

Strategy Reader

Strategy Research Initiative



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Prologue

The Strategy Research Initiative began as an informal gathering of mid-career strategy scholars in the hinterlands of Nova Scotia in 2007. The goal of the gathering was to create an organization of like-minded colleagues dedicated to the creation of institutions designed to advance research in strategy. Since that initial gathering, the group has expanded, met annually to discuss our progress, and pushed forward on projects aiming to achieve that original goal.

At its core, the Strategy Research Initiative is intended to be an action-oriented community -- we seek to build institutions and disseminate resources that help strategy researchers raise the bar of scholarship. The launch of our web site (<http://strategyresearch.net>) in 2009 represents the completion of two of our initiatives: the Strategy Reader and the Empirical Primer, both of which are also available on our web site. These were identified as "low-hanging fruit" -- useful undertakings that could be up-and-running in a short period of time. Longer-term (and more ambitious) projects are in the works.

The Strategy Reader is a commentary and listing of seminal articles in various domains within the field of strategy. It represents the contributors' collective viewpoint on what represents high quality research in strategy. Our hope is that the Strategy Reader will be a valuable resource for doctoral students in strategy and related fields. In the commentaries that follow the reading lists, you will find detailed discussion of the most salient challenges facing strategy researchers and of some successful approaches for addressing those challenges. Reflecting the diversity of the strategy field, the commentaries identify quite different challenges across domains.

Each of the reading lists and commentaries included in this reader is designed as a stand-alone resource on an important research topic within strategy. There are nonetheless many areas of intersection among the lists, and opportunities for combining these resources in interesting ways. The following brief summary of topics highlights linkages among the lists based on the ordering of the lists in the table of contents. We hope that this provides a useful tool for navigating the reader content, but stress that this represents only one of many ways in which the reading lists can be organized and we invite each user to digest and recombine the content however best suits their needs.

Introduction

Contributed by Joanne Oxley, Jan Rivkin, Michael Ryall

The strategy field encompasses a diverse set of research perspectives and topic areas. This diversity is a strength of the field, but it also poses a challenge to scholars in strategy: It is often unclear how various literatures relate and why certain papers are considered exemplars of high-quality strategy research. In 2008 and 2009, we – a group of research-oriented strategy faculty on the cusp of tenure – set out to help sort through these issues. Specifically, we aimed to produce two types of output. First, each faculty member generated a list of core readings in an area in which he or she had expertise. These lists were shared, critiqued, and refined. Second, through discussing the lists, the group came to, and articulated, a collective viewpoint on what represents high quality research in strategy.

The strategy reader consists of these two types of output, in reverse order. It begins with a statement that lays out the attributes of high quality strategy research. Though the contributors to the reader came from very different domains within the strategy field, conversations among the contributors produced a high level of convergence and agreement about characteristics of high quality research that cut across domains.

The reader then provides reading lists on a diverse set of topic areas within strategy. Each reading list identifies a set of 5-7 “core readings” that the contributor believes are essential reading for all strategy scholars. A longer list of 20-30 supplementary readings, grouped under sub-headings, provides more depth for the interested reader. Finally, each list offers a short critical commentary that describes how the research area has developed and how the core readings have contributed to the area’s development. The commentary also highlights readings that exemplify particular attributes of high quality research.

In creating this strategy reader, we made no attempt to cover all topic areas within strategy. Rather, we capitalized on individual contributors’ areas of expertise and experience to create a collection of reading lists and critical commentaries on a subset of important research topics within strategy. Commentaries are grouped into four main areas. We have included very brief summaries of each topic area: Performance , Industry Dynamics , External Influences , and Internal Organization & Fit . Each summary lays out a roadmap of the area's contents and highlights a few of the most salient connections among the topic areas.

Our primary target for this reader is the community of doctoral students in strategy. We hope, however, that others may find the reader to be interesting and valuable. The reading lists may be helpful starting points for faculty constructing syllabi for doctoral seminars, for strategy scholars who want to get up to speed on a new area within strategy, and for scholars from other disciplines who hope to get a sense of the strategy literature in particular areas. The most up-to-date version of the Strategy Reader can be found on our web site at: <http://strategyresearch.net>.

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I. High Quality Research in Strategy

The faculty who constructed this doctoral reader developed a consensus about the characteristics of high quality research in strategy. Here, we lay out those characteristics. The commentaries of individual reading lists describe how the characteristics apply to specific topic areas. We share these characteristics in hopes that doctoral students and other strategy scholars will aim for them in their own work.

1. High quality theory in strategy is unambiguous and rigorously derived, is true to the essentials of reality, produces refutable, measurable claims, and is often interdisciplinary. To date, formal methods have been underutilized in the development of strategy theory.

A fundamental aim of strategy research is to increase understanding of the core drivers of organizational performance, broadly construed. This requires that we develop a body of theoretical claims that identify valid connections between performance and its precedents. Such claims should be unambiguous, rigorously derived, consistent with the core drivers of the phenomenon under investigation, and measurable.

A theoretical claim is unambiguous when interpretation of its terms, premises, and conclusions do not vary from individual to individual. Such precision is necessary for progress for several reasons. Theorists must understand one another unambiguously in order to build on each other's work. Empiricists must understand and agree upon what is being claimed in order to test a theory's external validity. From a normative standpoint, imprecise theory is clearly no basis for consistent business practice.

A theoretical claim is rigorously derived when its conclusions do, in fact, follow from its stated premises. Rigorous derivation helps us assess the internal validity of a claim by allowing others to verify the claim independently. Moreover, theoretical rigor lays the foundation for a stream of literature rich in extensions and refinements as the "technology" of theory-generation advances (e.g., when new methods permit the generalization of earlier work).

Even before independent verification, theories are invariably subject to an important face validity check to ensure that theoretical claims are consistent with the core drivers of the phenomenon under investigation. Significant discrepancies may lead a theory to be rejected upon inspection. Note, however, that "core drivers" need not – indeed, should not – encompass

every aspect of reality. Simple theories that suppress many facets of the real world may well pass this test.¹

High quality theory also connects to reality in a second sense: The constructs in a theoretical claim must be measurable if the claim is to be tested, and potentially refuted, empirically. This is especially relevant in a field such as strategy which is centered on a phenomenon. To validate the connections between performance and its precedents that a theory predicts, we must be able to measure performance and those precedents.

Finally, phenomena in strategy, by their nature, touch on the domains of multiple disciplines such as economics, sociology, and psychology. An organization's pursuit of a strategy typically is animated by the pursuit of economic gain, requires the coordinated efforts of social groups, and requires decision making in the face of complexity beyond the psychological bounds of individual managers. Consequently, many of the best theory-building efforts in strategy are interdisciplinary. High quality interdisciplinary research brings disciplines together in ways that make the combination more than the sum of the parts.²

Theoretical work in strategy has relied on two classes of methods: natural-language arguments and more formal methods (e.g., mathematical analysis, high-level logic, simulations). Traditionally, natural-language arguments have been far more common than formal methods. This parallels the norm in management scholarship more broadly.³ It is useful to reflect on the ability of each class of methods to produce claims that display the characteristics described above.

A natural-language argument, in the hands of a skilled practitioner, is a powerful tool. The strengths of this method are its accessibility and its

¹ For example, Akerlof's (1970) Nobel prize-winning paper, "The Market for Lemons," contains a highly stylized (unrealistic) model of markets. Even so, most would agree that it successfully captures several key implications of asymmetric information between buyers and sellers.

² A relevant example is Henderson and Clark's (1990) classic paper on architectural innovation: there, notions from information economics, organizational theory, and product design combine to produce an explanation for the failure of established firms that no single discipline could produce. In contrast, weak theory efforts merge ideas from multiple disciplines by simply tacking them together—sometimes without reconciling the very different premises that motivate the underlying disciplines. Mongrel theory papers are rarely of high quality.

³ For example, Adner et al. (2009) find that the *Academy of Management Review* published only one article that actually used a formal approach in the period from 1998 to 2007.

flexibility. Verbally derived claims are ideal for injecting major new ideas into theoretical discourse. Because its treatment of premises, definitions, and conclusions is very unconstrained, verbal theory can point scholars in broad new directions without resolving all the details.⁴

The most obvious drawback of natural-language theory is its inherent ambiguity. Words have multiple meanings, and these meanings can change over time as language evolves. This ambiguity has led at times to persistent confusion over key ideas in strategy.⁵ Rigorous derivation is also an issue for natural-language theory. Verbal argumentation has no built-in mechanism to force a theorist to characterize, clearly and completely, the premises required to support his or her conclusions. Moreover, strategy scholars study phenomena so complex that it is difficult to maintain logical consistency around them with verbal arguments. The danger then of reliance on natural-language theory is that unstated assumptions and hidden inconsistencies in logic are difficult to ferret out, resulting in the potential for false claims to diffuse widely and to endure.⁶

In contrast, formal methods—mathematical analysis, high-level logic, and simulations—are designed to generate theory that is unambiguous and rigorously derived. Each of these approaches forces the theorist to (i) provide a complete and precise statement of terms, premises, and conclusions; and (ii) demonstrate the logical consistency among these items. These are compelling features and, on balance, the contributors to this volume feel that formal methods should play a significantly larger role in the development of theory in strategy.

Formal methods, however, have genuine limitations that may prevent them from making valid connections between firm performance and its precedents. Each formal method has specific requirements regarding the form and scope of the premises it allows. For example, certain game

⁴ Economics, for example, has a rich tradition of verbal claims that significantly changed the trajectory of its theoretical discourse. The work of Smith, Coase, and Williamson are but a few well known examples.

⁵ Postrel (2004), for example, devotes an entire paper to sorting through the many meanings of the term “competitive advantage.”

⁶ This process is often reinforced by poorly-crafted theory papers that rely on irrefutable tautology. For instance, a paper might define a capability as that which drives firm performance and then hypothesize that high-performing firms possess superior capabilities. Thus, it is not whether a theoretical claim is ultimately *refuted* that determines its quality, but whether it is *refutable*. A piece of theory may be judged to be high-quality because it provides important clarification of previously murky concepts, even though it is later refuted by empirical analysis.

theoretic analyses require the theorist to specify a game tree, identify agent payoffs for various outcomes, and make assumptions about players' beliefs regarding each other's strategy choices. This specificity is good in that it enforces clarity and rigor. A problem arises, however, when the requisite form and scope of the premises is not well-aligned with what scholars believe to be core drivers of the phenomenon under examination. This can lead to theories that lack face validity.⁷ A common challenge for all theorists is to craft theory that reflects the rich reality of strategy yet simplifies enough to be unambiguous and rigorous.

In sum, we urge scholars to ask the following questions when they pursue theoretical research in strategy:

- Are their theoretical claims unambiguous?
- Can their conclusions be derived rigorously from explicit premises?
- Do their premises sufficiently capture the core drivers of the phenomena they study?
- Have their theories produced propositions with measurable implications that can be refuted empirically?
- Do their theories merge ideas from multiple disciplines in ways that produce something greater than the simple sum of the discipline-based ideas?

2. High quality empirical research in strategy may either report empirical regularities or aim to test theory. In both cases, high quality research is transparent in data presentation and analysis. In the case of theory-testing research, high quality work uses data that conform closely to the theory to be tested as well as empirical designs and methods that generate valid inferences.

Empirical analysis continues to be the mainstay of the strategy field. This reflects both the field's origins in the practice of strategic management and its status as an applied social science field. We see two broad, appropriate uses of empirical analysis in strategy research.

⁷ For example, many strategy scholars consider the "rational expectations" assumption required in some economic models to be grossly inconsistent with the knowledge of real-world managers and, more importantly, inconsistent in a way that misses the essence of strategic decision making. Using such a model would unambiguously identify the rational expectations assumption and rigorously demonstrate its implications but, having heard this assumption, many would reject the validity of its implications on its face.

The first role of empirical analysis is to identify interesting facts related to organizational performance and its precedents—in other words, to find and describe the phenomena that need to be explained by strategy theory. This may seem like an obvious and important role, but in practice, it is remarkably rare to see strategy research papers that simply report empirical regularities. This gap reflects an apparent – and we believe, perverse – requirement that all submissions to strategy and management journals contribute to theory. In a recent eloquent and impassioned editorial, Don Hambrick bemoans this *theory fetish* that effectively “bans the reporting of facts – no matter how important or competently generated – that lack explanation but that, once reported, might stimulate the search for an explanation” (Hambrick, 2007: 1346).

Hambrick goes on to enumerate a variety of costs imposed on the field by this deification of theory, which is arguably absent from other disciplines or applied fields. A particularly salient cost to note here is the creation of incentives to contort data to conform to theoretical predictions. The most effective way to counter this tendency – and to place empirical analysis above suspicion on this front – is to be as complete and transparent as possible in displaying empirical data, presenting rich sample information, full descriptive statistics, etc. Transparency in data presentation and analysis is therefore an important hallmark of high quality empirical research in strategy.

Another important cost of a theory fetish is the creation of incentives to contort theoretical predictions to conform to data. Scholars are sorely tempted to analyze their data, find patterns, and then construct theoretical arguments that “predict” those patterns with eerie accuracy. Loose verbal theorizing is flexible enough to provide a rationale for virtually any pattern one observes. The best protections against this form of low quality research are the characteristics of good theoretical research laid out in Section 1 above.

A second role for empirical research in strategy is to test the propositions that theories generate. A strong link between theory and empirical analysis is a fundamental prerequisite for generating useful normative guidance for managers: Normative statements derived from empirical analysis are not meaningful (and indeed may be quite misleading) if not grounded in theory. Conversely, elegant theory is unlikely to have a positive impact on practice unless it is tested and validated by empirical research.

High quality efforts to test theory require reliable data that conform closely to the theoretical constructs being tested. That one should strive for congruence between theory and measures may seem obvious to the point of being trite, but our reading of empirical research in strategy suggests that many studies fall short of this ideal. A common problem is a mismatch between the unit of analysis in the theoretical model and the level of aggregation of the data used in the study. For example, firm-level data may be (mis)used to test a theory that focuses on interactions among individual agents or project teams. Such a mismatch may obscure significant empirical regularities or generate spurious inferences. A second common problem is to choose as an empirical proxy for a theoretical construct some measure that others can interpret as a proxy for a very different construct. When proxies allow different interpretations, empirical studies cannot distinguish between alternative theories. Because it is often impossible to identify perfect proxies, strategy scholars must be sensitive to alternative explanations for empirical regularities.

Theory-testing efforts should also employ empirical designs and methods that generate valid inferences that support or refute the stated hypotheses. Empirical designs and methods can be flawed in countless ways, but one problem is pervasive in strategy research: a failure to account adequately for the inherent endogeneity of strategic action. Almost all strategy theories assume that managers selectively choose strategic actions based on expected performance outcomes. This selection implies that firms whose managers choose different actions are likely different, often in ways the researcher can't observe. It is common in empirical research in strategy to observe differences in actions and in performance across firms and to conclude that the differences in action caused the differences in performance. This causal conclusion may be deeply flawed, however, if the researcher did not take care: It may be that underlying, unobserved differences across firms caused both the differences in actions and the differences in outcomes. Such mistakes can lead us to reject sound theories, support faulty theories, and offer bad advice to managers. Only careful empirical designs and methods can protect the strategy field from such mistakes.⁸

⁸ See, Hamilton & Nickerson (2003) and Bascle (2008) for discussion of potential sources of bias in strategy research, as well as suggested solutions. Note, however, that blind reliance on canned empirical 'fixes' to endogeneity problems to the exclusion of conceptual discussion can also be misleading, since choosing an empirical method often involves trading off different problems. For example, to correct for self selection one typically has to impose some fairly strict distributional assumptions, which themselves are often violated. Investigating and testing the validity of assumptions underlying different empirical models in particular settings is a crucial step.

In sum, scholars should ask themselves the following questions as they design and execute empirical studies in strategy; affirmative answers to these questions set high quality empirical papers apart from the rest.

- Are data presented and analyzed as transparently as possible?
- Does the research employ reliable data that conform closely to theoretical constructs?
 - In particular, do the level of analysis and the unit of observation in the study match those of the theory? Can the empirical proxies discriminate among alternative explanations?
- Are the empirical designs and methods chosen carefully to generate valid inferences?
 - In particular, have concerns about endogeneity and selection been addressed adequately?

3. High quality research in strategy appreciates and builds on prior high quality work in the field.

A number of forces conspire to discourage strategy researchers from reading (not just citing!) prior work and building on it. An intense focus on the business environment facing our MBA students today keeps our attention on current trends and distracts us from the broad sweep of history. The temptation of striving to be the next great strategy guru encourages us to emphasize how our ideas are new and different, not how they are related to what came before. A proliferation of new research papers makes it challenging to keep up with current findings, let alone to read and savour the classics.

Researchers who do manage to appreciate and build on prior work improve the quality of their research in several ways. They avoid dead ends that others explored. They benefit from the logic, language, and findings that others generated. They uncover threads that can be fruitfully taken up again as methodological advances make them more tractable. They discover the deep ideas that made the classics classic. Perhaps most important, they allow the field to progress in a cumulative and steady fashion, not in fits and starts.

Accordingly, we urge scholars to ask of their research:

- Does the research sufficiently incorporate the insights of prior high quality research in strategy?

We hope that the reading lists that follow encourage an affirmative answer to this question, in part by pointing students toward many examples of prior high quality research. Not every reading on every list is a perfect exemplar of high quality research, and the lists' commentaries highlight some of the shortcomings of the readings. Many of the readings that fall short are classics that were examples of high quality research *in their time*. Quality standards in strategy research have risen over time, and we hope that they will continue to do so.

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II. Performance

A fundamental aim of strategy research is to increase understanding of the core drivers of organizational performance, broadly construed. The reading lists in the first section of the reader surveys some of the basic building blocks of organizational performance and highlights some continuing debates regarding the source and sustainability of competitive advantage.

In ***Competition and Performance*** Michael Ryall traces the development of ideas in strategy about competition and its effect on firm performance. This reading list highlights recent theoretical contributions in coalitional game theory that expand and refine the traditional conception of competition that underpins many central ideas in strategy. These recent developments provide a precise formal apparatus with which to analyze how strategic decisions at the firm level alter the balance of competitive and super-competitive factors that affect value appropriation and performance.

Peter Zemsky's reading list on ***Sustainability*** reflects on some of the theoretical mechanisms underlying sustainability of competitive advantage, focusing in particular on the threat posed by imitation by rivals. Two broad views on sustainability are presented: the first, coming from the traditional IO literature, explores how mechanisms as diverse as switching costs, learning curves, network externalities and reputations create positive feedback such that a competitive advantage today reinforces itself and hence persists over time. As such, there are clear links between this topic area and topics covered in the lists on ***Innovation, Industry Evolution, Geography and Agglomeration***, and ***Organizational Learning***. The second view on sustainability shifts attention away from the maintenance of privileged product market positions to superior **resource** positions. In surveying and critiquing this literature, and highlighting recent developments, the readings here complement those in ***Competition and Performance*** and in ***Industry and Firm Effects***.

In ***Industry and Firm Effects*** Anita McGahan outlines the contours of a long-standing debate in strategy on the relative importance of industry, corporate, business-unit and year effects on the accounting profitability of firms. Reflecting the significant empirical challenges in this area, this list traces methodological developments aimed at identifying different sources of persistence in performance, and highlights areas of consensus and continued controversy. Recent innovations and extensions that emphasize the role of geography and individuals in shaping institutions that create

enduring industry effects are also explored; these recent developments provide links to topics covered in ***Geography and Agglomeration***.

Nick Argyres and Rachelle Sampson explore another fundamental building block of performance in ***Organization Form: Selection & Consequences***. They note that the selection of organizational form and its consequences for firm performance were arguably the very first phenomena to be studied in the strategy field. The list highlights development of the literature, based primarily on organizational economics, but also encompassing challenges to the economic approach from a sociological perspective, as well as readings that seek to reconcile the two approaches. Recent contributions in the areas of inter-firm alliances, internal organization and contract structure are also surveyed, including empirical studies designed to tackle the difficult issues associated with self-selection and endogeneity bias endemic to this area of research.

In the final reading list in this section, Peter Roberts focuses on some important intangible drivers of firm performance in ***Antecedents and Performance Implications of Reputation and Status***. The reading list reflects the broad disciplinary diversity of relevant literature and the commentary discusses both opportunities and challenges faced by researchers in this inter-disciplinary domain. Careful definition of terms and specification of causal mechanisms is highlighted as a crucial issue as we seek to understand the boundary conditions for presumed positive reputation/status-performance relationships.

Competition and Performance

Contributed by Michael Ryall

Core Readings

- Makowski, L., J.M. Ostroy. 2001. Perfect competition and the creativity of the market. *Journal of Economic Literature* 39(2) 479–535
- Porter, M.E. 1979. How competitive forces shape strategy. *Harvard Business Review* 57(2) 137–145
- Wernerfelt, B. 1984. A resource-based view of the firm. *Strategic Management Journal* 5(2) 171–180
- Brandenburger, A.M., H.W. Stuart. 1996. Value-based business strategy. *Journal of Economics & Management Strategy* 5(1) 5–24
- MacDonald, G., M.D. Ryall. 2004. How do value creation and competition determine whether a firm appropriates value? *Management Science* 50(10) 1319–1333
- Brandenburger, A., H. Stuart. 2007. Biform games. *Management Science* 53(4) 537–549

Supplemental Readings

Economists in Their Own Words

- Walras, L. 1874. *Elements of Pure Economics*. W. Jaffé trans. London: Allen and Unwin, 1954 (Original French ed. pub. 1874)
- Jevons, W.S. 1879. *The theory of political economy*. 5th ed. New York: Sentry Press
- Menger, C. 1871. *Principles of Economics*. J. Dingwall and B. F. Hoselitz trans. F. A. Hayek intro. Grove City: Libertarian Press, Inc., 1994 (original Austrian ed. pub. 1871)
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- Debreu, G. 1959. *Theory of Value: An Axiomatic Analysis of Economic Equilibrium*. New York: John Wiley and Sons, Inc
- Makowski, L., J.M. Ostroy. 1995. Appropriation and efficiency: A revision of the first theorem of welfare economics. *American Economic Review* 85(4) 808–27.

More from the Strategy Mainstream

- Barney, J. 1991. Firm resources and sustained competitive advantage. *Journal of Management* 17(1) 99
- Collis, D.J., C.A. Montgomery. 1997. *Corporate strategy: resources and the scope of the firm*. Chicago: Irwin
- Saloner, G., A. Shepard, J.M. Podolny. 2000. *Strategic management*. New York: John Wiley and Sons, Inc
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Additional Strategy Applications of CGT

- Brandenburger, A., B. Nalebuff. 1996. *Co-opetition*. Doubleday New York
- Gans, Joshua, Glenn MacDonald, Michael Ryall. 2008. The two sides of competition and their implications for strategy. Working Paper
- Ryall, M.D., O. Sorenson. 2007. Brokers and competitive advantage. *Management Science* 53(4) 566
- Chatain, O., P. Zemsky. 2007. The horizontal scope of the firm: Organizational tradeoffs vs. buyer-supplier relationships. *Management Science* 53(4) 550
- Adegbesan, J. 2008. On the origins of competitive advantage: Strategic factor markets and heterogeneous resource complementarity. Forthcoming, *Academy of Management Review*

CGT-Related Technical

- Myerson, R.B. 1991. *Game Theory: Analysis of Conflict*. Cambridge: Harvard University Press
- Stuart Jr, H.W. 2005. Biform analysis of inventory competition [j]. *Manufacturing and Service Operation Management* 7(4) 347–359
- Stuart, J. 1997. The supplier-firm-buyer game and its m-sided generalization. *Mathematical Social Sciences* 34(1) 21–27
- Bondareva, O.N. 1962. Theory of the core in the n-person game. *Vestnik* ;
- Shapley, L.S. 1965. On balanced sets and cores. DTIC Research Report AD0617079

Commentary

In this section, we trace the development of ideas in strategy about competition and its effect on firm performance. Perhaps not too surprisingly, strategy inherits much of its thinking about the workings of competition from economics. Our purpose is, first, to explore the traditional conceptualization of competition that underpins several central ideas in strategy and, then, to highlight recent work that aims to provide a more precise theoretical foundation for analyzing these issues.

The thesis developed here is that many of strategy's principal claims are built upon a notion of competition that is implicitly rooted in the ideas of the original neoclassical economists (Jevons, 1879; Menger, 1871; Walras, 1874). Paradoxically, even as much of the work in strategy seeks to distance itself from the bogeyman of neoclassical economics, it yet proceeds to incorporate the essential logic of that paradigm in its analysis of firm performance under competition. This typically leads to an overly simplistic intuition of how competition works. Recent theoretical contributions based upon coalitional game theory expand and refine the traditional view.

Makowski and Ostroy (2001) reviews the history of economic thought with respect to "perfect" competition and, in particular, summarizes a rich, innovative line of work on this subject by these two authors (Ostroy, 1980; Makowski & Ostroy, 1987; Makowski & Ostroy, 1993). The starting point is the "Standard Model" (SM) of economics -- the one commonly found in microeconomic textbooks, featuring price-taking, market-taking, and free entry. The SM traces its origin directly to the founders of neoclassical economics, who viewed "perfect competition" as the inexorable outcome of a production economy -- an equilibrium state of zero profits induced by free entry. Alternatively, this paper updates the definition of "perfect competition" to be a situation in which the "added values" of a market's agents add up to the aggregate economic value produced in that market.⁹ Under this definition, it quickly follows that a firm's economic profit in a "perfectly competitive market" is equal to its individual marginal product. That is, competition at its most intense *guarantees* every actor economic profit equal to its added value. This is a complete inversion of the traditional intuition regarding the effect of competition on performance.

⁹ An agent's "added value" is the incremental amount of economic value that agent contributes, by virtue of her presence, to the aggregate economic value created.

Next, it is instructive to consider a founding paper from two of strategy's primary streams: Porter (1979) on industry positioning and (Wernerfelt, 1984) on the resource-based view. Common to each of these is the conclusion that firms enjoy economic profit only in the presence of barriers to competition.¹⁰ This conclusion is consistent with the neoclassical SM, with competition conceptualized as a persistent force of performance erosion.

In (Porter, 1979), the logic of the SM is explicit (p. 137): "In the economists' 'perfectly competitive' industry, jockeying for position is unbridled and entry to the industry very easy." This leads to the conclusion (p. 137): "The weaker the forces collectively, however, the greater the opportunity for superior performance." (Wernerfelt, 1984) extends this logic to firm resources (p. 172): "For purposes of analysis, Porter's five competitive forces will be used, although these were originally intended as tools for analysis of products only." Similarly (p. 172), "One can identify types of resources which can lead to high profits. In analogy to entry barriers, these are associated with what we will call resource position barriers." The thinking of the original neoclassicists is, thus, deeply embedded within both the industry positioning and resourced-based views.¹¹

Brandenburger and Stuart (1996) is the first (and, hence, ground-breaking) paper to advocate the use of cooperative game theory (CGT) in strategy. Although positioned (p. 5) as "following Porter (1980)," this paper represents a marked departure from mainstream thinking up to that point. In particular, the cases examined here meet Makowski and Ostroy (2001)'s definition of perfect competition. As mentioned above, this guarantees that every agent is a full-appropriator. This, in turn, implies the normative conclusion that firms maximize their value-added ("value-based" strategy). It is worth pointing out that the analysis stops short of using the full CGT formalism to derive mathematical propositions, opting instead to illustrate its ideas with specific numerical examples. Thus, the efficacy of value-

¹⁰ Although we include only one (canonical) contribution from each of these important streams, the competitive logic they employ is consistent with later work (Barney, 1991). The firm positioning literature examines mobility barriers between industry groups and the RBV resource mobility barriers between rivals.

¹¹ Note the remarkable correspondence between Wernerfelt and the work of Makowski & Ostroy. Wernerfelt opens with the assertion that, "For the firm, resources and products are two sides of the same coin." Makowski and Ostroy (1995) provide an explicit demonstration that the product-market and firm-resource views are, in a precise technical sense, dual to one another.

based strategies in other than perfectly competitive markets is left as an open theoretical question.¹²

MacDonald and Ryall (2004) focuses upon the “positive” side of competition, characterizing the conditions under which competition alone guarantees strictly positive economic profit (not necessarily an amount equal to its added value). This paper represents a technical advance in the sense that it uses a complete version of the CGT setup to derive formal propositions. The main result demonstrates that the existence of a particular tension between the aggregate value actually produced in an industry and the values of the alternative, arm’s-length transactions available to a specific firm is both necessary and sufficient to guarantee that firm appropriates strictly positive economic profit. In both positive and normative terms, this suggests that superior performance may be associated with resource and industry positions that intensify this tension.

Brandenburger and Stuart (2007) makes a number of significant theoretical advances. Two innovations are especially important: *i*) the concept of a “biform” game; and, *ii*) the introduction of an individual “confidence index.” A biform game is a hybrid in which agents take strategic actions (represented in an initial noncooperative stage) the interactive consequence of which is to change the joint competitive landscape (represented by a second, cooperative, stage). The individual confidence index summarizes managerial assessments regarding relative performance on super-competitive dimensions (e.g., bargaining skill). This allows one to associate a specific level of value appropriation with each agent’s competitive interval.¹³ Taken together, we now have a complete formal setup with which to analyze -- in very refined ways -- how strategic decisions at the firm level alter the balance of competitive and super-competitive factors that affect performance.

In summary, many of strategy’s early conjectures followed from the view that, sans effective barriers, competitors fly into a market like a hail of bullets and drive economic profit to zero. This is consistent with the SM of the early neoclassicists. Recent applications of CGT demonstrate that competition is an inherently neutral force, with both positive and negative effects. This distinction matters. When unbridled competition implies full appropriation, superior performance is associated strategies that *lower* barriers and raise added value. When the negative effect of competition dominates, ideas in the earlier mainstream literature rest on a much firmer

¹² The answer to which is no; see Gans et al. (2008).

¹³ Typically, agents face a nontrivial *range* of payoffs that is consistent with competition.

foundation. Typically, economic performance is determined by a combination of competitive and super-competitive factors. Presently, the interplay between these determinants and the types of policy levers widely explored in the extant literature (e.g., resource deployments) remains largely unexplored.

Sustainability of Competitive Advantage

Contributed by Peter Zemsky

Core Readings

- Barney, J. B. (1991). "Firm Resources and Sustained Competitive Advantage," *Journal of Management* **17** pp. 99-120.
- Katz, M. and C. Shapiro (1994). "Systems Competition and Network Effects," *Journal of Economic Perspectives* **8** 93-115.
- Lenox, M., S. Rockart and A. Lewin (2006). "Interdependency, Competition, and the Distribution of Firm and Industry Profits," *Management Science* **52**, pp. 757-772.
- Levinthal, D. (1997). "Adaptation on Rugged Landscapes," *Management Science* **43**, pp. 934-950.
- Lippman, S. A. and Rumelt, R. P. (1982). "Uncertain Imitability: An Analysis of Inter-firm Differences in Efficiency under Competition," *Bell Journal of Economics* **13(3)** pp. 418-438.
- Pacheco-de-Almeida, G. and Zemsky, P. (2007). "The Timing of Resource Development and Sustainable Competitive Advantage." *Management Science* **53** 651-666.
- Priem, R. and J. Butler (2001). "Is the Resource-Based 'View' a Useful Perspective for Strategic Management Research?" *Academy of Management Review* **26** 22-40.
- Rivkin, J. W. (2000). "Imitation of Complex Strategies," *Management Science* **46** 824-844.
- Sutton J. (1991). *Sunk Costs and Market Structure: Price Competition, Advertising and the Evolution of Concentration*, MIT Press.

Secondary Readings

- Adner, R. and P. Zemsky (2006). "A Demand Based Perspective on Sustainable Competitive Advantage." *Strategic Management Journal* **27**, pp. 215-239.
- Barney, J. B. (1986). "Strategic Factor Markets: Expectations, Luck, and Business Strategy." *Management Science* **32**, pp. 1231-1241.
- Barney, J. B. (2001). "Is the Resource-Based 'View' a Useful Perspective for Strategic Management Research? Yes." *Academy of Management Review* **26**, pp. 41-56.

- Besanko, D., Dranove, D., Shanley, M., and Schaefer, S. (2007). *Economics of Strategy* John Wiley & Sons, Inc.
- Dierickx, I. and K. Cool (1989). "Asset Stock Accumulation and Sustainability of Competitive Advantage." *Management Science* **35**, pp. 1504-1511.
- Katz, M. and C. Shapiro (1992). "Product Introduction with Network Externalities," *Journal of Industrial Economics* **40**, pp. 55-84.
- Makadok, R. and J. B. Barney (2001). "Strategic Factor Market Intelligence: An Application of Information Economics to Strategy Formulation and Competitor Intelligence" *Management Science* **47**, pp. 1621-1638.
- Makadok, R. (2001). "Toward a Synthesis of the Resource-Based and Dynamic-Capability Views of Rent Creation," *Strategic Management Journal* **22**, pp. 387-401.
- Nelson, R. R., and Winter, S. G. (1982). *An Evolutionary Theory of Economic Change* Cambridge, Mass.: Belknap Press of Harvard University Press.
- Pacheco-de-Almeida, G. and Zemsky, P. (2008). "Time-Consuming Technology Development: How Imitation and Spillovers Affect Competitive Dynamics," NYU Stern School of Business Research Paper.
- Porter, M.E. (1980). *Competitive Strategy*, Free Press, New York, 1980.
- Porter, M.E. (1985). *Competitive Advantage*, Free Press, New York, 1985.
- Priem, R. L. and J.E. Butler (2001). "Tautology in the Resource-Based View and the Implications of Externally Determined Resource Value: Further Comments," *The Academy of Management Review* **26**, pp. 57-66.
- Reed, R. and R. DeFillippi, J. 1990. "Causal Ambiguity, Barriers to Imitation, and Sustainable Competitive Advantage." *Academy of Management Review* **15**, pp. 88-102.
- Rochet, J. C. and J. Tirole 2006. "Two-Sided Markets: A Progress Report." *RAND Journal of Economics* **37**, pp. 645-667.
- Shapiro, C and H. Varian 1999. "Networks and Positive Feedback" in *Information Rules: A Strategic Guide to the Network Economy* Harvard Business School Press.
- Sutton J. 1998. *Technology and Market Structure*. The MIT Press.
- Wernerfelt, B. (1984). "A Resource-Based View of the Firm," *Strategic Management Journal* **5**, pp. 171-180.

Commentary

The **sustainability of competitive advantage** has come to have a central place within the strategy field. Sustainability is of interest in contexts where firm heterogeneity is allowing some firm to outperform rivals at some point in time and concerns the extent to which the underlying heterogeneity and the resulting performance differences endure over time. There are a variety of threats to sustainability including mismanagement of organizational strengths and external shifts in the market that undercut the rents from a firm's position. The focus in these readings, as in much of the strategy literature, is on the threats to sustainability coming from imitation by rivals and entrants that reduce or eliminate the heterogeneity that is the basis for the superior performance. We limit ourselves to the theoretical development of this topic. For closely related empirical work, see Anita McGahan's entry in this Reader.

The "invasion" of economic concepts from industrial organization into the strategy field in the 1980s, which was spurred by the work of Michael Porter (1980, 1985), gave rise to considerable creative tension in the field. The new industry-level of analysis and the emphasis of economic drivers of superior performance, threatened to shift attention away from the traditional concerns of the field with internal business policies. To a large extent, the literature on sustainability comes directly from attempts to push back against the IO perspective.

Lippman and Rumelt (1982), in the first formal theoretical paper inspired by the distinctive concerns of the strategy literature, demonstrate how superior performance can arise without assumptions of imperfect competition and market power, which are the defining features of the IO approach. In their model there are a large number of potential entrants that can pay a fixed cost to enter an industry. The key assumption is that there is *imperfect imitability* so that each entrant's cost function is determined by an independent draw from a known distribution. In equilibrium, firms with bad draws exit and the remaining firms on average must have abnormal returns even when in the case where the firms are all small and have no market power. *Ex ante* however expected profits from entry are zero. The paper remains an outstanding example of high quality theorizing in strategy. Barney (1986) in his paper on strategic factor markets applies the same reasoning in his verbal argument that from an economics perspective superior performance must be the result of luck.

The “resource-based” alternative to IO economics crystallizes in **Barney (1991)**, one of the most cited papers in strategy. The paper brings together a variety of ideas that were circulating in the field at that time, including the concept of barriers to imitation from Lippman and Rumelt (1982). Important too is the idea from the classic paper by Wernerfelt (1984) that firms can be viewed not just as a collection of product market positions as in an IO approach, but as a collection of underlying resources. Finally, there is an emphasis on sustainability, which was the focus in Dierickx and Cool (1989). Reacting to Barney’s (1986) paper on strategic factor markets. They point out that many firm resources are not purchased but must be built up over time *and that there are a variety of mechanisms that sustain asymmetries in resource stocks*. All of this comes together in the resource-based perspective where firms are endowed with rent earning resource bundles and to the extent that a firm has valuable, rare and inimitable resources then it will enjoy superior performance. The issue of *ex ante* creation of the resources from Lippman and Rumelt (1982) and Barney (1986) is dropped and as in Dierick and Cool (1989). The emphasis is shifted to analyzing the extent to which resource asymmetries can be sustained in the face of imitation.

While the resource-based view provided a powerful rallying point for strategy scholars, as a platform for strategy scholarship it became clear over the ensuing decade that the received theory was not living up to the original high expectations. **Priem and Butler (2001)** forcefully argue that the core argument in Barney suffers from the potential failing of verbal theorizing of being a tautology, with the definition of competitive advantage being too close to the possession of the valuable and rare resources. In addition, the resource-based view has suffered from taking the Wernerfelt (1984) argument too far and viewing firms as only bundles of resources and ignoring the fact that ultimately these resources generate rents by creating competitive advantages in actual product markets. There is a danger that such a one-sided view allows empirical researchers in this tradition to ignore important complexities and endogeneities that arise from the fact that performance data is almost always generated from competitive interactions in product markets.

Both Priem and Bulter (2001) and Barney (2001) in his response, suggest that more formal theory is a promising avenue for advancing resource-based research. Indeed, almost two decades after Lippman and Rumelt (1982) there is a return to formal work on resources and sustainability, starting with Makadok and Barney (2001), who formally study strategic

factor markets.¹⁴ **Pacheco-de-Almeida and Zemsky (2007)** study both the sustainability and the creation of competitive advantage. In their model, a leader firm first decides how fast to develop a resource and then a follower decides whether and how fast to imitate the resource. There are time compression diseconomies as in Dierckx and Cool (1989) so that the faster a firm develops the resource, the greater the cost. The paper formalizes two notions of sustainability: whether the follower has an economic incentive to develop the resource and, if it does, how long it takes it to neutralize the leader's advantage. The paper shows how competitive advantage need not be equivalent to superior performance: Although the leader enjoys a period of competitive advantage for at least some interval of time, the net present value of its profits flows may be lower once one takes into account the differential cost of resource development. Finally, although the strategy literature often suggests that barriers to imitation are good for leaders and bad for followers, here the follower can be hurt by an increase in spillovers that aid imitation because the resulting fast imitation demotivates the leader to develop the resource in the first place.¹⁵

Turning to the IO tradition, a useful counter weight to the focus on internal, hard to imitate resources coming out of the strategy literature is provided by the large body of theory on network externalities, as well summarized by **Katz and Shapiro (1994)**.¹⁶ When consumer willingness to pay is an increasing function of a firm's market share due to network externalities, initial advantages are easily amplified and sustained. It is interesting to note the extent to which the concerns of the IO literature on network externalities overlap with those in strategy. Predictions about the outcomes of competition between standards and the existence of winner-take-all corner solutions are highly relevant, while issues of social welfare and pricing are more peripheral. There has been a recent resurgence of this literature with recent work on "two-sided" markets such as ebay and other

¹⁴ Other recent formal work on resources are Makadok (2001), which considers the interaction between resource picking and capability building, and Adner and Zemsky (2006), which follows up on the proposal in Priem and Bulter (2001) to elucidate how the ability of resources to create value in product markets may shift over time.

¹⁵ Pacheco-de-Almeida and Zemsky (2008) extend the analysis to show that the leader may benefit from reducing barriers to imitation when this can cause a follower to shift from a strategy of developing the resource in parallel to the leader to a strategy of waiting and imitating.

¹⁶ A classic paper on network competition of interest to strategy scholars is Katz and Shapiro (1992). For a good overview of the phenomenon see Chapter 7 on networks and positive feedback in Shapiro and Varian (1999).

platforms. However, as the survey by Rochet and Tirole (2006) makes clear, the primary concern here is on pricing decisions and hence this work is of more relevance to marketing than to strategy.

Although the effect of positive feedback where success breeds success is especially powerful when there are strong network externalities that give rise to winner take all markets, these effects are in fact pervasive and operate in many markets with high concentration. **Sutton (1991)** does a brilliant job of rigorously exploring how the escalation of advertising expenses underlies some of the most sustainable positions, those in the consumer packaged goods industries. Especially inspiring is his integration of game theory modeling, large sample empirical work and detailed case studies. The key theoretical predication is that in advertising intensive markets, concentration is sustained above a minimal threshold even as market size becomes large. The case studies show how the theory developed using simple two-stage games gives rise to positive feedbacks that can play out over time in real markets. Sutton (1998) extends the analysis from advertising to R&D intensive settings.

All of the above literature approaches sustainability from a neoclassical economics perspective of rational optimizing actors. **Rivkin (2000)** studies imitation using firms that engage in local search among a set of business policies. The paper makes use of NK simulation, which were first brought to strategy in Leventhal (1997). In this tradition, firm heterogeneity arises and is sustained because firms find locally optimal policies and are assumed to stop experimentation. This simulation approach delivers on the original promise in Nelson and Winter (1982) to rigorously study how idiosyncratic organizational routines arise and evolve using simulations, as well as providing micro-foundations for the idea of uncertain imitability from Lippmann and Rumelt (1984). Like the RBV, most NK models suffer from abstracting away from product market competition, although see Lenox et al. (2006) for a notable exception. For more on fit and imitation see Nicolaj Siggelkow's entry in this Reader.

Although the tension between an IO perspective and a resource-based perspective has provided some impetus to the development of the strategy literature, research on the theory of sustainability has the potential to help better integrate the study of firm resources and product market competition.

Industry and Firm Effects on Performance

Contributed by Anita M McGahan¹⁷

Core Readings

The key questions in this literature relate to the relative importance of industry, corporate, business-unit and year effects on the accounting profitability of firms. The research has moved toward covering firms in areas of the world other than the United States. The methodological challenges relate mainly to the proper interpretation of the effects given the fact that they are mutually determined. The most fruitful areas for further research include (a) identifying persistent, sustainable fixed effects, (b) expanding geographic coverage, (c) identifying the drivers of the effects, (d) evaluating whether relationships arise in performance at different levels of analysis, i.e., whether good industries tend not to host high-performing corporations, etc. and (e) interpreting the implications for public and business policies.

1. Schmalensee, R. (1985). Do markets differ much? *American Economic Review*, 75: 341-351.
2. Rumelt, R. (1991). How much does industry matter? *Strategic Management Journal*, 12, pp. 167-185.
3. McGahan, A.M. & Porter, M.E. (1997). How much does industry matter, really? *Strategic Management Journal*, 18 (summer special issue): 15-30.
4. McGahan, A.M. & Porter, M.E. (2002). What do we know about variance in accounting profitability? *Management Science*, 48(7): 834-851.
5. Ruefli, T.W. & Wiggins, R.R. (2003). Industry, corporate, and segment effects and business performance: A non-parametric approach. *Strategic Management Journal*, 24: 861–879.
6. McGahan, A.M. & Porter, M.E. (2005). Comment On 'Industry, Corporate And Business-Segment Effects And Business Performance: A Non-Parametric Approach' By Ruefli And Wiggins. *Strategic Management Journal*, 26(9): 873-880.

¹⁷ Thanks to Rich Makadok of Emory University for providing me with his doctoral seminar reading list, which served as the basis for this list

7. Waring, GF (1996), "Industry Differences in the Persistence of Firm-Specific Returns," *American Economic Review* 86, pp. 1253-65. DOCTORAL/NEAR-DOCTORAL PAPER.

Secondary Readings

The first group of readings listed in this section deal with methodological and interpretation issues related to the sizes of industry and firm effects. The results highlight important relationships between the effects at different levels.

1. Debate at DRUID in 2005 on the relative importance of industry and firm effects on performance between Anita McGahan, Sid Winter, Janet Bercovitz and Alfonso Gambardella: <http://www.druid.dk/streaming/ds2005/6.html>
2. Hansen, G. & Wernerfelt, B. (1989). Determinants of Firm Performance: The Relative Importance of Economic and Organizational Factors. *Strategic Management Journal*, 10(5): 399-411.
3. Roquebert, J.A., Phillips, R.L., & Westfall, P.A. (1996). Markets Versus Management: What 'Drives' Profitability? *Strategic Management Journal*, 17: 653-664.
4. Brush, T.H., & Bromiley, P. (1997). What does a small corporate effect mean? A variance components simulation of corporate and business effects. *Strategic Management Journal*, 18: 825-835.
5. McGahan, A.M. (1999a), "The Performance of U.S. Corporations: 1981-1994," *Journal of Industrial Economics* 47:4 (December), pp. 373-398.
6. McGahan, A.M. (1999b), "Competition, Strategy, and Business Performance: 1981-1997," *California Management Review* 41:3 (Spring), pp. 74-101.
7. Brush, T.H., Bromiley, P., & Hendrickx, M. (1999). The relative influence of industry and corporation on business segment performance: An alternative estimate. *Strategic Management Journal*, 20(6): 519-547.

The second group of studies listed here deals principally with corporate-parent effects as distinct from business-unit effects: why they arise and their relationships to decisions made by the CEO and constraints placed by the industry structure. The most recent of these studies also considers geographic diversification by multinational enterprises.

8. Chang, S.-J. & Singh, H. (2000). Corporate and industry effects on business unit competitive position. *Strategic Management Journal*, 21(7): 739-752.
9. Bowman, E.H. & Helfat, C.E. (2001). Does corporate strategy matter? *Strategic Management Journal*, 22(1): 1-23
10. Khanna, T. & Rivkin, J. (2001). Estimating the performance effects of business groups in emerging markets. *Strategic Management Journal*, 22(1): 45-74.
11. Ruefli, T.W. & Wiggins, R.R. (2005). Response to McGahan and Porter's commentary on 'Industry, corporate and business-segment effects and business performance: a non-parametric approach'. *Strategic Management Journal*, 26(9): 881-886.
12. Hough, JR (2006), "Business segment performance redux: a multilevel approach," *Strategic Management Journal*, Vol. 27: 1, pp. 45-61. DOCTORAL/NEAR-DOCTORAL PAPER
13. Makino, S, I Isobe and C Chan (2004), "Does Country Matter?," *Strategic Management Journal* 25:10, October, pp 1027-1043.
14. McGahan, A and R Victor (2009), "How Much Does Home Country Matter to Corporate Profitability?," *Journal of International Business Studies* (forthcoming).

The third group of secondary readings deals with a highly related subject: the persistence and sustainability of performance. The early work in this line concentrates on the persistence of firm performance in an aggregate sense. Later studies consider differences in persistence in industry, corporate-parent and business-unit effects.

15. Ghemawat, P (1991), *Commitment: The Dynamic of Strategy* (Free Press)
16. Mueller, DC (1986), *Profits in the Long Run* (Cambridge University Press)
17. McGahan, A.M. & Porter, M.E. (1999). The Persistence of Shocks to Profitability. *Review of Economics and Statistics*, 81(1): 143–153.
18. Furman, J L & McGahan, A M (2002), "Turnarounds," *Managerial and Decision Economics* 23:4-5 (June-August), pp. 283-300.
19. McGahan, A M & Porter, M E (2003), "The Emergence and Sustainability of Abnormal Profits," *Strategic Organization* 1:1 (February), pp. 79-108.

A fourth group deals with the interactions between industry and firm influences in the early phases of market development, principally as detailed case studies, with an emphasis on how individual entrepreneurship generates industry effects:

20. Feldman, M. P. and N. Lowe (2008) "Consensus from Controversy: Cambridge's Biosafety Ordinance and the Anchoring of the Biotech Industry." *European Planning Studies*, 3(16): 395.
21. Klepper, S. (2008), "Silicon Valley – A Chip Off the Old Detroit Block," manuscript.
22. Kaplan, S & Murray, F (2009), Entrepreneurship and the construction of value in bio-technology. *Research in the Sociology of Organizations*, forthcoming.
23. Baum, J A C & McGahan, A (2009), Outsourcing War: Private Military Companies and Command-and-Control Capabilities after the Cold War, manuscript.

Commentary

Since the publication of Richard Schmalensee's 1985 paper on industry and corporate effects on performance, scholars in the field of Strategic Management have written and published a series of papers examining how firm performance – typically represented as return on investment – relates to the firm's participation in particular industries as compared to the idiosyncratic influence of the firm itself distinct from competitors. By describing the relative importance of industry and firm effects, the literature seeks to identify whether managerial intervention through strategic action has a significant impact on firm performance compared to the effects of industry structure, which are often construed as mainly outside the control of corporate management.

There are several characteristics of this literature that are notable. The major studies in this line are descriptive rather than tests of well-defined theories, although a vigorous debate has emerged (see Hansen & Wernerfelt (1989), Roquebert, Phillips & Westfall (1996), McGahan and Porter (2002, 2005), Ruefli and Wiggins (2003, 2005)) about whether inferences are appropriate about causality. For an interpretation of the meaning of the effects based on their qualitative importance, see McGahan (1999b).

The principal empirical strategy is to examine a panel of business units or corporations using simple statistics on variance. The earliest papers in the line (see Rumelt 1991), McGahan & Porter (1997), Bowman & Helfat (2001), and Brush & Bromiley (1997)) evaluated whether random-effects or fixed-effects models were appropriate for identifying industry and firm effects. The more recent literature in the line acknowledges that the effects are nested and models the relationships between the effects directly (Hough (2006), Brush Bromiley & Hendrickz (1999), McGahan & Porter (2003)). A new approach uses rank ordering rather than variance to evaluate the influence of industry and firm effects on performance (Ruefli & Wiggins, 2003).

Some of the studies decompose variance at the level of the business unit (Rumelt (1991), McGahan and Porter (1997, 1999, 2002, 2003)) while others decompose variance at the level of the corporation as a whole (McGahan (1999a), Khanna & Rivkin (2001), Bowman & Helfat (2001)). When performance is modelled for the corporation as a whole, the corporate effects must be interpreted differently than when business-unit effects are also included (McGahan & Porter (2002), Bowman & Helfat (2001), McGahan (1999b)).

A parallel line has emerged on how the effects of industry and of the firm change over time (Mueller (1986), Ghemawat (1991), Waring (1996), McGahan & Porter (1999, 2003), Furman & McGahan (2001), Ruefli & Wiggins (2003)). While the precise empirical design of these studies differs significantly, the main idea in them is to identify which of the effects on performance tend to be most stable over time and to make inferences about their relative importance based on their stability.

Many of the studies in this literature rely on the Compustat business-segment (or “industry-segment”) reports, which relied on an accounting requirement in place from about 1980 to 1997 that firms report by line of business. Many of the companies covered in the reports changed their accounting conventions after about 1997 (and especially after 1999) when this requirement was liberalized. Also note that, in 1993, a change in pension accounting requirements led to the creation of many anomalous, temporary business units. Several studies have sought to overcome the disadvantages of the Compustat reports by applying decomposition techniques to other measures of performance (Chang & Singh (2000), McGahan (1999a)).

The main findings in the literature are that industry, corporate, and business-unit are all important to performance, although there is disagreement about which effects are most significant and why. The disagreements rest on questions about noisiness in the data that obscures industry effects, methods, etc. (McGahan & Porter (2005), Ruefli & Wiggins (2005), Hough (2006)). There is also evidence that all three types of effects are remarkably persistent, and that idiosyncrasies in performance are generally persistent. These results provide strong support for the idea that differences tend to be sustained regardless of their origins in industry and firm factors.

New studies in this line tend to fall in one of three categories. First, some scholars seek to establish the relative importance of geographic differences to performance by decomposing the variance of the performance of firms located in different parts of the world. The leading research in this area distinguishes different kinds of country effects, eg, the host-country effect (Makino et al (2004)) vs. the home-country effect (McGahan and Victor (2009)). Second, a line of research investigates in detailed case studies how industry and firm effects co-evolve, especially in the early phases of industry development or during periods of industry disruption. Leading papers consider how individual entrepreneurs shape institutions that create enduring industry effects (Kaplan and Murray

(2009)). Finally, a final set of studies is investigating the nature of competition and the consequences for how industry and firm influences on performance interact over both the short and long term. The leading research in this line shows how early competitive interaction sets behavioural precedents that are enduring (Klepper (2008), Feldman and Lowe (2008)).

Organization Form: Selection & Consequences

Contributed by Nicholas Argyres & Rachelle Sampson

Foundational

Coase, R. 1937. The nature of the firm. *Economica N.S.*, 4: 386-405

Chandler, A. 1962. *Strategy and Structure: Chapters in the History of Industrial Enterprise*. Cambridge, MA: MIT Press, esp. Chapters 1, 2, 6 and 7.

Klein, B., R. Crawford and A. Alchian. 1978. Vertical integration, appropriable rents, and the competitive contracting process. *Journal of Law and Economics*, 21: 297-326.

Armour, H. and D. Teece. 1978. Organizational structure and economic performance. *Bell Journal of Economics*, 9: 106-122.

Granovetter, M. 1985. Economic action and social structure: A theory of embeddedness. *American Journal of Sociology*, 91: 481-510.

Grossman, S. and O. Hart. 1986. The costs and benefits of ownership: A theory of lateral and vertical integration. *Journal of Political Economy*, 91: 907-928.

Williamson, O. E. 1991. Comparative economic organization: The analysis of discrete structural alternatives. *Administrative Science Quarterly*, 36: 269-296.

Secondary

Internal Organization:

Fligstein, N. 1985. The spread of the multidivisional form among large firms, 1919-1979. *American Sociological Review*, 50: 377-391.

Milgrom, P. and J. Roberts. 1988. An economic approach to influence activities in organizations. *American Journal of Sociology*, 94: S174-S179.

Zenger, T. 1994. Explaining organizational diseconomies of scale in research and development: Agency problems and the allocation of engineering talent, ideas and effort by firm size. *Management Science*, 40: 708-729.

Holmström, B. and P. Milgrom. 1994. The firm as an incentive system. *American Economic Review*, 84: 972-991.

Argyres, N. 1995. Technology strategy, governance structure, and interdivisional coordination. *Journal of Economic Behavior and Organization*, 28: 337-358.

Vertical Integration:

Chandler, A. 1977. *The Visible Hand: The Managerial Revolution in American Business*. Cambridge, MA: Belknap Press.

Monteverde, K. and D. Teece. 1982. Supplier switching costs and vertical integration in the automobile industry. *Bell Journal of Economics*, 13: 206-213.

Masten, S. 1984. The organization of production: Evidence from the aerospace industry. *Journal of Law and Economics*, 27: 403-418.

Masten, S., J. Meehan, and E. Snyder. 1991. The costs of organization. *Journal of Law, Economics and Organization*, 7: 1-22.

Poppo, L. and Zenger, T. 1998. Testing alternative theories of the firm: Transaction cost, knowledge-based, and measurement explanations for make-or-buy decisions in IT Services. *Strategic Management Journal*, 19: 853-877.

Nickerson, J. and B. Silverman. 2003. Why firms want to organize efficiently and what keeps them from doing so: Inappropriate governance, performance and adaptation in a deregulated industry. *Administrative Science Quarterly*, 48: 433:465

Alliances:

D. Teece. 1986. Profiting from technological innovation: Implications for collaboration, licensing and public policy. *Research Policy*, 15: 285-305.

G. Pisano. 1989. Using equity participation to support exchange: Evidence from the biotechnology industry. *Journal of Law, Economics and Organization*, 5: 109-126.

Gulati, R. 1995. Does familiarity breed trust? The implications of repeated ties for contractual choice in alliances. *Academy of Management Journal*, 38: 85-112.

Oxley, J. 1997. Appropriability hazards and governance in alliances: A transaction cost approach. *Journal of Law, Economics and Organization*, 13: 387-409.

Sampson, R. 2004. The cost of misaligned governance in R&D alliances. *Journal of Law, Economics and Organization*, 20: 484-526.

Contract structure:

- Macaulay, S. 1963. Non-contractual relations in business: A preliminary study. *American Sociological Review*, 28: 55-67.
- Joskow, P. 1987. Contract duration and relationship-specific investments: Empirical evidence from coal markets. *American Economic Review*, 77: 168-185.
- Poppo, L. and T. Zenger. 2002. Do formal contracts and relational governance function as substitutes or complements? *Strategic Management Journal*, 23: 707-726.
- Mayer, K. and N. Argyres. 2004. Learning to contract: Evidence from the personal computer industry. *Organization Science*, 5: 394-410.
- Ryall, M. & Sampson, R. 2009. Formal contracts in the presence of relational enforcement mechanisms: Evidence from technology development projects. *Management Science*, 55(6):906-25.

Commentary

This reading list traces research on organizational form that has built upon organizational economics. It also includes readings that challenge this economic approach from a sociological perspective, as well as readings that seek to reconcile the two approaches. In all of these readings, “organizational form” refers to the allocation of decision-making authority, coordination and adaptation mechanisms and the incentive structure within organizations and between them. Note that we refer to organizational form and governance structure interchangeably. The main objective of the readings in this section is to identify organizational forms that have the ideal balance of coordination, control and incentive power, given firm (or inter-firm, in the case of alliances) performance objectives.

The selection of organizational form and its consequences for firm performance were arguably the very first phenomena to be studied in the strategy field. Many scholars trace the origins of the field to Chandler’s (1962) *Strategy and Structure*. Chandler’s hypothesis -- that the multidivisional form (M-form) was an efficiency-enhancing, organizational innovation adopted to accommodate new strategies of diversification -- began a central stream in strategy research known by the title of the 1962 book. The first statistical test of this hypothesis, Armour and Teece (1978), is listed among the foundational readings in this section.

Chandler’s (1977) second book, *The Visible Hand*, emphasized the role of a different aspect of organizational form, vertical integration, in facilitating the growth and profitability of large U.S. firms during the mid-to-late 19th century. This work helped inspire the development of the transaction cost economics of Williamson (1975, 1985). Williamson re-introduced the twin fundamental questions first posed by Coase (1937): why do firms exist, and what limits their scope? While Coase’s arguments have been most often applied to explain vertical integration, the fundamental cost comparison of different organizational forms envisioned by Coase applies to horizontal integration as well.

Along with Klein, Crawford and Alchian (1978), Williamson developed transaction cost theory to help operationalize the theory first posed by Coase (1937). Williamson (1991) represents a relatively comprehensive statement of transaction cost theory, although interested readers should also refer to Williamson’s (1985) book, *The Economic Institutions of Capitalism* (New York, The Free Press) for a full treatment and many additional applications to various organizational phenomena. Transaction cost theory holds that firms, markets, and intermediate organizational

forms are discretely different governance structures, and that efficiency requires that they be matched with different kinds of transactions in a discriminating way. Readers should also be aware that the historical accuracy of Klein, Crawford and Alchian's (1978) central example of GM-Fischer Body has been questioned by several scholars (e.g., R. Casadesus-Masanell & D. Spulber (2000) "The Fable of Fisher-Body." *Journal of Law and Economics* 43, no. 1: 67-104]. Notwithstanding the challenges to the accuracy of this example, the theoretical argument in the Klein et al. paper remains valid and has substantial empirical backing in other contexts.

Grossman & Hart (1986) propose a closely related theory, that formalizes some, but not all, of the insights of transaction cost theory. The key implication of this theory is that residual rights of control over an asset should be allocated to the party whose contribution to the transaction is relatively more important to the performance of that transaction. This "property rights" theory offers greater precision than Williamson's verbal theory, particularly in defining ownership and in predicting the directions of integration. However, while property rights theory emphasizes selection of organizational form based on minimizing ex ante bargaining problems, it does not capture the problem of ex post adaptation that Williamson emphasized, and has also proven more difficult to test due to difficulties in developing appropriate measures for its key variables. Notwithstanding this, property rights theory has spawned a large theoretical literature and some empirical studies (e.g., Baker and Hubbard (2001) "Make Versus Buy in Trucking: Asset Ownership, Job Design and Information." *American Economic Review* Vol.93:551-72).

Transaction cost theory attracted the attention of sociologists because it in part arose from, and therefore spoke to, concerns articulated in organization theory. Sociologists tended to accept the first of transaction cost theory's basic behavioral assumptions, that economic agents are "rational but boundedly so" (Simon 1962). The second assumption, that agents may be opportunistic, was treated more skeptically. Granovetter (1985), for example, expressed concern that this latter assumption suggested an "undersocialized" view of economic life. Granovetter argues that, because individuals are usually embedded in networks of social relationships, even risky transactions can often occur through market exchange. Granovetter's article set the stage for a debate that continues to be important in the strategy literature, particularly the literature on contracting and alliances.

The secondary readings fall into four areas that build upon the foundational research. Research on internal organization in organizational economics has focused on the limits for firm size and scope. A major theme in this work is that firms cannot perfectly replicate the high-powered incentives available in market, and that this accounts for the limits to firm size and scope. Examples of this include Holmstrom and Milgrom (1994) (theory) and Argyres (1995) (empirical). An important counterpoint to these papers is Fligstein (1985), who challenged the assumption in organizational economics that internal organization is driven by efficiency, offering evidence that power and other considerations are more important.

Transaction cost theory's was originally developed to explain vertical integration, and much research on that phenomenon has followed. The readings in this section include some of the early empirical research that aimed to develop appropriate measures of transaction cost theory's main determinant of vertical integration, asset specificity. A notable example of high quality research here and one of the first empirical studies directly linking organizational form to performance is Masten, Meehan, Snyder (1991). This paper was the first to examine transaction cost hypotheses using methods that account for the self selection problem; that is, firms choose organizational form based on the perceived performance attributes of such forms. This problem makes it difficult to draw clear inferences from simple qualitative choice models of make-or-buy choices, a pervasive problem in any study seeking to link organizational form with performance. Masten et al. (1991) go beyond these simple models to estimate the costs of different organizational forms for a particular transaction, including the costs of internal organization. As such, the paper represents a substantial advance over prior work both conceptually and empirically, moving beyond a discussion of the costs of the market to a more thorough analysis of integration. Other important studies in the area include Poppo and Zenger (1998), who offer a test of transaction cost theory against other theories in strategy aiming to explain make-or-buy choices, and Nickerson and Silverman (2003), who show that sociological considerations can affect the speed at which economization on transaction costs occurs.

Alliances have been a very popular topic of research in the strategy field, and contributions from scholars using transaction cost theory have been very influential. These scholars have extended the transaction cost framework to operationalize relevant inter-firm drivers of transaction costs, such as concerns over intellectual property leakage. Two of the listed studies emphasize the importance of intellectual property protection considerations in determining the structure of alliance relationships (Pisano 1989; Oxley 1997), while Sampson (2004) shows that alliance

governance choices that are misaligned from a transaction cost standpoint are costly. Sociologists have studied other factors that may substitute for formal governance; Gulati (1995) finds evidence both for the effects of transaction costs as well as prior ties between partners on alliance structure. He argues that this finding reflects the development of trust, a factor that many sociologists have emphasized.

This theme of trust versus safeguarding through formal contractual provisions also appears in the literature on the details of contract structures. Macaulay (1963) emphasized that non-contractual relations are important in business to the extent that formal contracts may not only be unnecessary, but limiting, and many sociology-oriented scholars argue that trust substitutes for formal governance in contractual relationships. However, some recent empirical work on contract structure suggests that trust and formal governance may in some circumstances act as complements rather than substitutes. This may in part be due to the communication and learning benefits that formal contracting can provide to the parties (Poppo and Zenger 2002; Mayer and Argyres 2004), or because contracts clarify when renegotiation is required as well as when partners may legitimately either impose informal sanctions and/or terminate the relationship (Ryall and Sampson 2009). The fifth reading in the section, Joskow (1987), was among the first to find support for a transaction cost explanation of contract duration.

There are a number of important empirical challenges that researchers face when studying the determinants of organizational form and its relationship with firm performance. For example, the theoretical mechanisms described in these theories operate at a very micro level, so that tests require detailed data that is often difficult to collect. In addition, it is often difficult to fully account for differences in transaction characteristics. Researchers must be aware that unobserved factors may drive a firm's decision to select a particular organizational form. Without a thorough understanding of what underlies the choice of organizational form for a particular firm, inferences from data on form and performance will be limited and possibly biased. The empirical studies highlighted here represent a sampling of those papers that have made inroads at thinking deeply about the organizational form problem and/or taken novel empirical steps to correct for unobserved drivers of organizational form.

It is important to note that the economic and sociological theories that are featured on this reading list have been applied to a much wider range of phenomena than are represented here. Other areas of application, some of which relate to those on lists already in (or to be added to) this reader,

include diversification, corporate governance, the multinational enterprise, and political strategy.

The Antecedents and Performance Implications of Reputation and Status

Contributed by Peter Roberts

Assigned Readings (*) = particularly important

Benjamin, B.A., J.M. Podolny. 1999. Status, quality, and social order in the California wine industry. *Administrative Science Quarterly* **44** 563-589. (*)

Fombrun, C. 1996. *Reputation: Realizing Value from the Corporate Image*. Harvard Business School Press, Boston.

Fombrun, C., M. Shanley. 1990. What's in a name? Reputation building and corporate strategy. *Academy of Management Journal* **33** 233-258. (*)

Gorton, G. 1996. Reputation formation in early bank note markets. *Journal of Political Economy* **104** 346-397.

Hall, R. 1993. A framework linking intangible resources and capabilities to sustainable competitive advantage. *Strategic Management Journal* **14** 607-618.

Landon, S., C.E. Smith. 1997. The use of quality and reputation indicators by consumers: The case of Bordeaux Wine. *Journal of Consumer Policy* **20** 289-323.

Merton, R.K. 1968. The Matthew effect in science. *Science* **159** 56-63. (*)

Phillips, D.J., E.W. Zuckerman. 2001. Middle-status conformity: Theoretical restatement and empirical demonstration in two markets. *American Journal of Sociology* **107** 379-420.

Podolny, J.M. 1993. A status-based model of market competition. *American Journal of Sociology* **98** 829-872. (*)

Podolny, J.M. 1994. Market uncertainty and the social character of economic exchange. *Administrative Science Quarterly* **39** 458-483. (*)

Podolny, J.M. 2001. Networks as the pipes and prisms of the market. *American Journal of Sociology* **107** 33-60.

Podolny, J.M. 2005. *Status Signals: A Sociological Study of Market Competition*. Princeton University Press, Princeton.

Podolny, J.M., D.J. Phillips. 1996. The dynamics of organizational status. *Industrial and Corporate Change* **5** 453-471.

- Rao, H. 1994. The social construction of reputation: Certification contests, legitimation, and the survival of organizations in the American automobile industry: 1895-1912. *Strategic Management Journal* **15** 29-44. (*)
- Rhee, M., P.R. Haunschild. 2006. The liability of good reputation: A study of product recalls in the U.S. automobile industry. *Organization Science* **17** 101-117.
- Roberts, P.W., G.R. Dowling. 2002. Corporate reputation and sustained superior financial performance. *Strategic Management Journal* **23** 1077-1093. (*)
- Shapiro, C. 1983. Premiums for high quality products as returns to reputations. *Quarterly Journal of Economics* **98** 659-679.
- Stuart, T.E. 2000. Interorganizational alliances and the performance of firms: a study of growth and innovation rates in a high-technology industry. *Strategic Management Journal* **21** 791-811.
- Stuart, T.E., H. Hoang, R.C. Hybels. 1999. Interorganizational endorsements and the performance of entrepreneurial ventures. *Administrative Science Quarterly* **44** 315-349.
- Washington, M., E.J. Zajac. 2005. Status evolution and competition: Theory and evidence. *Academy of Management Journal* **48** 282-296.
- Weigelt, K., C. Camerer. 1988. Reputation and corporate strategy: A review of recent theory and applications. *Strategic Management Journal* **9** 443-454.

Commentary

One important area of strategy research touches on the relationship between various intangible assets and firm performance (Hall 1993). These assets include managerial and organizational networks, organizational cultures, as well as perceptual assets like reputation and status. The aim of this session is to introduce the strategy research that examines these latter two types of assets as well as the related research found within the economics and sociology literatures.

Corporate reputations (Fombrun 1996) and organizational status (Podolny 2005) are important to strategy researchers given their documented theoretical and empirical links to firm behavior and performance. Lacking complete information about the quality of firms or their product and service offerings, consumers, employees, investors and other suppliers draw inferences based on firm features that are observable – like reputation and status. Thus, firms with better reputations or higher status tend to participate in more attractive upstream and downstream exchanges. At the same time, the processes that generate reputations and status orderings can lead to semi-permanent firm heterogeneity (Podolny and Phillips 1996; Roberts and Dowling 2002).

Early treatments of corporate reputation within the strategy literature build from game-theoretic accounts of competitors interactions and focus on a firm's 'reputation for toughness' (Weigelt and Camerer 1988). More recently, attention has shifted to considering corporate reputation as an indicator of otherwise unobservable firm quality (Fombrun and Shanley 1990; Fombrun 1996). Reputations accumulate with prior firm actions and/or quality demonstrations (Shapiro 1983) to form one such quality indicator. As such, good reputations are now known to confer both survival (Rao 1994) and long-term financial performance (Roberts and Dowling 2002) benefits for the firms that possess them.

Since at least Merton (1968), we have also known that the social structure of fields or markets influences the returns that individuals and organizations receive for their quality demonstrations. Extending this basic insight, Podolny's (2001) discussion of networks as pipes and prisms shows how visible associations with prominent others generates another quality signal that helps reduce alter-centric uncertainty. Bridging to strategy research, we now appreciate that firms with higher status are advantaged in ways that lead to improved performance (Podolny 1993, 1994). This has been documented in the case of new entrepreneurial ventures (Stuart *et al.* 1999) and in the context of pricing dynamics within product markets (Benjamin and Podolny 1999).

Specific Issues in the Search for Deeper Understandings

In thinking about the financial performance implications of these two intangible assets, it is important to pay attention to the specific definitions of reputation and status that we invoke. For the most part, economists tend to focus on reputation while sociologists pay more attention to status. Therefore, it is tempting to simply think of the former (latter) as an economic (sociological) construct. This is the approach taken by Washington and Zajac (2005) in their discussion of reputation and status in the context of college sports. However, this is probably an over-simplification and it seems preferable to think of reputation as accumulating with a firm's own previous performance outcomes and other prior demonstrations. When stakeholders do not have ready access to this indicator, they may then draw their inferences from the even more indirect status-based indicator. This is the approach advocated by Podolny (2005).

With clearer definitions, we are better able to isolate and explicate the mechanisms that link reputation and status to firm performance. Decent examples of this are found in Podolny's (1993; 1994) explication of his status-based model of market competition and in Roberts and Dowling's (2002) elaboration of the link between corporate reputation and long-term financial performance. More generally, it is important to pay attention to the assumptions that are made about what information is available to the various market participants. Good examples of this practice are found in papers that stress the importance of alter-centric uncertainty for the expectation of signaling effects from producer status (Stuart 2000; Podolny 2001). This is also handled nicely in papers that might be considered a bit "further afield" for strategy researchers, including Landon and Smith's (1997) analysis of individual and collective reputation in Bordeaux wine markets and Gorton's (1996) analysis of reputation formation in early bank note markets in the United States.

By more carefully defining terms and specifying causal mechanisms, we have an enhanced capacity to move past expecting simple and unwavering positive performance effects for better reputations or higher status. An excellent example of this kind of progress is found in Phillips and Zuckerman's (2001) elaboration of middle status conformity theory. Based on specific assumptions about the performance implications of risky behavior for high, middle and low-status firms, they help us to understand why high and low status firms are more likely than middle status firms to engage in less legitimate business practices. Another recent study offers a possible boundary condition for the reputation-performance relationship by asking when favorable reputations might actually be harmful to the firms that possess them. This is the claim made and then supported by Rhee

and Haunschild (2006) in their analysis of reputation and product recalls in the automobile industry.

III. Industry Dynamics

In her list on **Entry** Anne Marie Knott focuses on research aimed at answering two important questions: (i) When are firms driven to enter new markets?; and (ii) Whether a firm should enter a particular market, or is entry viable? In exploring this fundamental topic, important links emerge with other topics included in the reader, most notably issues related to **Sustainability**, **Industry Evolution** and **Industry and Firm Effects**. In the interests of focus, Knott limits her attention primarily to the incentives for **incumbents** to enter new markets; see **Innovation** for some of the different incentives facing new entrants.

Innovation is a huge topic, and one of central interest to many strategy scholars. In his reading list on the topic, Bruno Cassiman goes back to basics, to early IO theory and an analysis of the incentives for R&D and innovation. He then goes on to look at other elements of the firm environment affecting the incentive to invest in R&D and innovation. The focus is primarily on theoretical developments in the field reflecting Cassiman's view that "we still do not have theoretical models that predict sufficiently well the observed phenomenon"; recent empirical applications in strategy research are also highlighted.

In **Industry Evolution** Brian Silverman looks at the combined effect of entry, exit and growth on cumulative changes in industry characteristics over time. In tracing the development of the literature, Silverman notes the impressive progression from models in which industry change is exogenously imposed to those that grapple with endogenously driven change. The literature also encompasses an impressive diversity of theoretical perspectives, including theories rooted in traditional industrial organization, evolutionary economics, and more sociological perspectives – organizational ecology, resource partitioning and niche overlap. Recent work has also paid increasing attention to firm heterogeneity (providing a link to, for example, **Sustainability**), and geography (see **Geography and Agglomeration**).

Market Entry: Theoretical Foundations (and some empirical evidence)

Contributed by Anne Marie Knott

Core Readings

- Bresnahan, T and P. Reiss 1990 Entry in Monopoly Markets. *Review of Economic Studies*, 57 (192) 531-554.
- Foster, L., J. Haltiwanger, and C. Syverson 2008. Reallocation, Firm Turnover, and Efficiency: Selection on Productivity or Profitability? *American Economic Review*, 98(1): 394–425
- Gilbert, R. 1989, "Mobility Barriers and the Value of Incumbency in Schmalensee and Willig (ed), *The Handbook of Industrial Organization*, North Holland: Elsevier Science Publishers: 475-536.
- Helfat, C. and M. Lieberman 2002. The Birth of Capabilities: Market Entry and the Importance of Pre-history", *Industrial and Corporate Change*, 11(4): 725-760.
- Lieberman, M. 1987. Excess Capacity as a Barrier To Entry: An Empirical Appraisal. *Journal of Industrial Economics*, 35(4): 607-627.
- Penrose, E. 1959. *The Theory of the Growth of the Firm*, New York: Wiley

Supplemental Readings

- Bain, J. S., 1949, A Note on Pricing in Monopoly and Oligopoly, *American Economic Review* 39 (2): 448-464.
- Berry, S. and J. Waldfogel 1999 Free entry and social inefficiency in radio broadcasting. *RAND Journal of Economics*, 30 (3): 397-420
- Dixit, A. 1979. A model of duopoly suggesting a theory of entry barriers. *Bell Journal of Economics*, 10 (1): 20-32.
- Gilbert, R.J. and D.M. Newbery (1992) 'Alternative Entry Paths: The Build or Buy Decision', *Journal of Economics and Management Strategy*, 1(1): 129-50.
- Jovanovic, B. 1982 Selection and the evolution of industry. *Econometrica* 50 (3): 649–670.
- Kreps, D. 1990. *A Course in Microeconomic Theory*, Princeton, NJ: Princeton University Press

Mankiw, G and M. Whinston 1986 Free entry and social inefficiency, *RAND Journal of Economics*, 17 (1): 48-58.

Porter 1982. *Competitive Strategy: Techniques for Analyzing Industries and Competitors*, Cambridge, MA: the Free press.

Schmalensee, R. 1978, Entry deterrence in the ready-to-eat breakfast cereal industry. *Bell Journal of Economics*, 9 (2): 305-327

Schmalensee, R. 1981 Economies of Scale and Barriers to Entry, *Journal of Political Economy*, 89 (6) 1228-1238.

Spence, A. M. 1977 Entry, capacity, investment and oligopolistic pricing, *Bell Journal of Economics*, 8 (2): 534-544.

Stackelberg, H. 1934 *Marktform and Gleichgewicht*. Berlin: J. Springer.

1 Introduction

Entry is important to strategy for two reasons. Offensively, entry is one of the most salient and prevalent strategic moves in the economy. New firms enter the economy at an annual rate of 10-12% of incumbents (entrepreneurial entry); existing firms enter new markets at about the same rate (Foster, Haltiwanger and Syverson 2008). Defensively, free entry should dissipate profits, so we want to understand the conditions under which firms can/should enter as well as the conditions under which incumbents can preclude entry. While both entrepreneurial entry and incumbent entry into new markets are related and important, because this is a strategy reader I focus on the latter.¹⁸

There are four basic questions regarding incumbent entry into new markets. These are *when* firms are driven to enter new markets, *where* (or which form of entry), *whether* to enter a particular market, and *how* to enter (entry mode). I treat the topics of when and whether in the main summary and leave the other topics for the appendix because they intersect topics elsewhere in the reader.

2 When firms are driven to enter new markets

The best foundational treatment of this question comes from Penrose (1959). There are essentially two answers: 1) firms facing negatively sloped demand curves inevitably exhaust growth in an existing market and therefore enter new markets to sustain growth; 2) firm capacity increases over time through learning curves and "receding managerial limits". These slack resources provide a stimulus (and means) to pursue new markets.

Helfat and Lieberman (2002) extend Penrose to look explicitly at the relationship between resources and the when, where and how of entry. One of their main insights is that the Penrosian view of entry, where a firm's entry choices flows from its resources, is inconsistent with views such as Jovanovic (1982), where firms capability is only revealed post-entry. While true, it is worth noting two things. First, Jovanovic's model pertains to entrepreneurial choice between self-employment and wage employment where ex ante uncertainty is more likely. Second, even in the case of incumbent entry into new markets there is likely to be some uncertainty regarding the utility of the resources in the new setting.

¹⁸ This forces me to exclude some nice models of entry: the evolutionary economics models of entry/exit/growth of new markets; models of entrepreneurial entry from the self-employment literature, and models of entrepreneurial spinoff from existing firms.

3 Whether to enter (is entry viable?)

Most scholars when asked to define the "entry" literature would focus exclusively on whether. Before getting into the formal entry literature from Industrial Organization Economics (IO), it is worth mentioning that probably the canonical piece on corporate entry is Porter (1982). His five forces framework synthesizes much of the IO literature aimed at rooting out anti-competitive practices and inverts it as a guide for identifying attractive industries. Thus while it is nominally a book on competitive strategy, a major component is whether a market has profit potential, and thus is more valuable for those contemplating entry into a market than those already engaged in the market.

With that as an aside, the formal literature on entry isn't. As Kreps (1990:342) says when closing his discussion of entry in *A Course on Microeconomic Theory*: "In a sense there are not theories of entry, but theories of no-entry". The goal of the entry literature is explaining why entry into markets ceases before profits are dissipated. Thus a better way to characterize the literature is one of entry barriers. The easiest way to navigate this literature is through survey pieces such as Gilbert (1989). However, the seminal piece in this literature is Stackelberg (1934) who derives permanent asymmetry in market shares (first mover advantage) in a duopoly where firm 1 chooses capacity before firm 2. This was followed much later by works proposing and modeling different entry barriers: *limit pricing* -pricing below the collusive monopoly price to dissuade entry (Bain 1949), *sunk costs* (Spence 1977, Dixit 1979), *product proliferation* (Schmalensee 1978), and *economies of scale* (Schmalensee 1981).

Most of this theory on entry barriers was motivated by salient examples. Thus an important companion literature is empirical tests of the theory. That work tends to find little support for the strategic use of entry barriers. Lieberman (1987) for example finds little evidence that excess capacity in the chemical industry is strategic, and Bresnahan and Reiss (1990) find that second entrants have the same costs and market opportunities as first entrants in 202 distinct markets.

One reason for the failure to find empirical support for entry deterrence is found in recent work proposing there is too much entry (Mankiw and Whinston 1986) and empirical support for that view (Berry and Waldfogel 1999).

4 Future directions for high quality research

There are problems with the entry barrier concept, thus there is potential to do high quality research that builds on prior work on entry. First, entry barriers don't appear to dissuade entry. New firms enter the economy at a rate of 10-12% of incumbents per year and existing firms enter new markets at a comparable rate. We shouldn't see this if entry barriers are effective.

Second, entry theory reveals an "entrepreneur's paradox": attractive industries are those with opportunity for sustainable profits. These are by definition are those which can't be entered (because of entry barriers). If markets don't have entry barriers, what assumptions allow firms to conclude there will be profits in equilibrium, which in turn cause them to enter?

Third, even ignoring entry barriers, the record on entry is dismal--60% of firms fail within 4 years. Why does entry persist? What does the entry decision function look like if it supports such seemingly irrational entry?

One approach we might take in reconciling all the above is to recondition entry theory—Rather than using it as a tool to examine the act of entry, offer it as a tool for examining *viable entry*. Some of the models lend themselves to this reapplication, but some do not (the models whose power comes from conditioning follower behavior, e.g., limit pricing).

As we retool entry theory, one question of interest is the extent to which the barriers are industry specific (demand, technology (minimum efficient scale-MES)) versus firm specific. Are entry barriers anything more than aggregated bases of competitive advantage for firms within the industry? To what extent do industry characteristics define the bases (exogenous entry barriers), versus the bases define the industry characteristics (endogeneous entry barriers). The high variance in entry rates across industries may help us understand this question.

What will be required to exploit this potential is closer alignment between entry theory and entry empirics. The international literature has contributed many of the stylized facts relating firm and market characteristics to entry decisions. This is because the international setting (like the chain setting) has a natural experiment quality—observing entry by the same firm in multiple markets and multiple firms in same market. What would help the "new theory of entry" is greater formalism in these empirics—more explicit characterization of the firm's decision function. This supports more direct test of existing theory. Better empirical characterization of the firm's decision function will in turn support more realistic formal modeling of firm strategic behavior and market outcomes.

Appendix

A1 Where do firms enter (or what avenues do they pursue?)

Given the imperative to enter new markets, there are three broad avenues of entry. The choice among these avenues is driven largely by the type of resource being redeployed: 1) *product development*: utilizing slack marketing/distribution resources to take new product/services to existing customers, 2) *geographic expansion*: utilizing slack operational resources to take existing products/services to new geographic markets, and 3) *diversification*: utilizing slack headquarters resources to introduce new products/services to new markets.¹⁹ For all these avenues "new" is relative to the firm rather than to the world.

A1.1 Product development²⁰

There are essentially two forms of product development. The first is *displacing innovation*--replace an exiting product with a higher quality or lower cost version. The other is *product line extension*--introducing alternative versions of the product or introducing complementary products. Because product line extension is essentially a hybrid of displacing innovation and diversification (which I discuss later), I focus on displacing innovation.

Formal treatment of new product development asks the questions: when will new products be introduced, with what improvement and by whom? It is best captured by models of R&D in which a monopolist faces potential displacement by a lower cost entrant (Gilbert and Newbery 1982, Reinganum 1983 are specific examples, but Reinganum 1989 offers a good survey). The literature asks when a monopolist will pre-empt the entrant by introducing a new product (or lower cost version of an existing product). While these models are important because of their precision in

¹⁹ An additional form of market entry is vertical integration. We ignore it here because firms vertically integrate for reasons other than the desire to enter new markets: to fortify positions in existing markets, e.g., Standard Oil's entry into railroads, or to solve market failure problems in the supply chain, e.g., General Motors acquisition of Fisher Body. Market failure rational for vertical integration is a central issue in transaction cost economics (discussed elsewhere in the reader).

²⁰ The product development discussed here may seem narrow in that it deals with displacing innovation. However one could argue that all innovation is displacing in that entirely new functions are rare (e.g., copiers replaced carbon paper, white out replaced erasers, calculators replaced adding machines, cell phones replaced landlines and pagers).

linking firm behavior to market structure, they are typically one-shot models of duopolists whereas the world of product development seems better characterized as continuous innovation by at least a handful of firms. A nice model which retains the duopoly structure but adds continuous innovation is Adner and Levinthal (2001). They show how the rate of innovation (new product versions) differs for monopoly versus duopoly markets given the degree of demand heterogeneity. Their main result is that under duopoly price and performance will improve in all periods, whereas under monopoly, improvements cease once the marginal customer has been brought into the market. Thus in addition to characterizing the "where" of entry, Adner and Levinthal's model also addresses the rate of entry.

A1.2 Geographic expansion

While the central entry issues for product development are when, what, and who of new products, the central question for geographic expansion is where firms will expand. Probably the best literature examining geographic expansion is the literature on foreign direct investment (FDI).²¹ Implicitly this literature treats location choice as maximizing the net present value (NPV) of entry by choice of location over an infinite choice set. In practice, the literature empirically examines factors affecting the NPV of a given location. While several factors affect all firms equally, such as the extent of demand, the cost of inputs/transportation and the institutional structure, the factors of greatest interest to strategy scholars are ones differentially affecting firms. Two of these are agglomeration economies (Shaver and Flyer 2000) and local experience (Shaver, Mitchell and Yeung 1997). Shaver and Flyer challenge the conventional wisdom that firms will cluster geographically to exploit agglomeration economies. They argue (and find) instead that agglomeration economies attract weakly endowed firms, while repelling well-endowed firms (under the logic that well-endowed firms contribute to rather than gain from agglomeration economies). Shaver, Mitchell and Yeung 1997 argue (and show) that in addition to the resources initially driving firms toward geographic expansion, firms develop new resources during expansion that affect subsequent expansion. In particular, expansion into each new country requires fixed investments (offering scale

²¹ An additional literature addressing geographic expansion is that on franchises. While the literature on franchises has dominated by transaction cost economics (because the franchise governance form is an anomaly in the choice between markets and hierarchies), this literature has interesting things to say about the optimal number of markets and the degree of overlap.

economies and learning) reducing the cost of subsequent entry in that country.

A1.3 Diversification

The first two forms of entry typically exploit scope economies from existing resources--product development exploits market resources associated with serving existing customers, whereas geographic expansion exploits operational resources associated with an existing product/service. These modes stand in contrast to diversification, where the resources being redeployed are headquarters' functions such as legal, human resources, information technology and financial control systems. The seminal piece in the diversification literature is Rumelt (1972). Rumelt and subsequent scholars tend to find that strategies of unrelated diversification lead to lower profits and stock market returns. Thus the "theories" of unrelated diversification are largely theories of why firms employ this strategy given its apparent inferiority. The dominant explanations are *free cash flow* (Jensen 1986)--firms exhaust more profitable opportunities and are reluctant to return the cash to shareholders, *incentive misalignment*--managers diversify to reduce their own employment risk (Amihud and Lev 1981, Shleifer and Vishny 1990), and *hubris* firms overestimate the opportunities to exploit existing resources in the new market (Roll 1986). All three explanations have substantial empirical support. On average diversified corporations trade at a 20% discount relative to their breakup value (Collis and Montgomery 2004). A recent twist in this literature (Villalonga 2004) holds that the problem is not diversification per se, rather the "diversification discount" really reflects the poor quality of firms employing the strategy.

A2. What mode of entry

The final issue of entry pertains to mode: whether to enter *denovo* (also called *greenfield*) versus acquiring an existing firm. As with all entry decisions the underlying logic is that firms choose the mode offering the highest NPV. The entry mode literature is largely empirical--how do various factors such as investment cost, technology and market structure affect the NPV of each mode.

Early studies in strategy tended to be all up tests of one mode versus another and tended to find that *greenfield* investments had higher performance. In an important paper, Shaver (1998) argues that the

conclusion is erroneous due to an omitted variable problem. In particular, choice of mode should be driven by firm endowments. Shaver shows that once we account for endogenous mode choice, there is no net benefit to greenfield strategies. Since then, empirical work has focused on the mode decision rather than mode performance. That work, e.g., Bryce and Winter (2006) finds that mode choice is driven both by market factors common to all firms as well as firm specific factors. With respect to market factors, when markets are growing, firms tend to enter via greenfield investment (because there are few acquisition candidates and/or they enjoy a high premium). Conversely when markets are concentrated, firms tend to enter via acquisition (to gain market power by removing a competitor). With respect to firm-specific factors, firms with substantial related knowledge and/or high levels of R&D choose denovo entry (since their approach is superior to that of targets). In contrast firms diversifying into a new area tend to enter via acquisition (to obtain the requisite knowledge).

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Innovation and Strategy: The Incentive to Innovate

Contributed by Bruno Cassiman

Incentives to Innovate:

- The Basics: Replacement Effect versus Business Stealing
- Appropriability and Spillovers
- Complementarity of (Innovation) Activities and Assets
- Organization of Innovation
- Market Structure and Competition

Key Readings

The *readings are considered foundational papers that develop the theory. The other key readings provide the background and empirical regularities for the topic.

- Acs, Z. and D. Audretsch, 1988, Innovation in Large and Small Firms: An Empirical Analysis, *American Economic Review*, 78, 4, p.678-690.
- *R. Gilbert, 2006, "Innovation and Competition," *Journal of Industrial Organization Education*, V1, Article 8.
- Jaffe, A. 1986, Technological Opportunity and spillovers of R&D: evidence from firm's patent, profits and market value, *American Economic Review*, 76, 984-1001.
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Supplemental Readings on Incentives for R&D and Innovation

The Basics

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Appropriability and Spillovers

- De Bondt, R., 1997. "Spillovers and innovative activities." *International Journal of Industrial Organisation*, 15(1), p.1-28.
- Griliches, Z. 1992. "The Search for R&D Spillovers," *Scandinavian Journal of Economics*, 94, p. S29-47.
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Complementarity of (Innovation) Activities and Assets

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Organization and Innovation

- Anton, J. and D. Yao, 1995. "Start-ups, Spin-offs and Internal Projects," *Journal of Law, Economics and Organization*, 11, p.362-378.
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Market Structure and Competition

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Commentary

Incentives to Innovate

Innovation is becoming central to the strategy of firms in order to secure a sustainable competitive advantage. In this PhD level seminar we attempt to uncover the fundamental drivers of a firm's incentive to innovate. The empirical readings present some empirical regularities on this relation while the theoretical papers present a theory (or contrast different theories) of what might be underlying this incentive. The seminar consists in understanding the potential drivers of the incentive for innovation, learning the existing theories and discussing how well these theories match the empirical findings of this multifaceted phenomenon. We start with a basic set-up which goes back to Arrow (1962), Gilbert and Newbery (1982) and Reinganum (1983). Next, we highlight different potential drivers that affect the incentives to innovate:

- Appropriability and Spillovers
- Complementarity of (Innovation) Activities and Assets
- Organization and Innovation
- Market Structure and Competition

The Basics: Incentives for R&D and Innovation

First we look at the early applications of Industrial Organization theory to R&D investments and innovation. The key starting point for this discussion is the observation that incumbents and new entrants, or, large and small firms differ in how innovative they are. Evidence of this is provided in Acs and Audretsch (1988).

We discuss the *replacement effect* where a monopolist has a weaker incentive to invest in R&D compared to firms in a competitive environment (Arrow, 1962) and contrast this with the *business stealing effect* where an incumbent has a stronger incentive to invest in R&D compared to a potential new entrant (Gilbert and Newbery, 1982). We also contrast the “economic of incentives for R&D and innovation” with alternative explanations that the management literature has provided such as inefficient investments by incumbents (Christensen, 1997) or asymmetric incentives to invest in R&D and innovation due to organizational rigidities (Clark and Henderson, 1993).

The discussion leads to the realization that incumbents and new entrants differ not only in their incentives to innovate but also in the type of innovations that they generate (more or less drastic, radical, disruptive,

basic, general,...) Unfortunately, we lack a clear definition for the way these innovations “differ”. This is where careful (new) theoretical modeling with clear definitions about R&D and innovation can help.

- Acs, Z. and D. Audretsch, 1988, Innovation in Large and Small Firms: An Empirical Analysis, *American Economic Review*, 78, 4, p.678-690.

Acs and Audretsch (1988) set up this discussion by providing evidence that small firms seem more innovative than large firms.

- R. Gilbert, 2006, Innovation and Competition, *Journal of Industrial Organization Education*, V1, Article 8.

The reading by Gilbert (2006) gives a clear basic treatment of the different theoretical models related to the incentives to invest in R&D going from Arrow (1962) to Gilbert and Newbery (1982) and Reinganum (1983). A comparable treatment can be found in Tirole (1988).

- Clayton Christensen, "How can great firms fail? Insights from the hard disk industry." Chapter 1 in *The Innovator's Dilemma*, Harvard Business School Press, 1997, pp 3-28.

Christensen (1997) explains the fact that incumbents have a hard time coming up with disruptive innovations because of their excessive focus on their large existing clients, suggesting that (incumbent) firms make inefficient investments in R&D and innovation. It is interesting to contrast this explanation with the previously discussed theoretical models.

- Henderson, Rebecca M. and Kim Clark (1990), "Architectural Innovation: The Reconfiguration of Existing Product Technologies and the Failure of Established Firms", *Administrative Science Quarterly*, Vol. 35, pp. 9-30.

Henderson and Clark (1990) argue that incumbents differ from new entrants in the constraints that their existing organization (and history) puts on the type of innovations developed. Again, this paper is interesting to

contrast the different explanations for the divergence between established firms and new entrants with the developed theoretical models. This paper provides an explanation for the asymmetric incentive to invest in certain technologies.

In the following discussion we go on to look at other elements of the firm environment that can affect the incentive to invest in R&D and innovation.

Appropriability and Spillovers

Arrow (1962) early on pointed out the fact that unlike regular investment incentives the incentives to invest in R&D and innovation are affected by the fact that not all returns to the investment can be appropriated, the so-called *spillovers*. Information leaking out to other market participants reduces the firm's incentive to invest in R&D and innovation. Jaffe (1986) provides evidence about the importance of these spillovers at the firm level affecting innovation, profits and market value. D'Aspremont and Jacquemin (1988) captures the essence of this spillover effect in a very simple and stylized model which allows us to be precise about the effects of spillovers and possible remedies such as the organization of research joint ventures.

Complementarity of (Innovation) Activities and Assets

So how can firms now profit from innovation? Teece (1986) in his seminal paper argues that firms can profit from innovation by either selling their innovation on the technology market or incorporating their innovation in a product or process and commercializing the idea. The decision depends on the complementary downstream assets that the firm controls. This discussion leads us to realize that the marginal returns to innovation depend on other related activities of the firm.

Milgrom and Roberts (1995) lays out the theory of complementarity between different activities of the firm – i.e. the fact that the marginal return to one activity depends on the level of another activity. They apply this to the choice between flexible versus standardized manufacturing technology and demonstrate that the choice of production technology has important consequences for many other – complementary – decisions of the firm.

Cassiman and Veugelers (2006) apply this to the innovation environment where they show that own R&D investments and external knowledge acquisition are complementary innovation activities affecting the incentive to invest in innovation.

Organization and Innovation

How companies organize internally can seriously affect incentives to invest in R&D and innovation. Saxenian (1994) compares how HP and Digital Equipment Company were organized in the early 90s. The companies were very similar in size and active in the same industries, but their internal organization differed radically, which led to important differences in their innovation outcomes. Aghion and Tirole (1994) provide a simple theoretical model that seems to capture these important effects of organization on R&D incentives by relating ownership of the innovation to the effort that researchers or other important agents expend in order to commercialize the innovation and appropriate returns from this innovation. Lerner and Mergers (1997) attempts to test the Aghion and Tirole (1994) model and discuss the relative importance of different elements in biotech alliances on innovation outcomes.

Market Structure and Competition

Finally, the relation between market structure and the incentives to invest in R&D and innovation is probably the most studied and debated in Economics. Cohen and Levin (1989) lay out the key empirical findings on this relationship. Vives (2008) is an attempt to integrate the disparate theoretical models that have been developed in the literature. Nevertheless, there is still little consensus on the effects of competition on the incentives to invest in R&D and innovation. As Vives (2008) shows, it depends on how competition is actually defined and on the type of innovation considered.

From this discussion on the drivers of the incentive to invest in R&D and innovation it becomes very clear that we still do not have theoretical models that predict sufficiently well the observed phenomenon. Many interesting questions (fortunately for researchers!) remain unresolved.

Industry Evolution

Contributed by Brian Silverman

Core readings

1. Gort, M. and S. Klepper (1982), "Time paths in the diffusion of product innovations," *Economic Journal*, 92: 630-53
2. Klepper, S. (1996), "Entry, exit, growth, and innovation over the product life cycle," *American Economic Review*, 86: 562-583.
3. Hannan, M.T., G.R. Carroll, E.A. Dundan and J.C. Torres (1995), "Organizational evolution in a multinational context: Entries of automobile manufacturers in Belgium, Britain, France, Germany, and Italy," *American Sociological Review*, 60: 509-528.
4. Thompson, P. (2005), "Selection and firm survival: Evidence from the shipbuilding industry, 1825-1914," *Review of Economics and Statistics*, 87(1): 26-36.
5. Tripsas, M. (1997), "Unraveling the process of creative destruction: Complementary assets and incumbent survival in the typesetter industry," *Strategic Management Journal*, 18(Summer): 119-142.
6. de Figueiredo, J.M. and B.S. Silverman (2007), "Strategic dynamics among dominant and fringe firms in a segmented industry," *Management Science*, 53: 632-650.
7. Hoetker, G. and R. Agarwal (2007), "Death hurts, but it isn't fatal: The postexit diffusion of knowledge created by innovative companies," *Academy of Management Journal*, 50(2): 446-467.

Supplementary readings

Stylized facts:

Caves, R.E. (1998), "Industrial organization and new findings on the turnover and mobility of firms," 36(4): 1947-1982.

Dunne, T., M.J. Roberts & L. Samuelson (1988), "Patterns of firm entry and exit in U.S. manufacturing industries," *Rand Journal of Economics*, 19(4): 495-515.

Geroski, P. A. (1995), "What do we know about entry?" *International Journal of Industrial Organization*, 13(4): 421-440.

Duelling models of industry evolution:

Jovanovic, B. (1982), "Selection and the evolution of industry," *Econometrica*, 50(3): 649-70.

Jovanovic, B. and G. MacDonald (1994), "The life cycle of a competitive industry," *Journal of Political Economy*, 102(2): 322-47.

Levinthal, D.A. (1991), "Random walks and organizational mortality," *Administrative Science Quarterly*, 36: 397-420. [Technically not about industry evolution, but related model of organizational age-survival relationship as function of random shocks.]

Sociological approaches to industry evolution:

Baum, J.A.C. and C. Oliver (1996), "Toward an institutional ecology of organizational founding," *Academy of Management Journal*, 39(5): 1378-1427.

Baum, J.A.C. and J.V. Singh (1994), "Organizational niches and the dynamics of organizational mortality," *American Journal of Sociology*, 100(2): 346-380.

Carroll, G.R. (1985), "Concentration and specialization: Dynamics of niche width in populations of organizations," *American Journal of Sociology*, 90(6): 1262-1283.

Dobrev, S.D., T. Kim and G.R. Carroll (2002), "The evolution of organizational niches: U.S. automobile manufacturers, 1885-1981," *Administrative Science Quarterly*, 47(2): 233-264. [Also listed below under "industry segments and submarkets."]

Industry evolution and agglomeration:

Klepper, S. (2007), "Disagreements, spinoffs, and the evolution of Detroit as the capital of the U.S. automobile industry," *Management Science*, 53: 616-631.

De novo and de alio firms (a.k.a., new entry and diversifying entry):

Khessina, O.M. and G.R. Carroll (2008), "Product demography of de nove and de alio firms in the optical disk drive industry, 1983-1999," *Organization Science*, 19(1): 25-38.

Klepper, S. and K.L. Simons (2000), "Dominance by birthright: Entry of prior radio producers and competitive ramifications in the U.S. television receiver industry," *Strategic Management Journal*, 21(10-11): 997-1016.

McKendrick, D.G., J. Jaffee, G.R. Carroll and O.M. Khessina (2003), "In the bud? Disk array producers as a (possibly) emergent organizational form," *Administrative Science Quarterly*, 48(1): 60-93.

Industry evolution and innovation:

- Abernathy, W.R. and J.M. Utterback (1978), "Patterns of innovation in industry," *Technology Review*, 80(7): 40-47.
- Agarwal, R. and D.B. Audretsch (2001), "Does entry size matter? The impact of the life cycle and technology on firm survival," *Journal of Industrial Economics*, 49(1): 21-43.
- Agarwal, R. and B. Bayus (2002), "The market evolution and take-off of product innovations," *Management Science*, 48: 1024-1041.
- Anderson, P. and M.L. Tushman (1990), "Technological discontinuities and dominant designs: A cyclical model of technological change," *Administrative Science Quarterly*, 35(4): 604-633.
- McGahan, A.M. and B.S. Silverman (2002), "How does innovative activity change as industries mature?," *International Journal of Industrial Organization*, 19(7): 1141-1160.
- Tushman, M.E. and P. Anderson (1986), "Technological discontinuities and organizational environments," *Administrative Science Quarterly*, 31(3): 439-465.

Industry segments and submarkets:

- Dobrev, S.D., T. Kim and G.R. Carroll (2002), "The evolution of organizational niches: U.S. automobile manufacturers, 1885-1981," *Administrative Science Quarterly*, 47(2): 233-264. [Also listed below under "sociological approaches to industry evolution."]
- King, A.A. and C. Tucci (2002), "Incumbent entry into new market niches: The role of experience and managerial choice in the creation of dynamic capabilities," *Management Science*, 48(2): 171-186.
- Klepper, S. and P. Thompson (2006), "Submarkets and the evolution of market structure," *Rand Journal of Economics*, 37(4): 861-886.
- Mitchell, W. (1989), "Whether and when? Probability and timing of incumbents' entry into emerging industrial subfields," *Administrative Science Quarterly*, 34:208-230.

Evolutionary theory writ large (see footnote 14 in the commentary):

- Langlois, R.N. (1992), "External economies and economic progress: The case of the microcomputer industry," *Business History Review*, 66(1): 1-50.

Langlois, R.N. and M.J. Everett (1994), "What is evolutionary economics," chapter 2 in L. Magnusson, editor, *Evolutionary and Neo-Schumpeterian Approaches to Economics*, Dordrecht: Kluwer Academic Publishing.

Murmann, J.P. (2003), *Knowledge and Competitive Advantage: The Coevolution of Firms, Technology, and National Institutions*, Cambridge UK: Cambridge University Press.

Nelson, R.R. and S.G. Winter (1982), *An Evolutionary Theory of Economic Change*, Cambridge, Mass.: Belknap Press of Harvard University Press.

Silverberg, G., G. Dosi and L. Orsenigo (1988), "Innovation, diversity and diffusion: A self-organisation model," *Economic Journal*, 98(393): 1032-1054.

Commentary

This reading list explores industry evolution, with a particular focus on its relationship to innovation. Industry evolution refers to cumulative change in industry characteristics, notably the processes of firm entry, exit, and growth. Models that explore such processes generally exhibit three features: 1) an emphasis on industry change as a dynamic process, 2) an embrace of path-dependence, and 3) a search for empirical regularities and irregularities across industries. Industry evolution is of particular interest to students of firm strategy because anticipating and exploiting environmental change is one of the most fundamental tasks that managers face, and the literature on industry evolution provides insights into the interdependencies among industry change, a firm's strategic choices, and changes in the basis of competitive advantage.

Early research on industry evolution focused on developing theory that could explain key empirical regularities noted in life spans of industries, notably related to entry rates, exit rates, firm growth and survival, market demand, and the nature of technological innovation. Recent research has increasingly emphasized heterogeneity among firms (that is, *which* firms survive and generate certain types of innovation) and heterogeneity within and among industries (that is, what industry characteristics, such as the existence of industry segments, change the nature of competition within an industry). Recent research has also used industry evolution models as theoretical and empirical baselines for exploring related issues such as diffusion of knowledge developed by failed firms, innovation-based competition between incumbents and entrants, and the effect of institutional change on industry development.

The seven core readings listed above provide a solid introduction to theoretical underpinnings and recent developments in the literature on industry evolution and firm strategy.²² This commentary traces the development of this literature. The optional readings in the annotated list following the commentary provide additional background and further development of core ideas for the interested reader. Articles referred to in the commentary that appear in the supplementary readings list are marked with an asterisk (*).

Much of the modern field of industry evolution stems from the concept of the industry life cycle, in which industries are conceived as progressing

²² These readings do not do justice to the broader literature on evolutionary theories of economic and organizational phenomena. The supplemental list appended to this commentary includes a handful of readings that would likely be considered core readings for that literature, most notably Nelson & Winter (1982).

through several stages: emergence (with the introduction of the first relevant product or service), growth (during which the industry experiences a wave of entry and sharply increasing demand), maturity/shakeout (characterized by more stable demand and a wave of net exit), and decline (characterized by stagnant or declining demand and a stable set of firms). Although this model seems to have first been articulated to help stock market investors make investment decisions (Grodinger, 1953), it quickly became a staple of strategy scholarship and managerial prescription (e.g., Levitt, 1965). Much of the research in industry evolution has focused on the development of theoretical explanations for the stylized facts associated with the industry life cycle. One impressive feature of this effort has been the progression from models in which industry change is exogenously imposed to those that grapple with endogenously driven change.

Core reading #1, Gort & Klepper (1982), provides a clear and accessible review of five competing theories of industrial development and their implications for net entry and exit rates, output growth, and technological change in an industry over time. Using data on 46 industries, Gort & Klepper develop several stylized facts regarding entry, exit, growth, and innovation, and run a “horse race” among these theories to determine which can best explain the stylized facts. The article is a valuable demonstration of how to derive clear, testable implications from theory, how to creatively and painstakingly collect data that will support a clear empirical test, and how to match theory to real-world stylized facts. With 25 years of hindsight, however, one striking feature of the model is its reliance on exogenous events to change key parameters. For example, although the specified entry function relies heavily on the nature of innovation in an industry, the model is silent on what triggers a change from “major” to “minor” innovation. Hence, although there is some path-dependence in the model, not all key parameters evolve in a historically dependent fashion.

Several scholars tackled the challenge of developing a fully endogenous model of industry evolution. In core reading #2, Klepper (1996) develops a formal model that has been widely influential in strategy research. In this model, firms with stochastically different innovative ability enter an industry at different times. Those that develop appealing product or process innovations grow, subject to adjustment costs. Firms become more productive over time thanks to process innovations. As each firm makes decisions about R&D investment and output, three key events occur: 1) entry slows as productivity improvements among incumbents raise the bar for profitable entry; 2) exit increases as increased competition ultimately drives out less competitive firms – i.e., those that are smaller (and hence

lack scale economies) or that are less capable at R&D; and 3) increasingly large incumbent firms increasingly shift R&D investment from product innovation to process innovation, as the returns to process innovation are increasing in firm output. The resulting model explains six stylized facts about industry evolution and offers a parsimonious story explaining how the interaction of profit-maximizing firms leads systematically to shakeouts, an emphasis on process innovation, the long-term survival of early entrants, and other features.

Other approaches to the development of such models of industry evolution include Jovanovic (1982*) who develops a model in which firms learn their efficiency only after entering an industry: those firms that are particularly efficient grow quickly, while those that are inefficient exit. This model can explain the simultaneous high likelihood of failure and of rapid growth for small firms vs. large firms, as well as explain the relationship between industry concentration and industry-wide rate of return. Alternatively, Jovanovic & MacDonald (1994*) explain shakeouts as a consequence of a major innovation that increases the minimum efficient scale of production; early adopters grow rapidly, forcing out laggards. Related natural-language models that emphasize the shift in type of innovation within an industry include Abernathy & Utterback (1978*) and Anderson & Tushman (1990*).

Although much of the early work on industry evolution was rooted in economics, sociological approaches to the topic flourished beginning in the late 1970s. Building on the work of Hannan & Freeman (1977; 1984), whose theory of structural inertia emphasized the obstacles to change encountered by organizations, organizational ecologists emphasized the influence of selection (that is, changes in the composition of firms in an industry) over adaptation (that is, changes in existing firms' activities) in changing the nature of an industry. The primary distinction of the organizational ecology approach centered on the role of "legitimation," the development of a taken-for-grantedness for a product or organizational form. The addition of a firm to a population or industry increases both competitive intensity and the industry's legitimacy. When population "density" (i.e., number of firms) is low, the legitimation effect swamps the competitive effect. However, the second derivative of legitimation with respect to population density is negative while that of competition is positive; thus, above some population level, competitive effects swamp legitimation effects, leading to net negative entry. Thus, population ecology offers an alternate explanation for certain stylized facts of industry evolution. Core reading #3, Hannan et al. (1995),

provides a clear articulation of density dependence and extends the original theory to consider whether it works at different geographic levels.

Alternate sociological approaches to industry evolution include resource partitioning (Carroll, 1985*; for refinements see Dobrev, Kim & Carroll 2002*) and niche-overlap (Baum & Singh 1994a), both of which focus on competition among organizations that rely on different sets of resources; as well as institutional ecology, which looks at the role of institutional pressures on founding and failure rates (Baum & Oliver 1996*).

Over the last decade, strategy scholars have devoted increasing attention to firm heterogeneity – which firms are likely to enter, survive, and thrive in an industry, which are at greatest risk of exit, and what contingent characteristics affect this? One stream of research has investigated the role of pre-entry experience as a predictor of post-entry success. Several studies have found that, compared to de novo firms, diversifying entrants have greater success rates in terms of survival (Klepper & Simons 2000*), even if their products are technically inferior to those of new entrants (Khessina & Carroll 2008*). Diversifying entrants also play a larger role in legitimizing the industry (McKendrick et al. 2003). Core reading #4, Thompson (2007), distinguishes between different types of pre-entry experience to identify which offers the most valuable benefits in a new industry. Of particular interest, Thompson finds that two key stylized facts – that older and larger firms have superior survival rates than younger and smaller firms – disappear after controlling for pre-entry experience.²³

Relatedly, core reading #5, Tripsas (1997), explores the competition between industry incumbents and entrants after a major innovation upsets an industry. Integrating the literature on competence-destroying innovation (Tushman & Anderson 1986) with that on appropriating the returns to innovation (Teece 1986), Tripsas proposes that the post-technological-shock balance of power between incumbent and entrant turns on the distribution of ownership over scarce complementary assets. If incumbents continue to own key assets that retain their importance post-innovation, then the incumbents will continue to dominate the industry. Conversely, if the assets are devalued, then the incumbents will be overwhelmed by the entrants. Tripsas tests her predictions in a study of the typesetting industry, which experienced three major innovations during the century that she studies. Not only does this article demonstrate a valuable theoretical integration across literatures, but it also demonstrates the use of qualitative and quantitative data to convincingly demonstrate results.

²³ The exploration of pre-entry experience links the industry evolution literature to that of diversification.

Also over the last decade, strategy scholars have devoted increasing attention to heterogeneity of the landscape within an industry. Whereas most of the early economic research assumed that all firms compete equally within an industry – that is, an industry is a homogenous-good market – a stream of literature has emerged that focuses on the importance of segments or niches within an industry. Core reading #6, de Figueiredo & Silverman (2007), develops a natural language model of competitive dynamics with both firm heterogeneity and industry segmentation. This model makes several predictions about the pattern of expansion by (dominant) incumbent firms across segments and about the way that competition plays out at the segment level. For studies exhibiting a more sociological approach – or, depending on your level of cynicism, reflecting less self-serving bias by the producer of this commentary – see Dobrev et al's (2002*) study of niche evolution or Mitchell's (1989) study of entry by incumbents into industry subfields. Related articles include King & Tucci's (2002*) study of entry into new generations of a product, and Klepper & Thompson's (2006*) model of industry dynamics in “subsectors” that appear and expire randomly.

The above literature frequently discusses exit rates from an industry, but what actually happens to a firm's assets after it exits? An exiting firm leaves behind various resources that might live on in an industry – former employees, equipment, industry-specific knowledge, and the like. Economists have generally been silent on this issue. Organizational ecologists have studied whether prior exits influence current entry, presumably by releasing useful resources, with mixed results. Core reading #7, Hoetker & Agarwal (2007), extends this by focusing on the diffusion of a particular asset – technological knowledge – following the exit of firms from the disk drive industry. They find evidence that a living, functioning firm enhances the ability of other firms to access knowledge.²⁴ More important, this article demonstrates how to use industry evolution concepts to explore strategic questions in related fields such as diffusion of technological knowledge.

²⁴ This is also relevant to the literature that explores how institutions influence the flow of scientific and technological knowledge (e.g., Furman & Stern 2006).

References in the commentary that are not included in the supplemental reading list:

Furman, J.L. and S. Stern (2006), "Climbing atop the shoulders of giants: The impact of institutions on cumulative research," NBER Working Paper #12523.

Grodinsky, J. (1953), *Investments*, New York: Ronald Press Company.

Hannan, M.T. and J. Freeman (1977), "The population ecology of organizations," *American Journal of Sociology*, 82(5): 929-964.

Hannan, M.T. and J. Freeman (1984), "Structural inertia and organizational change," *American Sociological Review*, 49: 149 - 164.

Levitt, T. (1965), "Exploit the product life cycle," *Harvard Business Review*, 43(Nov-Dec): 81-94.

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IV. External Influences

The set of external influences impacting firm strategy and performance are numerous and diverse. The reading lists in this section reflect that diversity. The first, ***External Stakeholder Influence***, by Witold Henisz takes a broad sweep across these issues looking at how various stakeholders in the institutional environment influence the organization and performance of individual firms. Henisz suggests that the theoretical modeling and empirical analysis of this strategic game of external stakeholder influence is among the most nascent domains in the field of strategy but also, arguably, among the domains with highest potential payoff. Henisz traces the practical origins of this field of study and examines its' roots in international business, political economy, sociology and strategic communication and highlights recent and ongoing efforts to integrate these diverse perspectives to develop a "strategic decisionmaking process for influence strategy."

In ***Public and Private Political Strategy***, John De Figueiredo examines some of the same external stakeholder influence activities, but places it in the context of the literature on "non-market strategy," whereby firms attempt to gain or protect competitive advantage through influencing government policy or interacting with other non-government actors such as the media and activists, domestically or in the international arena.

Juan Alcacer and Joanne Oxley take on a more narrowly focused set of issues in ***Geography and Agglomeration***, exploring the dynamic process underpinning the observed phenomenon that economic activity tends to concentrate geographically, generating localized industry "clusters." They describe how strategy researchers have built on fundamental insights from economic geography to develop models that recognize heterogeneity in firms' incentives to agglomerate, and to explore how organizational and social structures may undermine or substitute for agglomeration benefits based on firm location. Reflecting the importance of agglomeration in knowledge-intensive industries, important links can be drawn with themes in ***Innovation*** and ***Organizational Learning***; As a dynamic process, agglomeration is also closely related to issues of ***Industry Evolution*** and ***Sustainability***.

External Stakeholder Influence

Contributed by Witold Henisz

Required Readings

Bernays, Edward L. (1947) "The Engineering of Consent." *Annals of the American Academy of Political and Social Science*, Vol. 250, Communication and Social Action pp. 113-120.

Moran, Theodore 1973. "Transnational Strategies of Protection and Defense by Multinational Corporations: Spreading the Risk and Raising the Cost for Nationalization in Natural Resources," *International Organization* **27**(2):273-287.

Putnam, Robert D. 1988. "Diplomacy and domestic politics: The logic of two-level games." *International Organization* **42**:427-460.

*Rowley, Timothy J. (1997). Moving beyond dyadic ties: A network theory of stakeholder influences. *Academy of Management Review*, 22(4), 887-910.

McAdam, Doug (2009). "Social Movements and the Growth in Opposition to Global Projects."

Sebenius, James K. 1992 "Negotiations Analysis: A Characterization and Review" *Management Science* **38**(1):18-38.

Henisz, Witold J (2009). "Preferences, Structure, Influence: The Engineering of Consent."

Supplementary Readings

The Socio-Political Challenge Faced by (Multinational) Investors and the Limitations of a Purely Market-based Strategy

Vernon, Raymond. 1977. "Storm Over the Multinationals: Problems and Prospects", *Foreign Affairs*, 55(3):243-262.

Kobrin, S. J. 1987. Testing the Bargaining Hypothesis in the Manufacturing Sector in Developing Countries. *International Organization*, 41(1): 609-638.

*Woodhouse, Erik J. 2006. "The Obsolescing Bargain Redux? Foreign Investment in the Electric Power Sector in Developing Countries" *International Law and Politics* 38:121-219.

*Orr, R. and Scott, W.R. "Institutional Exceptions on Global Projects: A Process Model", *Journal of International Business Studies*, 2008

Zelner, Bennet, Witold Henisz and Guy Holburn. 2009. "Contested Implementation and Retrenchment of Neoliberal Reforms: The Case of the Global Private Power Industry, 1989-2001"

Henisz, Witold J., Levitt Raymond E. and Dan Settel 2009. "Socio-Political Governance Mechanisms"

Lessons and Insights from Economic Development and Economic History

North, D. C. (1991) "Institutions." *Journal of Economic Perspectives*, 5: 97-112.

Black, Bernard, Reinier Kraakman and Anna Tarassova 2000. "Russian Privatization and Corporate Governance: What Went Wrong?" *Stanford Law Review* 52(6): 1731-1808.

Rajan, Raghuram G. and Luigi Zingales 2003. "The Great Reversals: The Politics of Financial Development in the Twentieth Century" *Journal of Financial Economics* 69(1):5-50.

Kogut, Bruce & Andrew Spicer. 2004. *Critical and Alternative Perspectives on International Assistance to Post-Communist Countries: A Review and an Analysis*: World Bank.

Ellerman, David. 2005. *Helping People Help Themselves*. (Ann Arbor: University of Michigan Press) pp. 1-24 & 240-253.

Root, Hilton. 2006. *Capital and Collusion: The Political Logic of Global Economic Development* pp. 1-56 & 221-245

Tools and Insights from Political Economy

Bueno de Mesquita, Bruce. 2002. *Predicting Politics* (Columbus: Ohio State University Press)

Walker, Jack L. Jr. 1991. *Mobilizing Interest Groups in America: Patrons, Professions and Social Movements* (Ann Arbor: University of Michigan Press)

Jones, Bryan D. and Frank R. Baumgartner 2005. *The Politics of Attention: How Government Prioritizes Problems* (Chicago: University of Chicago Press)

Tools and Insights from Sociology

Georg Simmel, "Triad," in Kurt H. Wolff (Ed. and Trans.), *The sociology of Georg Simmel* (pp. 145-169). New York: Free Press. (Originalwork published in 1908).

Knoke, D. (1990). *Political Networks: The Structural Perspective*. Cambridge, UK, Cambridge University Press.

Keck, Margaret E. and Kathryn Sikkink 1999. "Transnational Advocacy Networks in International and Regional Politics." *International Social Science Journal* **51**(159):89-101.

Tools and Insights from Strategic Communications and Negotiations

Watkins, Michael T. 2001 "Principles of Persuasion" *Negotiation Journal* **17**(2):115-137.

Elsbach, Kimberly D. 2006. *Organizational Perception Management* (Mahwah NJ, Lawrence Erlbaum Associates)

The Debate Regarding the Financial Case for and Practice of Corporate Social Responsibility and External Stakeholder Engagement

Baron, David. 2001. "Private Politics, Corporate Social Responsibility, and Integrated Strategy," *Journal of Economics & Management Strategy* 10(1): 7-45.

Vogel, David. 2006. *The Market for Virtue: The Potential and Limits of Corporate Social Responsibility*. Washington, D.C.: Brookings Institution Press. (Chapter 1).

Elkington, John, Emerson, Jed and Below, Seb. (2006) "The Value Palette: A Tool for Full Spectrum Strategy" *California Management Review* 48(2): 6-28.

Skim/Review Stakeholder Engagement Manuals & Materials
Anglo American:

<http://www.angloamerican.co.uk/aa/development/society/engagement/seat>

International Finance Corporation:

[http://www.ifc.org/ifcext/enviro.nsf/AttachmentsByTitle/p_StakeholderEngagement_Full/\\$FILE/IFC_StakeholderEngagement.pdf](http://www.ifc.org/ifcext/enviro.nsf/AttachmentsByTitle/p_StakeholderEngagement_Full/$FILE/IFC_StakeholderEngagement.pdf)

UNEP/Accountability:

<http://www.accountability21.net/publications.aspx?id=904>

Brief Commentary

The realization of the economic value from a transaction often depends critically upon the institutional environment in which it is embedded. The institutional environment may pose hazards to value generation in the form of a risk of expropriation by a political actor or by a private actor using political influence. Alternatively, changes in the current regulatory or policy environment may alter the returns to a transaction. Such changes may either enhance the value of a transaction with positive economic returns or transform a transaction with negative economic returns to one with positive returns. Any of these actions may be motivated by political actors seeking to use their discretionary power to generate private gains for themselves or their constituents, to sustain their power or that of their allies, or to maximize social welfare. The behavior of political actors, their allies and constituents is influenced by the provision of information, ideological beliefs or by the (in)direct transfer of goods, services or cash.

Actors are connected via myriad economic, political and social ties. Those ties alter their beliefs and behavior. Modeling the policymaking process requires the incorporation of these network ties among a diverse set of external stakeholders into the design and implementation of their influence strategies. The network of relevant stakeholders encompasses the entire political, economic and social value chain for a given issue: producers (including producers of substitutes and complements), suppliers (including providers of finance and labor), buyers (including consumers), voters as well as those directly impacted or having preferences over the distribution of economic returns or externalities including but not limited to the media and activists.

The theoretical modeling and empirical analysis of this strategic game of external stakeholder influence is among the most nascent domains in the field of strategy but also, arguably, among the domains with highest potential payoff. The institutional environment is too often modeled as an exogenous parameter or, in the non-market strategy literature²⁵, simplified down to a carefully delimited set of lobbying or informational interactions among actors in a particular context. Given the nascent status of the research domain, the reader focuses more on the necessary building blocks or foundation of a potentially successful research program rather than on the evolution of the domain to date. These building blocks are found in diverse social science disciplines including economic and financial history, law, international business, political economy, world polity, development,

²⁵ For more depth on this topic see the survey by De Figueiredo.

social movement theory, sociology, communications, psychology and corporate social responsibility.

The core questions and challenges for researchers are:

- (1) What strategic actions by a focal firm can minimize the downside risk of expropriation posed by political actors or by private actors using political influence?
- (2) How does one surmount the inherent data hurdles both in capturing an organization's influence strategy and linking it to performance particularly in the international context?
- (3) How should researchers balance the insight gained into particular mechanisms and tactics available through deep dives into a specific context against the loss of generalizability?

Practical Origins. Edward Bernays was among the earliest, most influential and most prolific scholars and practitioners of influence strategies. Bernays (1947) offers insights into his repertoire that he claimed allowed him and other practitioners to “engineer consent” among the public in the interests of his clients. Bernays shaped the perception of key external stakeholders transforming the apathetic or uninterested into powerful allies of his client. He tapped into not only economic interests but also, and more commonly, emotional levers of aspiration and fear to promote his preferred outcome often relying on supposedly independent experts and staged media events to raise awareness and alter opinion. While Bernays was an exemplary practitioner and author, the tactics he used were not original but rather drew upon the writings of his uncle Sigmund Freud and many other studies of human motivation and action. Any attempt to formalize or codify influence strategies should thus also draw insight the multiple domains in which it has long been and will continue to be deployed.

International Business. As international business is defined by the extension of an economic transaction across a political frontier, political risk has long been a central concern among scholars in the field. While the vast majority of the political risk management literature has emphasized the strategies of avoidance or adaptation as responses to political risk, a few classic pieces of scholarship and some more recent ones emphasize strategies that firms can take to influence the outcome of policymaking processes. Moran's (1973) classic comparative case study of Kenecott and Anaconda Copper's sensitivity to the expropriation risk posed by the Chilean government in the 1960s emphasizes the formation and deployment of a diverse network of

external stakeholders including providers of capital to the project and to the government that guarantees it with whom Anaconda crafted an alignment of interests. As a result of this concerted effort to construct a strong network of advocates to whose concerns the Chilean government was sensitive, Anaconda better weathered the wave of nationalizations than its peers.

Political Economy. A deep understanding of the context and process of policymaking forms an important component of any effort to model external stakeholder influence. Putnam's (1988) analysis of two sided negotiations highlights many of the key inputs from this literature into such a model including multi-party negotiations among principals who vary in their power and credibility representing agents who vary in the homogeneity of their preferences. As a result of these and other characteristics of the players in the game and the strategies that they employ, the range of possible bargaining outcomes varies widely.

Sociology. Models of political economy typically treat actors as rational autonomous utility maximizing actors. Yet, the connections among these actors clearly influence their beliefs and behavior. Furthermore, powerful ideas and frames or events can create cascade effects particularly under the right political opportunity structure which seem to invoke mechanisms beyond the realm of political economy. For deeper insights into these two necessary extensions, we turn to the discipline of sociology. Rowley (1997) applies network tools and metrics to extend extant stakeholder theory. The article both serves as a useful introduction into the stakeholder perspective and the implications and methods for the incorporation of networks of policymakers into a model of strategic influence. Rowley introduces and applies such constructs as network density and centrality to help predict the form of interaction between a focal firm and its external stakeholders. Another vein of sociology of relevance to external stakeholder influence strategy focuses not just on the power of structure that links political actors but rather the impact of mobilizing structures and cognitive frames within a political opportunity structure. McAdam (2009) offers a rich summary of the social movement literature specifically designed to inform the understanding of political and social resistance to large global investment projects.

Strategic Communications and Negotiations. While Political Economy and Sociology offer the theoretical tools to depict the policymaking process, they are less forceful or at least less rich in the depiction of strategic behavior than the reality suggested by the process oriented studies in international business or policy. For more insight into the strategies of

persuasion, we turn to the literature on strategic communications and negotiation. Sebenius (1992) offers a summary of the key insights from the negotiations literature

Integration. Henisz (2009) seeks to incorporate the insights from the political economy, sociology and strategic communications and negotiations literatures into a strategic decisionmaking process for influence strategy that incorporates information on the preferences, power, issue salience and beliefs of individual actors as well as the strength of ties between them to identify the efficacy of various influence strategies at their disposal using simulation techniques.

Conclusion. The literature on external stakeholder influence introduced here is interdisciplinary and as of yet lacking in both rigorous formal theoretical development and empirical analysis. Yet, as we survey the drivers of organizational performance heterogeneity, particularly across international markets or in times of domestic political and economic upheaval such as the global crisis of 2009, the ability to strategically “change states of mind” or generate “a feeling of personal comfort in social relations that is sometimes called solidarity” (Barnard 1968: 148) is clearly among the key drivers of performance. We hope that the introduction offered to the literature here can motivate subsequent development by scholars in or entering the field of strategy.

Full Commentary

Introduction

The realization of the economic value from a transaction often depends critically upon the institutional environment in which it is embedded. The institutional environment may pose hazards to value generation in the form of a risk of expropriation by a political actor or by a private actor using political influence. Alternatively, changes in the current regulatory or policy environment may alter the returns to a transaction. Such changes may either enhance the value of a transaction with positive economic returns or transform a transaction with negative economic returns to one with positive returns. Any of these actions may be motivated by political actors seeking to use their discretionary power to generate private gains for themselves or their constituents, to sustain their power or that of their allies, or to maximize social welfare. The behavior of political actors, their allies and constituents is influenced by the provision of information, ideological beliefs or by the (in)direct transfer of goods, services or cash.

Actors are connected via myriad economic, political and social ties. Those ties alter their beliefs and behavior. Modeling the policymaking process requires the incorporation of these network ties among a diverse set of external stakeholders into the design and implementation of their influence strategies. The network of relevant stakeholders encompasses the entire political, economic and social value chain for a given issue: producers (including producers of substitutes and complements), suppliers (including providers of finance and labor), buyers (including consumers), voters as well as those directly impacted or having preferences over the distribution of economic returns or externalities including but not limited to the media and activists.

The theoretical modeling and empirical analysis of this strategic game of external stakeholder influence is among the most nascent domains in the field of strategy but also, arguably, among the domains with highest potential payoff. The institutional environment is too often modeled as an exogenous parameter or, in the non-market strategy literature²⁶, simplified down to a carefully delimited set of lobbying or informational interactions among actors in a particular context. Given the nascent status of the research domain, the reader focuses more on the necessary building blocks or foundation of a potentially successful research program rather than on

²⁶ For more depth on this topic see the survey by De Figueiredo.

the evolution of the domain to date. These building blocks are found in diverse social science disciplines including economic and financial history, law, international business, political economy, world polity, development, social movement theory, sociology, communications, psychology and corporate social responsibility.

The core questions and challenges for researchers are:

- (4) What strategic actions by a focal firm can minimize the downside risk of expropriation posed by political actors or by private actors using political influence?
- (5) How does one surmount the inherent data hurdles both in capturing an organization's influence strategy and linking it to performance particularly in the international context?
- (6) How should researchers balance the insight gained into particular mechanisms and tactics available through deep dives into a specific context against the loss of generalizability?

An Iconic and Provocative Figure

Edward Bernays was among the earliest, most influential and most prolific scholars and practitioners of influence strategies. His career began promoting unheralded artistic causes including off-Broadway plays on sexually transmitted diseases, Italian opera singers and Baltic Symphony orchestras. His success in the artistic audience won him wide acclaim and he successfully used his network of influence in New York to secure a position within the US government's propaganda office during World War I. There he rose to a position of leadership spearheading the million man marches in Europe that lauded President Wilson. He turned next to the private sector championing shareholder interests of companies selling cigarettes, automobiles and breakfasts of eggs & bacon. He also maintained his ties to government devising the duck and cover drills to build support for the Cold War defense build-up. Occasionally, he could serve both clients at once as was the case in his infamous work for The United Fruit Company and the CIA in Guatemala where he helped to organize a coup against the Guatemalan government sponsoring land reform inimical to United Fruit's interests and Eisenhower's ideology. Bernays (1947) offers insights into his repertoire that he claimed allowed him and other practitioners to "engineer consent" among the public in the interests of his clients.

Bernays shaped the perception of key external stakeholders transforming the apathetic or uninterested into powerful allies of his client. He tapped into not only economic interests but also, and more commonly, emotional levers of aspiration and fear to promote his preferred outcome often relying on supposedly independent experts and staged media events to raise awareness and alter opinion. Among his most famous exploits was the tripling of cigarette smoking by women. At an appointed hour, on a pre-ordained street corner before photographers from all major media outlets who had been summoned by their trusted confidante, a group of women planted in suffragist parade drew “Torches of Freedom” out of their garters and lit them in an effort to draw attention to the desire for equality: the right to vote and the right to smoke.

While Bernays was an exemplary practitioner and author, the tactics he used were not original but rather drew upon the writings of his uncle Sigmund Freud and many other studies of human motivation and action. Any attempt to formalize or codify influence strategies should thus also draw insight the multiple domains in which it has long been and will continue to be deployed.

International Business

As international business is defined by the extension of an economic transaction across a political frontier, political risk has long been a central concern among scholars in the field. While the vast majority of the political risk management literature has emphasized the strategies of avoidance or adaptation as responses to political risk, a few classic pieces of scholarship and some more recent ones emphasize strategies that firms can take to influence the outcome of policymaking processes.

Moran’s (1973) classic comparative case study of Kenecott and Anaconda Copper’s sensitivity to the expropriation risk posed by the Chilean government in the 1960s emphasizes the formation and deployment of a diverse network of external stakeholders including providers of capital to the project and to the government that guarantees it with whom Anaconda crafted an alignment of interests. As a result of this concerted effort to construct a strong network of advocates to whose concerns the Chilean government was sensitive, Anaconda better weathered the wave of nationalizations than its peers. Other classic international business contributions include Raymond Vernon’s multiple texts beginning with *Sovereignty at Bay* and culminating with *In the Eye of the Hurricane: The Storm over Multinationals*. Vernon’s (1977) *Foreign Affairs* article offers a timeless introduction into his perspective on the complex pressures from host

and home country governments shaped by competing ideologies and interests that define the strategic challenge for multinational corporations.

The bargaining power model of international business has sought to formalize and codify this process with some empirical success as demonstrated by Kobrin (1987) who predicts the outcome of a bargaining process over the equity share of a host country subsidiary between the host country government and a multinational firm. Over the subsequent twenty years, the political risk was, however, largely moribund as the neoliberal wave of economic reform shifted attention to the realization of economic opportunities and away from political complications.²⁷

The East Asian (or IMF) crisis offered an important stimulus for reevaluation by practitioners and scholars alike. Woodhouse (2006) examines the boom and bust of private investment in independent power projects emphasizing the limits of a contractual response to political risk. Orr and Scott (2008) draw insights from a wider array of global projects to develop a process model of political and social risk management. Hennis, Zelner and Holburn (2009) quantitatively analyze the determinants of renegotiation in a large sample of power projects developing and finding empirical support for a recursive model of policy change that combines cognitive, political and economic factors at the state and state-system level to predict the incidence of renegotiations in a given country-year.

²⁷ Recent overviews of two additional related literatures are provided in the supplementary reading list.

(1) The international development literature has long sought to incorporate political and social factors in the process of economic growth. Nobel-prize winner Douglass North (1991) has long emphasized the central role played by institutions in the process of economic growth. More recently, scholars have sought to highlight certain pathologies or patterns of dysfunction by which political interests conspire to retard economic growth. Prominent among these is the capturing of the state by a narrow set of economic interests (Black, Kraakman and Tarassova 2000; Rajan & Zingales 2003; Root 2006). More subtle but perhaps more challenging problems are posed by the gaps between our economic models of growth and socio-political processes that underlie them (Kogut & Spicer 2004). Ellerman (2005) offers a process model for economic development that focuses upon instead of ignores these gaps by emphasizing the need for development policy to teach the poor how to fish as opposed to providing fish. The interdisciplinary scope of Ellerman's (2005) text and its process orientation is echoed by the process model of Orr & Scott (2008) in international business as well as the corporate stakeholder engagement manuals introduced at the end of this syllabus.

(2) The literature on corporate social responsibility has similarly struggled with the best means to incorporate non-economic factors into the process of strategy formulation and implementation. Baron (2001) highlights the strategic case for doing so while Vogel (2006) critically reviews the empirical evidence to date. Elkington (2006) provides the perspective of a leading practitioner which is echoed by a review of toolkits used in the development and corporate sectors.

Political Economy

A deep understanding of the context and process of policymaking forms an important component of any effort to model external stakeholder influence. Putnam's (1988) analysis of two sided negotiations highlights many of the key inputs from this literature into such a model including multi-party negotiations among principals who vary in their power and credibility representing agents who vary in the homogeneity of their preferences. As a result of these and other characteristics of the players in the game and the strategies that they employ, the range of possible bargaining outcomes varies widely.

The work of Bruce Bueno de Mesquita summarized in *Predicting Politics* formalizes and extends this bargaining framework. Walker (1991) provides additional contextual detail and insight into the interest groups that form the basis of action in Bueno de Mesquita's model and Jones and Baumgartner (2001) do the same for the process of their interaction to influence government policy. Together these additional supplementary readings deepen the conceptual insights offered by Putnam with additional formal modeling tools and contextual detail that emphasize strategic action by firms in a rich and complex policymaking space encompassing both formal political actors as well as other members of the value chain.

Sociology

Models of political economy make two simplifying assumptions which are clearly contradicted by the deep process oriented studies of Walker and Jones & Baumgartner. Formal models treat actors as rational autonomous utility maximizing actors. Yet, the connections among these actors clearly influence their beliefs and behavior. Furthermore, powerful ideas and frames or events can create cascade effects particularly under the right political opportunity structure which seem to invoke mechanisms beyond the realm of political economy. For deeper insights into these two necessary extensions, we turn to the discipline of sociology.

Rowley (1997) applies network tools and metrics to extend extant stakeholder theory. The article both serves as a useful introduction into the stakeholder perspective and the implications and methods for the incorporation of networks of policymakers into a model of strategic influence. Rowley introduces and applies such constructs as network density and centrality to help predict the form of interaction between a focal firm and its external stakeholders.

Another vein of sociology of relevance to external stakeholder influence strategy focuses not just on the power of structure that links political actors but rather the impact of mobilizing structures and cognitive frames within a political opportunity structure. McAdam (2009) offers a rich summary of the social movement literature specifically designed to inform the understanding of political and social resistance to large global investment projects.

Supplementary readings build upon these core concepts by providing the sociological analogue to the argument of Putnam (1988) from political science in the writings of George Simmel on the *Triad*; demonstrating the modeling the policymaking process as a network (Knoke, 1990) and applying such a network perspective infused with powerful cognitive frames to the understanding of the behavior of non-governmental activists (Keck and Sikkink, 1990).

Strategic Communications and Negotiations

While Political Economy and Sociology offer the theoretical tools to depict the policymaking process, they are less forceful or at least less rich in the depiction of strategic behavior than the reality suggested by the process oriented studies in international business or policy. For more insight into the strategies of persuasion, we turn to the literature on strategic communications and negotiation. Sebenius (1992) offers a summary of the key insights from the negotiations literature which are further extended by the more recent managerially-oriented supplementary article by Watkins (2001). Elsbach (2006) offers a similarly broad and thorough summary of the literature on organizational perception management including the shaping of organizational identity and the maintenance of reputation in the face of crisis or adversity.

Integration

Henisz (2009) seeks to incorporate the insights from the political economy, sociology and strategic communications and negotiations literatures into a strategic decisionmaking process for influence strategy that incorporates information on the preferences, power, issue salience and beliefs of individual actors as well as the strength of ties between them to identify the efficacy of various influence strategies at their disposal using simulation techniques.

Conclusion

The literature on external stakeholder influence introduced here is interdisciplinary and as of yet lacking in both rigorous formal theoretical development and empirical analysis. Yet, as we survey the drivers of organizational performance heterogeneity, particularly across international markets or in times of domestic political and economic upheaval such as the global crisis of 2009, the ability to strategically “change states of mind” or generate “a feeling of personal comfort in social relations that is sometimes called solidarity” (Barnard 1968: 148) is clearly among the key drivers of performance. We hope that the introduction offered to the literature here can motivate subsequent development by scholars in or entering the field of strategy.

Public and Private Political Strategy

Contributed by John M. de Figueiredo

Core Readings

1. Public Politics

Baron, David P. 1999. "Integrated Market and Nonmarket Strategies in Client and Interest Group Politics," *Business and Politics* 1(1): 1-31.

Ansolabehere, Stephen, John M. de Figueiredo, and James M. Snyder (2003). "Why Is There So Little Money in Politics?" *Journal of Economic Perspectives* 17: 105-130.

2. Private Politics

Baron, David P., and Daniel Diermeier. 2007. "Strategic Activism and Nonmarket Strategy," *Journal of Economics and Management Strategy* 16(3): 599-634.

Eesley, Chuck, and Michael Lenox. 2006. "Firm Responses to Secondary Stakeholder Action." *Strategic Management Journal* 27(8): 765-782

3. International Non-Market Political Strategy

Iaryczower, Matias Pablo T. Spiller and Mariano Tommasi (2006). "Judicial Lobbying: The Politics of Labor Law Constitutional Interpretation." *American Political Science Review*, 100:85-97.

Fisman, Ray. 2001. "Estimating the Value of Political Connections," *American Economic Review* 91: 1095-1102.

Supplementary Readings

I. Overview

Baron, David. (2010). *Business and Its Environment, 6th Ed.* Chapter 1.

This is the standard MBA textbook in the field.

Oberholzer-Gee, Felix, and Dennis Yao (2008). "Integrated Strategies." Harvard Business School Working Paper.

de Figueiredo, John M. (2009). "Integrated Political Strategy," *Advances in Strategic Management*, forthcoming.

II. Public Politics

A. Institutional Detail: Background and Early Papers

Olson Mancur 1965. *The Logic of Collective Action*. (Cambridge: Harvard University Press)

Tsebelis, George. 2002. *Veto Players: How Political Institutions Work*. Princeton: Princeton University Press

Schattschneider, E. E. 1935. *Politics, Pressures and the Tariff*. New York: Prentice-Hall.

Stigler, George (1971). "The Economic Theory of Regulation," *Bell Journal of Economics* 3: 3-18.

Peltzman, Sam (1976). "Toward a More General Theory of Regulation," *Journal of Law and Economics* 19(2): 211-240.

Yoffie, David. 1988. "How an Industry Builds Political Advantage," *Harvard Business Review* (May-June): 82-89.

Spulber, Daniel. 1989. *Regulation and Markets*. Cambridge: MIT Press.

Baron, David P. 1995. "The Non-Market Strategy System," *Sloan Management Review* 37(1): 73-85.

Baron, David P. 1995. "Integrated Strategy: Market and Non-Market Components," *California Management Review*: 47-65

B. Theory

Hillman, Amy J., Gerald D. Keim & Douglas A. Schuler. 2004. "Corporate Political Strategies: A Review and Research Agenda." *Journal of Management* 837-857.

Grossman, Gene M. and Elhanan Helpman 2001. *Special Interest Politics* (Cambridge: MIT Press)

Snyder, James M. (1991). "On Buying Legislators," *Economics and Politics*, July 1991, vol. 3, no. 2, pages 93-109.

C. Empirical

de Figueiredo, John M., and Brian S. Silverman (2006). "Academic Earmarks and the Returns to Lobbying," *Journal of Law and Economics* 49(2): 597-625.

Snyder, James M. (1990). "Campaign Contributions as Investments: The U.S. House of Representatives 1980-1986," *Journal of Political Economy*, December 1990, vol. 98, no. 6, pages 1195-1227.

Stratmann, Thomas (1992). "Are Contributors Rational: Untangling Strategies of Political Action Committees," *Journal of Political Economy*, June 1990.

Bonardi, J. P.; Holburn, G.; Vanden Bergh, R. G. 2006. "Non Market Strategy in Regulated Industries: Theory and Evidence from U.S. Electric Utilities," *Academy of Management Journal* 49(6).

III. Private Politics

A. Theory

Baron, David P., and Daniel Diermeier. 2007. "Strategic Activism and Nonmarket Strategy," *Journal of Economics and Management Strategy* 16(3): 599-634.

Baron, David P. 2005. "Competing for the Public Through the Media," *Journal of Economics and Management Strategy* 14(2): 339-376.

Gilligan, Thomas, and Tim Feddersen (2001). "Saints and Markets: Activists and the Supply of Credence Goods," *Journal of Economics and Management Strategy* 10(1): 149-71.

Simcoe, T.S. (2009). "Standard Setting Committees" Working Paper, University of Toronto.

B. Empirical

Groseclose, Timothy, and Jeff Milyo. 2005. "A Measure of Media Bias," *Quarterly Journal of Economics* 120(4): 1191-1237.

Eric Uhlmann, George Newman, Victoria Brescoll, Adam Galinsky, and Daniel Diermeier. 2008. "[The Sounds of Silence: Effects of an Engaged, Defensive, and No Comment Response to a Crisis on Corporate Reputation.](#)" Northwestern University Working Paper.

IV. International Perspectives

A. Theory

Persson Torsten and Guido Tabellini 2000. *Political Economics: Explaining Economic Policy* (Cambridge: MIT Press)

Acemoglu, Daron and James A. Robinson 2006. *Economic Origins of Dictatorship and Democracy*. New York: Cambridge University Press.

B. Strategic Action by Firms

Levy, Brian and Pablo T. Spiller "The Institutional Foundations of Regulatory Commitment: A Comparative Analysis of Telecommunications Regulation" *Journal of Law, Economics and Organization* 10: 201-246.

Henisz, Witold J. 2000. "The Institutional Environment for Economic Growth." *Economics and Politics* 12(1):1-31.

Tsebelis, George. 1999. "Veto Players and Law Production in Parliamentary Democracies: An Empirical Analysis." *American Political Science Review*. 93(3): 591-608

Bueno de Mesquita, Bruce. 2002. *Predicting Politics* (Columbus: Ohio State University Press)

Commentary

The field of non-market strategy examines how players outside of the market and the firm's value chain affect the performance of the firm. Recent extensions to the field elucidate how firms develop market and non-market strategies together (in integrated strategies) that allow the firms to obtain competitive advantage. The field is structured around two questions. First, how do firms affect non-market institutions and actors. Second, how do these non-market institutions and actors affect markets, competition, competitive advantage, and firm performance. The papers in this syllabus provide an overview of some of the main topics in the field of public and private politics and strategy. The Baron book listed in the extended syllabus overview section provides a synopsis of the field.

Although the importance of government to the business enterprise was recognized decades ago, the research in "business-government relations" focused almost exclusively on how government rules affect business outcomes. Building on the foundational work of Stigler (1971) and Peltzman (1976), the field of non-market strategy began to coalesce around 1990. At that time, a number of papers in strategic management (Yoffie 1988, Spulber 1989, Baron 1993 [first edition], 2010 [sixth edition]) developed conceptual models of how firms affect government policies.

Today the field is largely comprised of three parts: public politics, private politics, and international non-market strategy. Public politics, the most advanced stream of literature, examines how firms gain competitive advantage through influencing government policies. A challenge in this field, however, has been to craft general equilibrium formal models where firms affect policies AND policies affect firm performance. Baron (1999) provides an excellent example of a paper that incorporates firms with differential capabilities competing in the marketplace and these same firms engaging in non-market competition in government institutions using spatial modeling of capabilities and preferences. In particular, it demonstrates which policies firms will prefer and when they will form coalitions. Given the paucity of theoretical papers modeling general equilibrium behavior, this is an excellent path for future research. This said, empirical papers in public politics abound, covering lobbying, campaign contributions, grassroots organization, and strategic facilities location. The challenge many of these studies face, however, is that they suffer from omitted variable bias because of endogenous right hand side variables. One way to solve this problem is with natural experiments. A second way is to use instrumental variables. In examining the effect of campaign contributions on legislator voting behavior, Ansolabehere et al (2003) demonstrate the

problem of not using instruments and show how instrumental variables can change the outcomes of empirical analysis.

The second area of non-market strategy is private politics. Private politics refers to an action by stakeholders (and reactions by firms) who are outside the government, the firm, and the firm's value chain, that attempts to influence the firm's behavior and performance. These actors include activists, the media, standards setting committees, and others—each of which might have a substantial impact on the competitive advantage of the firm. Despite a consensus that private politics can have a significant impact on the firm's performance, this area of non-market strategy is very under-theorized and under-analyzed empirically, creating great opportunities for substantial contribution by researchers. One of the most advanced formal models of activism is found in the Baron and Diermeier (2007). It develops an extended model of activist behavior with testable empirical predictions on the targets the activists will choose and the likely reaction of firms. Empirical work in the field is also in its relative infancy. The Eesley and Lenox (2006) paper illustrates the most advanced empirical test to date on the effect of activism on corporate behavior.

The final area of non-market strategy examines international strategy. Although most work in this area is published in international business, international economics, or comparative politics journals, the non-market strategy approach endogenizes the international political institutions—making policy susceptible to the firms that political agents regulate. The applications in this area, however, are generally lacking formal models or are only partial equilibrium analyses—examining government effects on investment. An exception to this is the Iaryczower et al (2006) paper that examines how groups can affect policy through lobbying the judiciary. This “lobbying” in turn, affects judicial decision-making. The paper presents a formal model of this general equilibrium phenomenon and then tests it examining the labor strikes and judicial decisions in Argentina. The second paper, though not endogenizing institutions, is a seminal paper on political connections—which is an important area of the nexus of firm strategy (and firm governance) and political institutions. Fisman (2001) very creatively measures the value of having politically connected board members on a corporation's board in Indonesia using a natural experiment, the death of the Indonesian leader Suharto.

While no “overview” paper of the field is assigned in the core reading, an instructor might find it very useful to choose one paper from Section I of the Overview section of the extended syllabus to assign to PHD students. While these three papers take very different tacks, they might help pull

together nicely a seemingly disconnected field. I highly recommend that one of these papers be used in addition to the core syllabus.

Overall, these papers are building blocks toward the main goal of the field—developing theories and empirical work that describe successful integrated strategies—strategies that integrate nonmarket and market strategies for the firm to gain sustainable competitive advantage. While progress has been made on some of the pieces, we have yet to reach a comprehensive understanding of integrated strategies.

Geography, Agglomeration, and Firm Strategy

Contributed by Juan Alcacer and Joanne Oxley

Core readings

◆ Denotes papers from Ph.D. dissertation work.

1. Hanson, Gordon H. 2001. *Scale Economies and the Geographic Concentration of Industry*. *Journal of Economic Geography* 1 (3):255–276.
2. Ellison, Glenn, and Edward L. Glaeser. 1997. *Geographic Concentration in U.S. Manufacturing Industries: A Dartboard Approach*. *The Journal of Political Economy*. 105 (5):889–927.
3. Myles J. Shaver and Flyer, Fredrick. 2000. *Agglomeration Economies, Firm Heterogeneity, and Foreign Direct Investment in the United States*. *Strategic Management Journal*. 21:1175-1193.
4. ◆ Alcacer, Juan. 2006. *Location Choices across the Value Chain: How Activity and Capability Influence Collocation*. *Management Science* 52 (10):1457–1471.
5. Sorenson, Olav, and Toby E. Stuart. 2001. *Syndication Networks and the Spatial Distribution of Venture Capital Investments*. *American Journal of Sociology* 106 (6):1546–1588.
6. Agrawal, Ajay, Iain Cockburn and John McHale. 2006. *Gone But Not Forgotten: Knowledge Flows, Labor Mobility, and Enduring Social Relationships*. *Journal of Economic Geography* 6: 571-591.

Supplementary Readings

Theoretical Foundations

1. Marshall, Alfred, 1920. *Principles of Economics*. London: MacMillan and Co.
2. Fischer Jeffrey H., Joseph E. Harrington Jr. 1996. *Product Variety and Firm Agglomeration*. *RAND Journal of Economics*, 27: 281-309.
3. Krugman, Paul. 1998. *Space: The Final Frontier*. *Journal of Economic Perspectives* 12 (2):161–174.

4. Lieberman, Marvin, and Shigeru Asaba. 2006. *Why do Firms Imitate Each Other?* *Academy of Management Review* 31 (2): 366-385.

Advances in Empirical Methods

5. Ellison, Glenn, Edward L. Glaeser, and William Kerr. 2007. *What Causes Industry Agglomeration? Evidence from Co-agglomeration Patterns*. National Bureau of Economic Research Working Paper Series (No. 13068). Available from <http://www.nber.org/papers/w13068>.
6. Head, Keith, John Ries, and Deborah Swenson. 1995. *Agglomeration Benefits and Location Choice: Evidence from Japanese Manufacturing Investments in the United States*. *Journal of International Economics* 38 (3/4):223–247.
7. Davis, Donald R., and David E. Weinstein. 2002. *Bones, Bombs, and Break Points: The Geography of Economic Activity*. *American Economic Review* 92 (5):1269–1289.
8. Dumais, Guy, Glenn Ellison, and Edward L. Glaeser. 2002. *Geographic Concentration as a Dynamic Process*. *Review of Economics & Statistics* 84 (2):193–204.

Firm Heterogeneity and Agglomeration Benefits

9. Flyer, Fredrick, and J. Myles Shaver. 2003. *Location Choices under Agglomeration Externalities and Strategic Interaction*. In *Advances in Strategic Management*, edited by J. A. C. Baum and O. Sorensen. Oxford, UK: JAI Press. pp. 193–213.
10. Pashigian, B. Peter, and Eric D. Gould 1998. *Internalizing Externalities: The Pricing of Space in Shopping Malls*. *Journal of Law and Economics*, 41: 115-42
11. Chung, Wilbur, and Juan Alcacer. 2002. *Knowledge Seeking and Location Choice of Foreign Direct investment in the United States*. *Management Science* 48 (12): 1534-1554.
12. Kalnins, Arturs, and Wilbur Chung. 2004. *Resource-Seeking Agglomeration: A Test of Market Entry in the Lodging Industry*. *Strategic Management Journal*, 25(7): 689-699.
13. Alcacer, Juan, and Wilbur Chung. 2007. *Location Strategies and Knowledge Spillovers*. *Management Science* 53 (5):760–776.

Agglomeration, Innovation and Entrepreneurship

14. Saxenian, AnnaLee. 1991. *The Origins and Dynamics of Production Networks in Silicon Valley*. *Research Policy* 20 (5):423–437.
15. Audretsch, David B., and Maryann P. Feldman. 1996. *R&D Spillovers and the Geography of Innovation and Production*. *American Economic Review* 86 (3):630–640.
16. Zucker, Lynne G., Michael R. Darby, and Marilyn B. Brewer. 1998. *Intellectual Human Capital and the Birth of U.S. Biotechnology Enterprises*. *American Economic Review* 88 (1):290–306.
17. Almeida, Paul and Bruce Kogut. 1999. Localization of Knowledge and the Mobility of Engineers in Regional Networks. *Management Science* 45 (7): 905-917.
18. Michelacci, Claudio, and Olmo Silva. 2007. *Why So Many Local Entrepreneurs?* *Review of Economics & Statistics* 89 (4):615–633.

Commentary

This reading list explores the relationship between economic geography and firm strategy, with a particular focus on agglomeration externalities. Agglomeration refers to the dynamic process underpinning the observed phenomenon that economic activity tends to concentrate geographically, generating localized “clusters,” particularly within certain industries (e.g., software in Silicon Valley or movie production in Hollywood).

Agglomeration is of particular interest to scholars of firm strategy because choosing appropriate locations for operations is one of the most basic decisions that managers must make, and the literature on agglomeration provides insights into the interdependencies and dynamics of location decisions for firms within an industry, and the likely impact on competitive advantage.

The six core readings provide a good introduction to the theoretical underpinnings and recent developments in the literature on geography, agglomeration and firm strategy. This commentary traces the development of these ideas as they relate to firm strategy, highlights some of the main challenges that have emerged for research in this area, and discusses recent advances that address these challenges; the supplementary readings provide additional background and further development of core ideas for the interested reader. Articles referred to in the commentary that also appear in the supplementary readings list are marked with an asterisk (*).

Most people trace the beginning of the modern era of economic geography to the work of Alfred Marshall. In his path-breaking work, Marshall (1920)* focused on three sources of positive externalities or agglomeration benefits—inter-firm technological spillovers, access to specialized labor, and access to specialized intermediate inputs—which have since been at the heart of theoretical and empirical developments in the field. Demand-side economies associated with customer search costs have also been explored in subsequent work (see Fischer and Harrington, 1996*).

Core reading #1, (Hanson, 2001), provides a concise and accessible review of the main strands of the theoretical literature that builds on Marshall’s insights. The lines of argument in this theoretical literature are quite rich and increasingly sophisticated, particularly since the emergence of the “new economic geography” in the early 1990s (for a review, see, Krugman, 1998*). However, as Hanson explains, the challenge in this

research domain lies less in developing new theoretical mechanisms that might explain agglomeration and more in empirical assessment of (i) whether agglomeration actually exists and (ii) the importance of different agglomeration mechanisms.

The significant empirical challenges faced by scholars of agglomeration are rooted in the fact that the basic empirical phenomenon - firms clustered in one location - may be generated by agglomeration economies or by location-specific traits that are quite unrelated to the presence of other firms in the cluster. Establishing causal links between agglomeration and organizational performance thus requires that we are able to differentiate empirically between two possible causes of location choice —physical location traits and proximity to other firms. If one were able to control for all location traits, one could of course separate these two elements. However, this is practically impossible. In the last few years the literature has followed several approaches to address this problem. The first, and arguably the most important approach is that illustrated in Core reading #2 (Glaeser and Ellison, 1997). This approach, still under development (see, e.g., Ellison, Glaeser & Kerr, 2007*) is known as the dartboard approach. The basic idea is that not all locations are equal and there is a natural underlying propensity for firms to flock to a given location given its geographical traits. This propensity is the null hypothesis against which one should compare actual data to determine whether there is agglomeration. In the analogy adopted by Glaeser and Ellison, each location is a ring within a dartboard, with more inherently attractive locations located towards the exterior of the board. Since the external rings of a dartboard are larger it is therefore more likely that a random dart thrown at the board will hit these locations.

Other approaches to overcoming the empirical challenges of assessing agglomeration effects include the use of location fixed effects (see Head, Ries, and Swenson, 1995*); analysis of cluster dynamics, which allows researchers to control for time-invariant location traits, (e.g., Davis and Weinstein, 2002*; Dumais et al., 2002*); and more qualitative research aimed at uncovering specific agglomeration mechanisms (e.g., Saxenian, 1991*).

Reflecting its roots in economics, most of the research on agglomeration through the late 1990s assumed that all firms benefit equally from clustering. Core reading #3, (Flyer & Shaver, 2000) introduces firm heterogeneity and thus brings agglomeration more firmly onto the strategy terrain. The argument offered in this canonical reference in agglomeration and firm strategy is quite simple: given heterogeneity in firm-specific

capabilities and market positions, the benefits and costs associated with locating in a cluster are also likely firm-specific; as a result, while many firms will be attracted to locating in a cluster, some firms will prefer to choose an isolated position and locate away from a cluster. For example, more capable firms may contribute disproportionately to other firms in the cluster through knowledge spillovers while having little to gain from the spillovers generated by inferior firms. Consistent with this argument, Flyer and Shaver (2000) find evidence of adverse selection in clusters (as smaller and weaker firms disproportionately agglomerate) in a sample of foreign greenfield investments in U.S. manufacturing industries. Critics have nonetheless noted that the “flee and follow” logic proposed here suggests complex cluster dynamics that are underspecified in the natural language arguments advanced in this paper, so that the implications for agglomeration patterns remain unclear. In a follow-on paper Flyer and Shaver (2003)* address this concern by developing a formal model which provides important additional results and qualifications. Subsequent empirical work has further refined our understanding of this basic phenomenon.

Core reading #4, (Alcacer, 2006) takes the idea of heterogeneity in agglomeration costs and benefits to a deeper level, exploring the strategic implications of agglomeration across different value chain activities in the face of heterogeneous capabilities. At the functional level, Alcacer argues that agglomeration is most likely to be beneficial for R&D activities, while locating sales operations within clusters may generate few benefits and increase inter-firm competition, so reducing profitability. Evidence from an empirical study that applies and builds on the Ellison and Glaeser methodology (Core reading #2) finds that, consistent with Flyer & Shaver (2000), more-capable firms collocate less than less-capable firms, and that this is most pronounced among sales subsidiaries, followed by production and then R&D.

Core reading #5, (Sorenson & Stuart, 2001) represents a departure from the economic foundations of the prior readings and focuses instead on the social and institutional structures that shape spatial patterns of exchange. The paper provides a brief review of the sociological view of the importance of proximity in determining the likelihood of social or economic exchange, and demonstrates how social networks in the venture capital community diffuse information and expand the spatial radius of exchange. This paper also provides an entry point to the emerging research exploring effects of agglomeration on entrepreneurship (see, e.g., Michelacci and Silva, 2007*).

The idea that knowledge flows (and knowledge spillovers) may be mediated by social networks points to a potential limit or boundary condition for agglomeration benefits: To the extent that social networks span firm boundaries, (or become decoupled from firm identities altogether), then this may undermine or substitute for agglomeration benefits based on firm location, with intriguing implications for firm strategy. Core reading #6 (Agrawal, Cockburn & McHale, 2006) explores this possibility. By estimating the knowledge “flow premium” associated with an inventor’s prior location Agrawal et al provide evidence that indeed social relationships, not just physical proximity, are important for the types of knowledge flows that constitute one of the fundamental externalities posited to lead to agglomeration. Sitting at the intersection of economics, social networks and innovation research, this paper also provides a useful perspective on the relationship among these related but distinct lines of research and an entry point into the literature on location and innovation (e.g., Audretsch and Feldman, 1996*; Zucker, Darby, and Brewer, 1998*).

The study of geography and agglomeration as contributors to industry and organizational performance represents a well-developed and vibrant stream of research within strategy. The theoretical underpinnings of models of agglomeration and location choice are strong and continue to develop as additional mechanisms and sources of heterogeneity are explored. Empirical work continues to grapple with the thorny challenge of designing and implementing empirical studies that can isolate causal mechanisms; the articles in this reading list represent some of the most successful efforts to date, and should provide inspiration and guidance for future work.

V. Internal Organization and Fit

In **Strategy Process** Jan Rivkin suggests that research on the **process** aspects of strategy is less prominent than research on strategy **content**, in part because processes are inherently challenging to research. There is nonetheless a large literature on this important topic, and Jan walks us through this literature by first returning to the original division in the literature between strategy content and strategy process and introducing the debate between those who believe that strategies are discovered by an emergent process of search and those who see more deliberate, rational processes in play. Special attention is played to the resource allocation research of Joe Bower, his students, and Robert Burgelman which has had enormous influence on the field. More recent contributions are also highlighted, as well as several reflections by leading process researchers on methods and motivations for studying strategy process, intended to fuel a discussion of quality standards in this research domain. In highlighting the process aspects of different strategy domains, the readings in this list complement those in several other topic areas, most notably **Innovation** and **Organizational Form and Performance**.

Organizational Learning also has a strong process component, as emphasized in Olav Sorenson's reading list on the topic. This list traces organizational learning research – aimed at understanding how organizations improve in their ability to perform some operation with experience – focusing on two of the most prominent lines of thought: learning curves (economies of experience) and the choice of exploration versus exploitation. Highlighted developments in learning curve research include extensions that allow different firms to experience different learning curves depending on their organizational structure, their portfolio of products and other dimensions of firm heterogeneity.

As Nicolaj Siggelkow notes in **Fit** the concept of fit has a long tradition in the field of strategy and organizational studies. There is a large body of literature on each of two broad classes of fit: internal fit – the consistency among various elements under the control of an organization, and external fit – the appropriateness of an organization's chosen elements given the organization's environment. Of these, internal fit relates closely to issues in the topic areas of **Organizational Form and Performance** and **Strategy Process**. Siggelkow traces the development of the literature on fit, assessing progress in answering three key questions: (i) What does it conceptually mean for an organization to have "fit"? (ii) How can one empirically measure fit? And (iii) What are the implications of fit for

performance and the sustainability of performance differences among organizations?

Strategy Process

Contributed by Jan W. Rivkin

Asterisks (*) identify core readings. Mortarboards (☞) designate articles that emerged, at least in part, from an author's doctoral research.

Origins of the content / process divide and basics of managerial decision making

Alfred D. Chandler, Jr. 1962. *Strategy and Structure: Chapters in the History of the Industrial Enterprise*. MIT Press, Chapters 1, 6-8.

Kenneth A. Andrews. 1971. *The Concept of Corporate Strategy*. Dow Jones-Irwin, Chapters 1-3.

- * Herbert A. Simon. 1957. *Administrative Behavior: A Study of Decision-making Processes*. Free Press, Chapters 1, 5.

Richard M. Cyert and James G. March. 1963. *A Behavioral Theory of the Firm*. Prentice-Hall, selections, especially Chapter 6.

The emergent vs. deliberate debate

Henry Mintzberg. 1978. "Patterns in Strategy Formulation." *Management Science* (24), 934-948.

- * Henry Mintzberg. 1990. "The Design School: Reconsidering the Basic Premise of Strategic Management." *Strategic Management Journal* (11), 171-195.
- * H. Igor Ansoff. 1991. "Critique of H. Mintzberg's 'The Design School: Reconsidering the Basic Premise of Strategic Management.'" *Strategic Management Journal* (12), 449-466.

Henry Mintzberg. 1991. "Learning 1, Planning 0: Reply to Igor Ansoff." *Strategic Management Journal* (12), 463-466.

Richard Pascale. 1984. "Perspectives on Strategy: The Real Story Behind Honda's Success." *California Management Review* (26: 3), 47-72.

- * Graham T. Allison. 1969. "Conceptual Models and the Cuban Missile Crisis." *American Political Science Review* (43), 689-718.

- 🎓 Nicolaj Siggelkow. 2002. "Evolution Toward Fit." *Administrative Science Quarterly* (47), 125-159.

The resource allocation process school of thought

- * Joseph L. Bower. 1970. *Managing the Resource Allocation Process*. Harvard Business School Press, Chapters 1-3.

Robert A. Burgelman. 1991. "Intraorganizational Ecology of Strategy Making and Organizational Adaptation: Theory and Field Research." *Organizational Science* (2): 239-262.

- * Robert A. Burgelman. 1994. "Fading Memories: A Process Theory of Strategic Business Exit in Dynamic Environments." *Administrative Science Quarterly* (39), 24-56.

Robert Burgelman. 2002. *Strategy is Destiny: How Strategy Making Shapes a Company's Future*. Free Press.

Joseph L. Bower and Clark G. Gilbert. 2005. "A Revised Model of the Resource Allocation Process." Chapter 20 in *From Resource Allocation to Strategy*. Oxford University Press, 439-455.

- 🎓 Tomo Noda and Joseph L. Bower. 1996. "Strategy Making as Integrated Processes of Resource Allocation." *Strategic Management Journal* (17: special issue), 159-192.

Personal, cognitive, and political influences on strategic decision making

Donald C. Hambrick and Phyllis A. Mason. 1984. "Upper Echelons: The Organization as a Reflection of its Top Managers." *Academy of Management Review* (9), 193-206.

Donald C. Hambrick. 2005. "Upper Echelons Theory: Origins, Twists and Turns, and Lessons Learned." Chapter 6 in Ken G. Smith and Michael A. Hitt (eds.), *Great Minds in Management: The Process of Theory Development*, Oxford University Press, 109-127.

William Ocasio. 1997. "Towards an Attention-Based View of the Firm." *Strategic Management Journal* (18: special issue), 187-206.

- 🎓 Mary Tripsas and Giovanni Gavetti. 2000. "Capabilities, Cognition and Inertia: Evidence from Digital Imaging." *Strategic Management Journal* (21), 1147-1161.

Kathleen M. Eisenhardt and L.J. Bourgeois. 1988. "Politics of Strategic Decision Making in High-velocity Environments: Toward a Midrange Theory." *Academy of Management Journal* (31), 737-770.

- ☛ Gabriel Szulanski. 1996. "Exploring Internal Stickiness: Impediments to the Transfer of Best Practice Within the Firm." *Strategic Management Journal* (17: special issue), 27-43.

The process of process research

- * Kathleen M. Eisenhardt. 1989. "Building Theories from Case Study Research." *Academy of Management Review* (14), 532-550.

Andrew M. Pettigrew. 1992. "The Character and Significance of Strategy Process Research." *Strategic Management Journal* (13, winter special issue), 5-16.

Andrew H. van de Ven. 1992. "Suggestions for Studying Strategy Process: A Research Note." *Strategic Management Journal* (13, summer special issue), 169-188.

Gabriel Szulanski, Joseph Porac, and Yves Doz. 2005. "The Challenge of Strategy Process Research," *Advances in Strategic Management* (22), xiii-xxxv.

Guide to the Readings

As the introduction to this Ph.D. reader emphasizes, a core aim of strategy research is to pinpoint what drives organizational performance and, in doing so, to explain why performance differs across organizations. Most strategy research examines how the *content* of an organization's strategy, as embodied in the choices it makes, affect performance. A less prominent stream of research focuses on the *process* by which strategic decisions are made.

Strategy-making processes deserve the attention of scholars for at least two reasons. First, to the extent that processes differ across companies and influence organizational outcomes, an understanding of process helps us account for performance differences across firms. Second, if we strategy scholars aim to influence the strategic decisions of practicing managers, we must first have a realistic understanding of how managers make strategic decisions.

Though deserving of scholars' attention, strategy-making processes are inherently challenging to research. The processes by which organizations make strategic decisions are complex and multifaceted, lending themselves to multiple interpretations. Theoretical work in this domain relies on natural-language arguments, and as the introduction to this reader highlights, such arguments are rarely unambiguous and rigorously derived. Empirical work usually faces a dilemma: To explore process, one must dive deeply into individual firms, but this makes it difficult to develop large enough samples to test hypotheses with statistical significance. Moreover, empirical research on process is fraught with the kinds of endogeneity problems discussed in the reader's introduction. Process choices *and* performance may both be driven by other, unobserved factors, and this makes it challenging to pinpoint the impact of process choices on performance. Similarly, empirical work may be stymied by reverse causation: organizational performance may drive process choices rather than vice versa.

These challenges have not stopped generations of scholars from tackling process research. The reading list above introduces doctoral students to process research through five sets of readings. The **first set** takes students back to the original division in the literature between strategy content and strategy process. The readings by Andrews (1971) and Chandler (1962) introduce the content / process distinction and, importantly, emphasize that the two are deeply intertwined in practice. Simon (1957) and Cyert and March (1963) then focus on how managers make decisions. These two classics introduce foundational notions such as bounded rationality,

satisficing behavior, local search, and standard operating procedures. They argue persuasively that managers are not simply omniscient profit maximizers. This is crucial, for if managers were all-knowing and did nothing but maximize profit, we might not need to understand the process by which they reached strategic decisions. We could just focus on the link between decisions and performance—essentially, the strategy content question. In the world that Simon, Cyert, and March describe, however, there is ample room for strategy process to affect performance.

The first set of readings also echoes the reader's introductory comments about high-quality research in strategy. Chandler and Simon illustrate the frame-setting power of good verbal theory, but on reflection, the students should also see the ambiguity in these seminal works. Cyert and March provide a very early example of an effort to employ more formal methods, especially computer simulation.

The **second set** of readings introduces the debate between those who believe that strategies are discovered by an emergent process of search and those who see more deliberate, rational processes in play. The debate is captured most sharply in the exchange between Mintzberg (1990, 1991) and Ansoff (1991). Because this exchange does not represent high-quality research per se, I also include underlying research pieces by Mintzberg (1978) and Pascale (1984). The reading by Allison (1969), which predates the Mintzberg-Ansoff debate, suggests that we need not choose between the emergent and deliberate views. Instead, our theories may be richer if we embrace both. This possibility comes to life in Siggelkow (2002). In this account of the evolution of the choices of Vanguard, the mutual fund company, we see emergent and deliberate efforts combine over time to craft an intricate and effective strategy.

Allison's rational policy and organizational process models of organizational decision making correspond closely to the deliberate and emergent views, respectively. Allison also introduces a third model, in which bureaucratic politics drives decision making. This corresponds well to Bower's (1970) characterization of the resource allocation process. Accordingly, the **third set** of readings focuses on the resource allocation research of Joe Bower, his students, and Robert Burgelman. The Bower & Burgelman research stream constitutes the longest-running and arguably the deepest stream of work on strategy process in large, complex organizations. A premise of the research stream is that a firm's strategy comes to be primarily via its process for allocating resources; the resource allocation process is argued to be the heart of the strategy-making process. Together, Bower and Burgelman introduce a model of the resource allocation process in which three levels of organizational hierarchy

(front-line, middle, and top managers) engage in rounds of four subprocesses: definition of strategic initiatives, impetus for those initiatives, determination of structural content, and determination of strategic context. The first two are bottom-up while the second pair is top-down. To some degree, this integrates the emergent and deliberate views of Mintzberg and Ansoff.

I include this work on the resource allocation process not only because it has had enormous impact but also because it raises the question of quality standards in research on strategy process. Each of Bower (1970) and Burgelman (1991, 1994, 2002) engages in deep longitudinal research within a single organization: National Products and Intel, respectively. How can a researcher reach compelling conclusions on the basis of observing one organization? How have these researchers made tradeoffs among the attributes of high-quality research identified in the reader's introduction (e.g., between an accurate depiction of the phenomenon under investigation and lack of ambiguity in terms, premises, and conclusions)?

The reading list includes a time series of Burgelman pieces as well as Bower and Gilbert's (2005) retrospective chapter in order to show how these researchers have learned and how their perspectives have evolved over time. Bower and Gilbert also identify open questions in the field, questions that may interest doctoral students. Noda and Bower (1996)—writing on the basis of Noda's dissertation—illustrate how much a doctoral student can accomplish when working on strategy process. Their work follows two regional Bell operating companies—organizations intentionally set up to be similar—as successive rounds of resource allocation lead the two to very different strategies for wireless communications. This approach of seeking and exploiting a natural experiment could address many of the empirical difficulties identified in the reader's introduction.

The **fourth set** of readings is admittedly a potpourri. It includes a set of influential articles that have introduced other considerations into the study of strategy process, and it illustrates some of the methodological options open to process researchers. Hambrick and Mason (1984) identify ways that the personal characteristics of top managers may shape strategic processes and choices. This paper launched a stream of research on the impact of “upper echelons” of management on strategy, a stream that Hambrick (2005) reviews in a personal and engaging manner.

Ocasio (1997) focuses on cognitive aspects of the strategy process—particularly the allocation of managerial attention. The paper illustrates a tension mentioned in the reader's introduction, between being true to a phenomenon's core drivers (where the paper succeeds) and being

parsimonious, unambiguous, and rigorously derived (where the paper has gaps). Like Ocasio, Tripsas and Gavetti (2000) emphasize the role of cognition in the strategy-making process. Specifically, they examine how cognitive processes underpinned inertia in the failed strategy of Polaroid, the film maker.

Eisenhardt and Bourgeois (1988) emphasize the role of power and politics in the strategy-making process. The inductive, multi-case work of Eisenhardt and Bourgeois is potentially important to doctoral students since this style of field research has become fairly common on the strategy job market. Szulanski (1996) also considers politics and, more broadly, intrafirm communication in his study of the transfer of best practices within a firm.

The **final set** of readings is a set of reflections by leading process researchers—Eisenhardt (1989), Pettigrew (1992), Van de Ven (1992), and Szulanski, Porac, and Doz (2005)—on methods and motivations for studying strategy process. The readings are intended to fuel a discussion of quality standards in this research domain. Indeed, while constructing the reading list and considering alternative readings, I repeatedly encountered a thorny issue: In the strategy field, the quality standards for process research, and perhaps for qualitative research in general, are unclear. Indeed, I can point to questions and doubts about many of the readings I have selected. It is important for all doctoral students to grapple with the quality questions and for students interested in process research to realize that they are entering an arena where standards are ambiguous.

Throughout the reading list, I have tried to include relatively influential research papers that came, at least in part, from doctoral research. (See the papers designated by mortarboards.) I have done so in order to inspire doctoral students to think about what they themselves might accomplish in process research during their doctoral days. The quality levels of these pieces can, and should, be debated with the students. The effort to include some dissertation-based papers, combined with the limit on the number of readings, had an unfortunate effect: Many influential pieces of process research were excluded from the reading list. Most appear in the references of the listed papers. Perhaps the biggest oversight is that I have excluded altogether a body of research on making decisions in groups. The literature on group decision-making might deserve its own, separate reading list for doctoral students.

Organizational Learning

Contributed by Olav Sorenson

Core Readings

Rapping, L (1965) "Learning and World War II production functions," *Review of Economics and Statistics*, 47: 81-86

March, JG (1991) "Exploration and exploitation" *Organization Science*, 2: 71-87

Darr, E, L Argote and D Epple (1995) "The acquisition, transfer and depreciation of knowledge in service organizations: Productivity in franchises." *Management Science*, 41: 1750-1762

Thompson, P (2001) "How much did the Liberty shipbuilders learn? New evidence for an old case study." *Journal of Political Economy*, 109: 103-137

+Sorenson, O (2003) "Interdependence and adaptability: Organizational learning and the long-term effect of integration." *Management Science*, 49: 446-463

Supplemental Readings

March, JG, LS Sproull and M Tamuz (1991) "Learning from samples of one or fewer." *Organization Science*, 2: 1-13

Burgelman, R (1994) "Fading memories: A process theory of strategic business exit in dynamic environments." *Administrative Science Quarterly*, 39: 24-56

Baum, JAC, and P Ingram (1998) "Survival-enhancing learning in the Manhattan hotel industry, 1898-1980," *Management Science*, 44: 996-1016

Sorenson, O (2000) "Letting the market work for you: An evolutionary perspective on product strategy," *Strategic Management Journal*, 21: 577-592

Denrell, J and JG March (2001) "Adaptation as information restriction: The hot stove effect." *Organization Science*, 12: 523-538

Schilling, MA, P Vidal, RE Ployhard and A Marangoni (2003) "Learning by doing something else: Variation, relatedness and the learning curve." *Management Science*, 49: 39-56

Thompson, P (2007) "How much did the Liberty shipbuilders forget?" *Management Science*, 53: 908-918

+ Denotes research from a dissertation

Commentary

Organizational learning refers to the idea that organizations improve in their ability to perform some operation with experience. For example, over time, firms generally see a steady decline in their costs when they continue to produce the same product. Underlying this macro-level pattern is a micro-level foundation of variation, selection and retention. Firms experiment with new ways of doing things, either intentionally or unintentionally. Most of these experiments fail, but a few of them prove useful. Firms attempt to identify these better ways of doing things and incorporate them into their standard operating procedures. Because firms improve their performance with experience, these dynamics create a kind of positive feedback loop that endows early movers with an advantage with respect to performance.

The literature on organizational learning covers a diverse array of ideas that go well beyond even the optional readings listed above. However, the two most prominent lines of thought are probably learning curves (economies of experience) and the choice of exploration versus exploitation.

The literature on learning curves has been most active in economics and reflects the idea that firms improve their performance on some task as they do more and more of it, so-called learning-by-doing. In this literature, the researchers do not observe variation, selection and retention; they simply assume that it produces cost curves that decline at a declining rate with cumulative production (or performance curves that increase at a declining rate with production).

Much of the early research focused on the production of military hardware during World War II because the U.S. government had all manufacturers use the same production plans and paid cost-plus rates for their output. As a result, researchers did not need to worry about how differentiation or pricing might influence performance. Rapping (1965), for example, estimated the slopes of these experience curves for the production of aircraft.

More recently, researchers have been exploring a number of interesting extensions to this baseline expectation, extensions that allow different firms to experience different curves depending on their organizational structure, their portfolio of products and other dimensions of firm heterogeneity. From the perspective of strategy, these extensions obviously have greater relevance to the choices that firms face because they relate the steepness

of the learning curve to organizational design, product positioning and geographic location choices.

With respect to geographic location choice, Darr, Argote and Epple (1995), for example, studied pizza restaurants and demonstrated that learning-by-doing appears to spill over to other restaurants in the region, particularly those with connections to each other (e.g., through common ownership). Presumably, firms communicate the information that they gain through experimentation to one another, either actively or perhaps passively through the movement of employees across firms. Several studies since then have found similar results in the operation of hotels, in the production of footwear and Liberty ships and in the provision of other goods and services (e.g., Baum and Ingram, 1998; Schilling, et al, 2003, examine similar issues through an experimental design). Learning-by-doing, therefore, may contribute to agglomeration externalities (see Alcacer and Oxley reading list).

Another topic has been how the strategy and structure of the organization influences the efficiency with which it can learn. Sorenson (2003), for instance, discusses how the degree of interdependence between the processes involved in production can inhibit the firm's ability to isolate and implement more effective routines. Consistent with this perspective, he finds that vertically integrated firms learn more slowly, particularly in stable environments. Following a similar logic, one might also expect multi-unit and multi-product firms to learn faster because they can engage in parallel experimentation (for evidence, see Baum and Ingram, 1998, and Sorenson, 2000). Firms might also vary in systematic ways in their ability to retain knowledge (Thompson, 2007). Managers, therefore, must consider not just the static implications of their choices but also their implications for learning and therefore for the longer run.

Despite the wealth of empirical evidence for these learning curves, these studies are not without their problems. Other unobserved factors that vary over time potentially threaten the validity of interpreting these curves as learning. Thompson (2001), for example, gathered data on the capital investments of Liberty shipbuilders, which also increases monotonically over a firm's life, and demonstrated that investment can account for nearly half of the apparent "learning" of the shipbuilders.

The other main line of research considers the opportunity costs to learning-by-doing. Yes, firms can continue to get better and better at some area of expertise, but over time this inertia tends to result in a mismatch between the firm and the environment. A firm could become very good at doing something that the market no longer values, something that March (1991)

describes as a “competency trap” (see also Denrell and March, 2001). A fundamental tension therefore exists between whether the firm builds on its strengths versus whether it explores the environment for alternatives that might have greater appeal. Much research has sought to relate this tension to policy choices at the level of the firm (e.g., Sorenson, 2000, connects it to the breadth of a firm’s product line).

Internal and External Fit

Contributed by Nicolaj Siggelkow

Core readings

Internal fit:

Chandler, A. D., Jr. 1962. **Strategy and Structure: Chapters in the History of Industrial Enterprise**. Cambridge, MA: MIT Press. (esp. Chapters 1, 2, 6, 7)

Miller, D. 1996. Configurations revisited. **Strategic Management Journal**, 17: 505-512.

New approaches to internal fit:

Milgrom, P. R. & Roberts, J. 1995. Complementarities and fit: Strategy, structure, and organizational change in manufacturing. **Journal of Accounting and Economics**, 19: 179-208.

*Rivkin, J. W. 2000. Imitation of complex strategies. **Management Science**, 46: 824-844.

*Siggelkow, N. 2002. Evolution toward fit. **Administrative Science Quarterly**, 47: 125-159.

External fit:

Lawrence, P. R. & Lorsch, J. W. 1967. **Organization and Environment**. Boston: Harvard Business School Press. (esp. Chapters 1, 4, 6, 8)

Van de Ven, A. H. & Drazin, R. 1985. The concept of fit in contingency theory. **Research in Organizational Behavior**, 7: 333-365.

Supplementary readings

Conceptual background pieces:

Miller, D. & Friesen, P. H. 1984. **Organizations: A Quantum View**. Englewood Cliffs, NJ: Prentice Hall.

*Siggelkow, N. 2001. Change in the presence of fit: The rise, the fall, and the renaissance of Liz Claiborne. **Academy of Management Journal**, 44: 838-857.

Michael E. Porter and Nicolaj Siggelkow. 2008. "Contextual Interactions within Activity Systems and Sustainability of Competitive Advantage." **Academy of Management Perspectives**, 22 (2), pp. 34-56.

Empirical tests of external fit:

Drazin, R. & Van de Ven, A. H. 1985. Alternative forms of fit in contingency theory. **Administrative Science Quarterly**, 30: 514-539.

Gresov, C. 1989. Exploring fit and misfit with multiple contingencies. **Administrative Science Quarterly**, 34: 431-453.

Conceptual work on complementarities:

Milgrom, P. R. & Roberts, J. 1990. The economics of modern manufacturing: Technology, strategy, and organization. **American Economic Review**, 80: 511-528.

Empirical work on complementarities:

Athey, S. & Stern, S. 1998. **An empirical framework for testing theories about complementarity in organizational design**. NBER Working Paper #6600, Cambridge, MA.

Ichniowski, C., Shaw, K., & Prenzushi, G. 1997. The effects of human resource management practices on productivity: A study of steel finishing lines. **American Economic Review**, 87: 291-313.

Cassiman, B. & Veugelers, R. 2006. In search of complementarity in innovation strategy: Internal R&D and external knowledge acquisition. **Management Science**, 52: 68-82.

Simulation work on interdependencies:

Levinthal, D. 1997. Adaptation on rugged landscapes. **Management Science**, 43, 934-950.

Siggelkow, N. & Rivkin, J. W. 2005. Speed and search: Designing organizations for turbulence and complexity. **Organization Science**, 16: 101-122.

* An asterisk denotes a reading that emerged, at least in part, from an author's dissertation research.

Commentary

The concept of fit has a long tradition in the field of strategy and organizational studies. As a result, it has been employed in many ways. While fit is generally employed to describe a state of consistency among various elements, the nature of these elements can differ. Consequently, it is helpful to distinguish between two broad classes of fit: internal fit – the consistency among various elements under the control of an organization, and external fit – the appropriateness of an organization’s chosen elements given the organization’s environment.

In general, both types of fit are assumed to have performance implications. Firms with high internal and external fit are assumed to outperform firms with less fit. Since a core aim of strategy research is to pinpoint the drivers of differences in organizational performance (as noted in the introduction to this PhD reader), the topic of fit has always been one of the key concepts in strategy.

Central questions in this arena have been:

1. What does it conceptually mean for an organization to have “fit”?
2. How can one empirically measure fit?
3. What are the implications of fit for performance and the sustainability of performance differences among organizations?

Since the topic of fit has been part of strategy and organizational studies for a long time, a large range of possible readings exist. Two seminal works in this context are Chandler (1962) on the internal fit between a firm’s strategy and structure, and Lawrence & Lorsch (1967) on external fit, the cornerstone of what has also become known as “contingency theory.” Not only are these two books foundational to the topic of fit; they also represent exemplars of rich, inductive research. They illustrate, as the reader’s introduction suggests, how verbal theorizing can be used to inject major new ideas into a stream of literature. From an empirical standpoint, these studies show the power of a close match between theoretical constructs and the data used to test a theory.

The next generation of research applied and tested contingency ideas in various settings. In the area of internal fit, the work by Miller (and Friesen) is particularly pertinent. These authors used the term “configuration” to describe internally consistent set of choices, and they studied various performance consequences. The review by Miller (1996) gives a good

overview of this line of research. A nice complement to the study of external fit is the article by Van de Ven & Drazin (1985). In this piece, the authors describe a range of ways that fit has been conceptualized and tested in the literature.

The newest generation of research has extended prior research in three directions:

First, a particular type of internal fit – complementarity – has been formalized and made very precise. Milgrom & Roberts (1995) provides a good overview of this concept and uses a case (on Lincoln Electric) to illustrate the ideas. The complementarity framework has spawned a large theoretical and empirical literature; as a result, it is helpful for students to get exposed to this framework.

Second, simulation modeling has made it possible to study more formally the effects of interdependencies and fit. In this context, the NK-model, a model originally developed in evolutionary biology, has become an important research tool to create “performance landscapes.” Performance landscapes can serve as a helpful visualization for the notion of fit. A performance landscape consists of “horizontal” dimensions representing the various choice elements of an organization and a “vertical” dimension representing the resulting performance associated with each combination of choices. Interactions create multiple peaks on these landscapes, with each peak representing an internally consistent set of choices – a configuration that displays high internal fit. The paper by Rivkin (2000) is a good example showing how simulation can shed light on key strategy issues, such as imitation, and the role that fit and interdependence play in creating and sustaining performance differences among organizations.

Both complementarity research and simulation models of fit exemplify a point made in the reader’s introduction: Formal methods can advance strategy theory by clarifying a researcher’s assumptions and showing rigorously how claims follow from assumptions.

The third new research direction on fit builds on the fact that, with few exceptions, prior work on fit has tended to be static in nature. The work by Siggelkow (2002) provides a more dynamic view of fit, describing the evolution of fit within organizations. It is also a more recent example of an inductive, qualitative research project.

Supplementary readings:

The book by Miller and Friesen (1984) is a good introduction to the notion of “configuration.” The book touches on a range of issues, including the dynamics of fit, which, however, were not picked up by researchers for a long time.

The paper by Siggelkow (2001) discusses the notions of internal and external fit and provides precise definitions of these terms. This paper combines some of the conceptual insights that come from simulation work with an extensive case discussion of a single firm (Liz Claiborne).

The paper by Porter & Siggelkow (2008) contains an accessible and fairly comprehensive review of the work based on Milgrom & Robert’s framework of complementarity and the simulation work based on the NK-model. It also discusses the shortcomings of both approaches.

An alternative to the Van de Ven & Drazin (1985) ROB chapter is the Drazin and Van de Ven (1985) ASQ article. The ROB chapter has more detail on the various ways in which fit has been tested; the ASQ article contains actual empirical tests of various fit conceptualizations. A further example of empirical work on external contingencies is Gresov (1989). Here, the issue is raised that many firms face several contingencies at the same time, raising a serious difficulty for simple contingency logic.

The paper by Milgrom & Roberts (1990) is generally seen as the starting point for the formal work on complementarities. However, their 1995 article is more accessible and contains the results of the 1990 paper.

The empirical challenges of testing complementarities are often understated. The manuscript by Athey and Stern (1998), while a demanding read, is important for those who attempt to engage in serious empirical work in this domain. This paper provides a good overview of the challenges that must be overcome if a test of complementarity is supposed to be conclusive.

Two good examples of empirical tests are Ichniowski, Shaw & Prennushi (1997) and Cassiman & Veugelers (2006). The Ichniowski et al. piece is very carefully constructed empirical research on complementarities. The Cassiman & Veugelers paper is interesting as it extends research beyond pair-wise interaction effects, a problem of complementarity research noted in the Porter & Siggelkow (2008) article.

Lastly, with respect to simulation studies, the Levinthal (1997) article is the first in the management literature to use the NK-model, while the

Siggelkow & Rivkin (2005) paper illustrates how to use simulations to explore the boundary conditions of received wisdom with respect to contingency relationships in the realm of organizational design.

VI. Contributors to this Volume

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