social psychology from Professor Clay Alderfer and Professor Rosabeth Kanter,[2] and I acted as a teaching assistant for an MPPM (which became the Yale MBA degree) class cotaught by Professor Kimberly and Professor Richard Hackman.[3]

At some point, I decided that I would try to set up the one and only joint PhD degree program between the Sociology Department and SOM at Yale. With the support of the chair of the Sociology Department, I asked Professor Kimberly to be my chair and received permission from the graduate school dean to create this special degree program. As part of this special dual degree, I agreed to take two sets of qualifying exams: one for sociology and one for the administrative sciences program within SOM.[4]

Each exam was 2 days long and involved answering six or so essay questions per day. My 2 days of examination in sociology focused mostly on network sociology and mathematical sociology.[5] My conclusion about those literatures, and especially the network sociology literature, was that, with a few notable exceptions (e.g., Cartwright & Harary, 1956; Granovetter, 1975; White, 1970; White, Boorman, & Breiger, 1976)—this literature was largely descriptive and atheoretical.[6] This was not an encouraging conclusion for me, and I decided that I would probably not focus on network sociology as an area of specialization (after I finished my dissertation).[7]

My exam in organization theory for the business school side of my PhD program was no less discouraging. This was early days for organization theory. The only established theoretical traditions in the field were resource-dependence theory (Pfeffer & Salancik, 1978) and contingency theory (e.g., Lawrence & Lorsch, 1967; Thompson, 1967).[8] Over time, I have come to more fully appreciate some of this early theory work, especially Lawrence and Lorsch (1967), but at the time, I thought that the organization theory literature was theoretically rather thin. Certainly, I had not felt intellectually challenged by the readings for my exams. I thought that I could read and understand these theories easily and that they did not generate many counterintuitive insights. There was, for me, more common sense in this literature than deep theoretical insight. Again, this was not an inspiring conclusion for a person who was beginning to contemplate a career in organization theory in a business school.

Then Professor Bill Ouchi came to SOM to give a research seminar. I had never heard of him at the time, but I went to his talk just a few weeks after passing my exams. What I heard was an incredibly articulate scholar describing a theoretical perspective that was totally new to me. It turns out that he was presenting a version of his paper "Markets, Bureaucracies, and Clans," which was later published in *Administrative Science Quarterly* (Ouchi, 1980). While Bill was very clear and precise in his language, I had no idea what he was talking about.

However, to me, this was good news. Here was an obviously very smart person talking about what appeared to be a logically coherent theoretical point of view, and it made no sense to me. I thought to myself, "This is a theoretical perspective in organization theory that challenges me to think differently about why organizations exist, how they operate, and how they create economic value." After Professor Ouchi was done, I went up and asked him what else I should read to understand this perspective. He recommended that I read Oliver Williamson's (1975) *Markets and Hierarchies*. So, I went to the library,[9] borrowed a copy of Williamson's book, and read it.

I found it even more incomprehensible than Professor Ouchi's speech.[10]

But, again, this was good news to me. These people—Ouchi and Willamson—were clearly very bright. They were clearly engaging in a serious research activity and in serious conversations with other serious people. But I had no idea what they were talking about.

At that point, it was too late for me to change my dissertation topic. But in the back of my mind, I remained intellectually intrigued by what I came to know as transactions cost economics and committed to learn more.

Early Days as an Assistant Professor

For the next several months, I worked diligently on my dissertation. During this time, I demonstrated conclusively that I could run the computer and write fairly good computer code—in SPSS, SAS, Fortran, and APL—but I learned almost nothing about organizations. Nevertheless, I went on the job market, to business schools, where I had two in-person interviews (University of California–Los Angeles [UCLA] and Stanford) and one offer (UCLA), which I accepted.

Since speaking at Yale, Professor Ouchi had moved from Stanford to UCLA. I'm not sure, but I think that even if Stanford had given me an offer, I probably still would have taken the UCLA job. I did not fully understand this at the time, but my move to UCLA was essential to the development of what became resource-based theory.

Even before I started at UCLA, in the fall of 1980, Professor Ouchi organized a conference to which I was invited. It was a conference on the "new institutional economics." Of course, I went, even though I didn't actually know the difference between "new" institutional economics and "old" institutional economics.

At this conference, sponsored by the consulting firm Booz Allen Hamilton, Professor Ouchi assembled a group of scholars that represented some of the best and brightest scholars in this area of work, including Armen Alchian, Harold Demsetz, Ben Klein, Bill McKelvey, Bill Ouchi, Richard Rumelt, David Teece, and Oliver Williamson. To say that I felt a bit intimidated by this collection of scholars is a gross understatement. I arrived at this conference deeply interested in this line of reasoning but largely ignorant of its logic or implications. I left the conference with a solid grounding in this approach to analyzing organizations, convinced that this was the future of organization theory.

I also learned that my economic training was deeply lacking.[11] If I was going to contribute to organization theory using this "new institutional economics," I was going to have to increase my understanding of economic theory. As soon as I arrived at UCLA, I began a program to teach myself economics.

As part of this effort, I visited several professors in different fields and asked them, "As a new organization theory scholar, what in your area of expertise should I read?" One of the professors I visited was Professor Tom Copeland in the UCLA Department of Finance. Tom recommended that I read Jensen and Meckling's (1976) article on agency theory. I got a copy of the article and gave it a quick read, much in the same way I had read many organization theory papers in preparation for my qualifying exams. After this quick read, I went back to Tom's office and summarized the paper. His response: "No. You're wrong. You don't understand the paper."

While embarrassed for having wasted Professor Copeland's time, I was nevertheless excited about the possibility of tackling yet another intellectual challenge. So, I took up my copy of Jensen and Meckling (1976), and I did nothing but read this article for 6 weeks. It took so long, especially in the early parts of the paper, because every sentence implied a literature about which I knew nothing. I would read a sentence, spend 2 days delving into the literature upon which that sentence built, read the next sentence, delve into that literature, and so forth. It was an extremely rewarding intellectual experience.

Six weeks after wasting Professor Copeland's time, I went back to his office and explained the central messages of Jensen and Meckling (1976). He told me that I now understood their argument.

In fact, I was deeply impressed by their argument. Not so much by agency theory per se—although I have found this to be a convenient theory in some of my work in the field of entrepreneurship (e.g., Barney, Fiet, Busenitz, & Moesel, 1996; Busenitz, Moesel, Fiet, & Barney, 1997; Fiet, Busenitz, Moesel, & Barney, 1997; Moesel, Fiet, Busentiz, & Barney, 1996). Also not so much by their theory of capital structure—because capital structure (at the time) was not particularly interesting to me. What I was most impressed by was efficient capital market theory. To summarize this theory in a single sentence, if capital markets are efficient, in the semistrong sense (Fama, 1970), then all publicly available information about the value of an asset will be reflected in its price. Now, since these early days, there has been a great deal of debate about whether or not markets are efficient in this sense (Summers, 1986). Much of this debate has been about whether or not market prices represent the true underlying value of an asset. This is a great question but has nothing to do with semistrong market efficiency per se. Semistrong market efficiency asserts only that in these kinds of markets, publicly available information about the value of an asset will be reflected in the price of this asset.

It turns out, as I would discover later, that semistrong market efficiency introduces an important constraint in strategic management theory, a constraint that was not yet acknowledged. If capital markets are semistrong efficient, then the price of a firm's equity will reflect all publicly available information about the value of that equity, including information about the strategy of that firm. If other factor markets (Barney, 1986b) besides capital markets are semistrong efficient, then the price of access to resources in these markets will reflect the value of these resources in enabling a firm to choose and implement its product market strategies. If these factor markets are semistrong efficient, then even if firms successfully choose and implement product market strategies, they will not generate economic profits from doing so.

But I get ahead of myself. All I sensed after reading—and I mean, really reading—Jensen and Meckling (1976) was that capital market efficiency introduced an interesting constraint into my thinking. I knew enough about theorizing that constraints like this were very useful. They enabled you to ignore some things, like capital market efficiency, to focus on the implications of these constraints for other things, like the ability of firms to earn an economic profit.

The Switch to Strategic Management

For my first couple of years at UCLA, I saw myself primarily as an organization theorist, trying to understand the implications of the "new institutional economics" for phenomena that were interesting to organization theorists: the boundaries of a firm, a firm's organizational structure, compensation in a firm, and so forth.[12] Then, around 1983, a teaching need arose in the department, and I was asked to teach a class in strategic management.

There had been a master's class in strategic management at Yale, but I had not taken it. Indeed, there were no PhD seminars at Yale in strategic management. I knew that teaching Strategic Management involved the use of cases, and I thought this would be a good skill to learn. So, I agreed.

Indeed, the first quarter I taught Strategic Management, I did teach lots of cases but provided very little of a theoretical framework to the students to help them understand the implications of the cases. It went reasonably well, although in retrospect, I've often thought that my students should have received a rebate from UCLA for the lack of content in this class. However, given my apparent success in the class, my department asked me to teach it again. This time, I thought that in addition to lots of cases, it might be a good idea to read a bit about the field in which I was teaching. Obviously, the dominant author at the time was Professor Michael Porter, so I purchased his book and read it (Porter, 1980).

I hated it. Really, I had a visceral negative reaction to the book, which I couldn't explain.

Upon reflection, my reason for not liking Porter's (1980) book was that it applied a particular brand of economic theory—the structure, conduct, and performance paradigm (Bain, 1956)—that was inconsistent with the efficient capital market theory I learned from Jensen and Meckling (1976). The book kept making recommendations to managers that would generate profits only if capital markets were inefficient, and it did not acknowledge this constraint.

Of course, with a few more years of experience, I came to understand the fundamental importance of all of Porter's early work. Porter was among the first scholars to rigorously apply economic thinking to strategic management. Without Porter, the field of strategic management may have withered and died just as it was beginning.[13] Indeed, I am on record saying that Porter has had more influence on the field of strategic management than any other strategic management scholar ever (Barney, 2002).

But I still disagreed with him. Indeed, I spent most of 1984 writing two papers explaining why. The first was initially titled "Why Michael Porter Is Wrong."[14] It later was published, in *Management Science* in 1986, under the revised title "Strategic Factor Markets: Expectations, Luck, and Business Strategy" (Barney, 1986b). The second was initially titled "Strategy From the Inside Out." It was later published, in *JOM* in 1991, under the revised title "Firm Resources and Sustained Competitive Advantage" (Barney, 1991). Yes—the first draft of Barney (1991) was written in 1984 and published 7 years later.

Resource-Based Theory: The Early Years

Altogether, I had three major publications in the fall of 1986: "Strategic Factor Markets" in *Management Science* (Barney, 1986b), "Organizational Culture: Can It Be a Source of Sustained Competitive Advantage?" in *Academy of Management Review* (*AMR*; Barney, 1986a), and an edited book (with Bill Ouchi), *Organizational Economics*, published by

Jossey-Bass (Barney & Ouchi, 1986). I conceptualized these three publications as follows: *Organizational Economics* provided all the tools needed to understand the theory I was developing, "Strategic Factor Markets" presented a very general theory of how firms can generate economic profits, and "Organizational Culture" showed how this theory might be applied.

Of course, it did not turn out this way. The *Organizational Economics* book sold fairly well, but its links with my other work were not widely recognized.[15] The "Organizational Culture" paper was widely read and cited, but its role as an application of a more general theory of strategy and economic profits was not widely understood. And "Strategic Factor Markets"—the paper that I thought of as revealing an entirely new theory of profit generation in the field of strategic management—was (for many years) ignored.

This was the case, even though "Strategic Factor Markets" barely survived the review process and had been significantly revised in doing so. It had three reviewers: Reviewer 1 said there was nothing new in this paper, Reviewer 2 said that it was too abstract for the field of strategic management, and Reviewer 3 said that it was the most important paper he or she had ever read in the field of strategic management. After five rounds (yes, five) of revision, none of the reviewers had changed their mind. Yet, Arie Lewin, the field editor for strategic management at *Management Science* back then, took a chance and accepted the paper.

After all this work, its publication was followed by the sound of one hand clapping in an echo chamber. My mother was proud. Bill McKelvey, Bill Ouchi, and Dick Rumelt, all UCLA colleagues, thought it was a good paper but did not see the radical theoretical innovation that I saw. But for most of the field, it was as if this paper did not exist.

Over the next several years, I had the objective of trying to show the field of strategic management how important "Strategic Factor Markets" was. I did this in two ways. First, I applied these ideas directly to what I thought were interesting topics in strategic management. For example, in 1988, I published a paper on returns to mergers and acquisitions in *Strategic Management Journal* (*SMJ*) that applied factor market logic (Barney, 1988). Second, I continued to develop the second paper I had written in 1984 ("Strategy From the Inside Out") as a way to "market" the theory that I thought I had developed in the "Strategic Factor Markets" paper. Ultimately, this would become a more general version of the basic ideas presented in my "Organizational Culture" paper published in *AMR*.

But getting this second paper published was not trivial. Indeed, there was a lag of 7 years between when I wrote the first draft in 1984 and its ultimate publication in 1991. I submitted it to *SMJ*; it was rejected. I submitted it to *AMR*; it was rejected. I submitted it to the Academy of Management meetings; it was rejected. I submitted a rewritten version to *SMJ* and then to *AMR*; rejected and rejected.

There are many reasons why this paper was rejected so many times. First, I had a hard time positioning the paper in the strategic management field without insulting most of the strategic management field. Even when I thought I had solved this problem, the field was so dominated by the Porter framework that most reviewers had no idea what I was talking about. Frankly, it also took me several years to learn how to present the arguments in the paper clearly and succinctly.

In the summer of 1985, I moved from UCLA to Texas A&M.[16] In 1989, a colleague of mine at Texas A&M, Professor Ricky Griffin, asked me to become an associate editor at *JOM*. I accepted this invitation and shortly afterward approached Ricky, who was the *JOM* editor, about the possibility of organizing a special theory forum on the resource-based view. He agreed.

Beginning in the early 1980s, an almost "underground movement" of strategy scholars had begun to do work that differed in important ways from the Porter paradigm. One of the first papers in this "movement" was by Professors Rumelt and Wensley (1981). This paper, called "In Search of the Market Share Effect," was published only in the *Academy of Management Proceedings* in 1981. It argued that if the "market for market share" was competitive, then changes in market share could not be expected to generate firm profits.[17] This paper contradicted a notion that was popular at the time—that market share was a primary determinant of firm profitability—and was very influential in the development of "Strategic Factor Markets."

In 1982, Professors Steve Lippman and Richard Rumelt published a paper in the *Bell Journal of Economics* that suggested that the inability of competitors to know why a certain firm was performing well, what they called "uncertain imitability," could be a source of profits, in equilibrium, for a firm (Lippman & Rumelt, 1982). In 1984, two more strategic management papers in this "movement" were published: one by Professor Rumelt as a chapter in an edited volume associated with a strategic management conference (Rumelt, 1984) and one by Birger Wernerfelt (1984) as an *SMJ* paper titled "A Resource-Based View of the Firm."[18] Rumelt's paper introduced the idea of "barriers to imitation"—a concept that continues to have currency in resource-based theory, and Wernerfelt's paper developed a "dual argument"—suggesting that all the conclusions derived from arguments about competitive advantages in product markets could also be derived from arguments about competitive advantages in resource markets.

The year 1986 saw the publication of several additional resource-based papers, including my "Strategic Factor Markets" (Barney, 1986b) and "Organizational Culture" (Barney, 1986a) papers and a paper by Wernerfelt and Montgomery (1986) in the same issue of *Management Science* as "Factor Markets" that questioned then-popular approaches for analyzing the attractiveness of an industry. The years 1988 and 1989 saw the publication of my *SMJ* paper using resource-based theory to analyze returns to mergers and acquisitions (Barney, 1988)[19] and Hansen and Wernerfelt (1989) *SMJ* paper on the importance of organizational factors in understanding the determinants of firm performance.[20]

This growing interest in the relationship between firm-level resources and capabilities and firm performance was also reflected in conferences that were being held, mostly among younger strategic management scholars. Particularly important to me were two conferences organized by assistant professors in the Strategy Group at Wharton held in the late 1980s. The Wharton faculty invited only assistant professors in strategy to these conferences. They were held in huge mansions on the Jersey Shore, due east of Philadelphia. No papers were presented. Instead, faculty were asked to facilitate discussions around important questions of the day.

Many of the professors who later became major actors in the development of resource-based theory attended these conferences: Raffi Amit, Colin Camerer, Kate Conner, Connie Helfat, Cynthia Montgomery, Margie Peteraf, and many others. We had time and space to talk about new ideas in new ways. We developed relationships that are important to this day. I still remember how frustrated Colin Camerer was when Kate Conner and I continued to collude in playing the board game *Risk*. He kept saying, "You are not behaving rationally," even as Kate and I ended up dominating the world. I also remember my beach walk with Raffi Amit. When we started the walk, he had only a limited familiarity with resource-based thinking; by the time we came back, he was primed to make some seminal contributions to this theoretical approach (e.g., Amit & Schoemaker, 1993).

An event at the 1990 Academy of Management conference also was important. A panel was organized at that conference on resource-based theory that included Raffi Amit, Garth Saloner, Cynthia Montgomery, David Teece, Margie Peteraf, and myself. It was in a very large ballroom in a San Francisco hotel, and the place was packed—standing room only. The energy in the room was palpable. People seemed deeply interested in moving beyond the Porter framework, and this newfangled "resource-based view" seemed to hold significant promise.

The *JOM* Special Issue

Given these developments, I was fairly confident that a special issue on resource-based theory would attract a fair number of high-quality manuscripts, and I issued the call for the *JOM* issue to which Ricky Griffin had agreed. In the end, around 20 papers were submitted. A couple of papers were actually on resource-dependence theory, and they were shifted over to the regular review process. The rest of the papers, including my paper now titled "Resources and Sustained Competitive Advantage," were put under review. Of course, I asked some of my colleagues at Texas A&M to take the lead in managing the review of my paper.

Since that time, I have been associated with editing several special issues at several different journals. Rarely have I seen a set of submissions as insightful and provocative as these papers. While reviewers had many challenging comments and suggestions, to me they were simply polishing what were already precious gems. Several of these papers have gone on to have important legacies.

For example, Kate Conner's (1991) paper discussed the entire evolution of industrial organization economics and showed how resource-based logic represented a new way of thinking about the theory of the firm. I still assign this paper to PhD students who need to get an overview of industrial organization economics. Connie Helfat's paper with Richard Castinias (Castinias & Helfat, 1991) showed that if employees were thought of as resources, then their ability to help create and share in firm profits would largely eliminate agency problems. This paper had a major influence on my most recent work introducing stakeholders into resource-based theory (Barney, 2018). My paper (Barney, 1991) introduced the "valuable, rare, inimitable, and nonsubstitutable" conditions that resources and capabilities must have to generate a sustained competitive advantage and argued that resources and capabilities will be costly to imitate when they are socially complex, path dependent, or causally ambiguous.[21]

Over time, Barney (1991) has often been cited as the seminal statement of resource-based theory. While this has been very gratifying, I personally believe that my statement of resource-based theory starts with "Strategic Factor Markets," continues with "Resources and Sustained Competitive Advantage," and culminates in my 2018 *SMJ* paper that shows that resource-based theory's models of profit generation and appropriation must logically adopt a stakeholder perspective (Barney, 2018).

Despite the enormous success of this special issue, I have two clear regrets, both having to do with papers that had been submitted to the special issue that were not published in the issue. First, Margie Peteraf submitted her paper, later published in *SMJ* (Peteraf, 1993), under the title “The Cornerstones of Competitive Advantage.” I clearly should have accepted that paper. Second, David Teece submitted a paper that later was published in *SMJ* (Teece, Pisano, & Shuen, 1997) with the title “Dynamic Capabilities and Strategic Management.”[22] That paper received a revise-and-resubmit request, but it was not resubmitted in time to get published in the special issue. I should have argued for more time to accept this paper as well. In short, as good as this special issue was, it could have been better.

In the end, the *JOM* special issue, while not as strong as it could have been, has nevertheless had a significant impact on the field of strategic management and, as this special issue demonstrates, on many other specialties in the field of management as well.

Discussion

Over the years since the publication of the *JOM* special issue, I have often been asked, typically by PhD students and assistant professors, how to write influential theory papers. Of course, I am reluctant to generalize too much from my own idiosyncratic experience. However, in conversations with other influential business scholars (e.g., Richard Rumelt, Bill Ouchi, David Teece, Oliver Williamson, Sid Winter, and many others) and from reading biographies of people who have been influential in their fields of expertise, such as those of Abraham Lincoln (Goodwin, 2005), Albert Einstein (Isaacson, 2008), Mother Teresa (Spink, 2011), Steve Jobs (Isaacson, 2011), Thomas Jefferson (Meacham, 2012), and many others, I have come to a few tentative conclusions.[23]

First, timing matters, and it is largely out of one’s control. Without Porter’s work that applied economic logic to the field of strategic management, it would have been more difficult to develop resource-based theory’s version of such an economic logic. Moreover, the domination of the field by the Porter framework set the stage for a reaction against that framework by addressing an issue, the relationship between firm-level resources and capabilities and firm performance, that even Professor Porter acknowledged the structure-conduct-performance paradigm was not well equipped to address (Porter, 1981).

However, while timing matters, it is not the only thing that matters. There were hundreds, if not thousands, of strategic management scholars who were working during the 1980s, and only a handful focused on firm-level sources of superior performance. Indeed, I remember one Academy of Management doctoral consortium during the 1980s where a faculty member asked how many of the PhD students attending the consortium were doing their dissertation on strategic groups, a concept that was an important part of the Porter framework (Barney & Hoskisson, 1990). Virtually every one of these students raised their hand.

I remember thinking to myself, “Students, it’s time to look for another topic besides strategic groups.” Given the theoretical opportunity to develop an alternative to the Porter framework in the 1980s, why did relatively few strategic management scholars take this opportunity?

First, as we are beginning to understand from work in the field of entrepreneurship (Alvarez & Barney, 2007), opportunities, including opportunities to develop new theory, do not exist independently, as objective phenomena, only waiting to be discovered by unusually alert individuals. Their existence can be known with certainty, ex post, only after they have been successfully exploited. Rather, they are created through an iterative process of trial and error, by individuals who end up building the opportunity they ultimately exploit. This was certainly the case with resource-based theory: Some of us thought that there might be a theoretical opportunity to study firm-level determinants of firm performance in the field, but that opportunity was created through an iterative process that involved a fair amount of trial and error. In my case, this process took over 7 years. In the end, the creation of a new theoretical perspective takes as much persistence as it does inspiration.

Second, the development of resource-based theory was not the only worthwhile theory opportunity to create in the field of strategic management in the 1980s. In many ways, this was the "golden era" of theory development in the field. Transactions cost theory, agency theory, incomplete contract theory, top echelon theory, real options theory, strategic dynamics theory, evolutionary theory, and many other theories were developed and applied to strategic management issues during this period. Resource-based theory hardly held a monopoly on interesting theoretical questions during this decade.

Third, because resource-based theory was in the process of being created, it was not at all clear through much of the 1980s that it would actually emerge as a major theoretical force in the field. Reasonable people disagreed about its theoretical and empirical potential. They still do.

In the end, it seems to me that there are two approaches for choosing what kind of research to do. First, you can carefully analyze the literature, identify a gap in that literature, explain why it is an important gap, and then fill it. Alternatively, you can examine your own interests, preferences, and capabilities, then choose a topic that you are particularly well suited to study. The first approach is analogous, in many ways, to the "industry picking" approach to strategy derived from the Porter framework: Find yourself an "attractive" topic and then exploit it. The second, not surprisingly, is analogous to the "inside to outside" approach of resource-based theory: Discover your unique strengths and gifts, and then choose a topic that takes full advantage of them.

My own sense is that the most influential work, theoretically or empirically, takes this second approach. As I stated in a book chapter published many years ago (Barney, 2005: 280),

> It has been said that all writing is autobiographical. If true, then one's research—because it is such an intense and focused form of writing—must be a particularly intimate form of autobiography. In this sense, all scholarship is self-revelatory. It is as if there is embedded, within the body of one's published work, a hidden Rorschach test that reveals more than even the author sometimes knows.
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> The most influential scholars, I think, embrace the self-revelatory nature of research. They understand that the "search for truth" is conditioned by our personal experiences, and that the definition of what constitutes an "interesting question" is only partly a matter of logic and epistemology. After all, from among all the "interesting questions" one could pose, why is a particular question asked?

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Notes

1. For example, over the past few months, both Xavier Castaner (in connection with the 2020 EURAM Conference) and Margie Peteraf (in connection with the 2021 Outstanding Scholarly Contribution Award in the Strategic Management Division of the Academy of Management, which Margie and I shared) have encouraged me

to write down this history. Also, both Rita Katila and Rich Makadok have recently encouraged me to summarize the history of the development of resource-based theory for students in their PhD seminars.

2. Though both these seminars were titled "The Social Psychology of Organizations," they could not have been more different. Professor Alderfer's course was taught in the School of Organization and Management (SOM), and Professor Kanter's course was colisted between the Sociology Department and SOM.

3. Later on, most of my work as a teaching assistant was with the MPPM course in statistics.

4. Bob Miles had moved from Yale SOM to Harvard Business School, so he could not be on my committee. We, however, have interacted many times over the years, and he was always a good friend and mentor. Professor Boorman continued as a member of my PhD committee until he was replaced by Professor Fritz Drasgow and Professor Woody Powell.

5. An interesting side note: The outside reader for my sociology exam was Professor Mark Granovetter.

6. Certainly, my dissertation, which applied blockmodeling techniques to the analysis of social network data collected by Coleman (1961), was mostly descriptive and largely atheoretical.

7. Of course, social network theory has evolved significantly from these early days. See, for example, Burt (1992); Gulati, Nohria, and Zaheer (2000); Gulati (1998); and Uzzi (1997).

8. I took my exams in the spring of 1978, just after Hannan and Freeman's (1977) paper on population ecology was published. Ironically, their paper was not on my reading list. When I finally read their paper, I was deeply impressed. I've always thought that I would have become a population ecologist if I had read Hannan and Freeman's paper for my qualifying exams.

9. A library is a large building where they used to keep books.

10. I later learned that I was not alone in this response to Professor Williamson's sometimes turgid prose.

11. I have never taken a formal class in economics, although ironically, I have now taught several of them.

12. In these early days, I taught an MBA course on organization theory. My credibility as a teacher—when I started at University of California–Los Angeles (UCLA), I was only 26 years old, well less than the average age of my students—was helped enormously by opportunities to consult with Bill Ouchi. Bill had just published the first best-selling book by a business school professor (*Theory Z*; Ouchi, 1982), and he graciously brought me along on his many consulting visits—where I learned how modern corporations actually worked.

13. A point made brilliantly by Don Hambrick in a session in the Academy of Management in 2018.

14. Only an intellectually arrogant young assistant professor would title a paper like this.

15. Except by Ed Zajac, who sent me a kind note suggesting that my ideas about strategic management in that book were very interesting.

16. I was scheduled to go up for tenure at UCLA in the late fall of 1986. When I had to make the decision to leave UCLA for A&M (the spring of 1986), I did not think I would have the record I needed for promotion at UCLA. It was difficult for me to leave UCLA, because many of the faculty there—especially Bill Ouchi, Dick Rumelt, and Bill McKelvey—had been instrumental in my intellectual development. Of course, it turned out that A&M was an equally fertile pasture for me.

17. It was fun to attend this Academy of Management session—three papers demonstrated that there was a correlation between market share and firm performance, and Rumelt and Wensley's (1981) paper demonstrated that this correlation must be specious.

18. This conference was held at the University of Southern California in 1983. Despite being on the faculty at UCLA (only about 15 miles away), I was not invited to attend. This was because in 1983, I was still very much an organization theory scholar who knew almost nothing about strategic management.

19. This is just a sample of the resource-based work that was being published during those years. A more complete review can be found in Barney and Arikan (2001).

20. At the time this essay was written, these three articles had, respectively, 5,000, 1,500, and 78,000 citations.

21. Professor Teece had been invited to give a presentation to the Economics Department at Texas A&M. I went to his presentation, and afterward we went to lunch—at Tom's Barbeque. At Tom's, they served only barbeque. It was served on a sheet of butcher paper along with a slab of cheese, an onion, and a butcher knife to eat it all. Over lunch, Professor Teece discussed some of his ideas, and I encouraged him to write a paper for the *Journal of Management* special issue. Those ideas later formed the basis of the dynamic-capabilities paper that was published in *SMJ*.

22. I am fully aware that reading biographies to understand how people become influential in their field of expertise is a classic example of "selecting on the dependent variable." There may be many men and women in

history who engaged in exactly the same behaviors as those people whose lives have been chronicled by biographers and yet did not become sufficiently influential in their field of expertise to warrant biographical treatment.

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