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The Strategy Field from the Perspective of *Management Science*: Divergent Strands and Possible Integration

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We reflect on the evolution of the strategy field as seen through the window of *Management Science*. Reflecting the diverse disciplinary roots of strategy research, we identify a broad-ranging body of work that varies with respect to the assumptions made regarding individual rationality and the level of analysis at which the research is carried out. We argue that recent developments begin to delineate a potentially unifying conceptual framework for treating the field's defining questions—the conceptual apparatus of evolutionary economics. We conclude by laying out important challenges for evolutionary economics if it is to serve as a foundation for both the positive and the normative research agendas of the strategy field.

Key words: business strategy; evolutionary economics; behavioral theory of the firm

1. Introduction

The field of business strategy is roughly contemporaneous with the 50-year history of *Management Science*.¹ *Management Science* as a journal is thus an interesting window on how the field has developed. It is a particularly apt window because of the journal's problem-driven orientation, which draws on a range of disciplines, and thereby mirrors important elements of the strategy field itself. The strategy field is intellectually broad in its disciplinary roots, which range from economics to organizational sociology, and in the problem domains that define its scope of applications. Although this intellectual diversity is both appealing and a source of robustness, it also poses enormous challenges for the coherent development of strategy as a field, and may perhaps

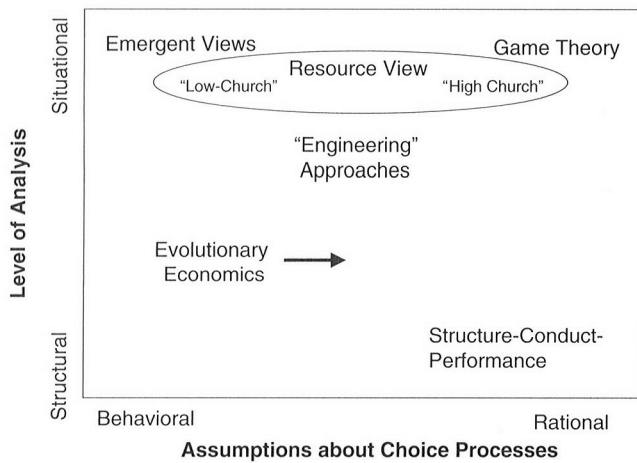
¹ The strategy field has its roots in early courses on business policy that attempted to integrate the distinct functional fields to which students were exposed (Ghemawat 2002). A basic strategic framework linking firm capabilities to the firm's competitive environment was offered by Andrews (1971), where strategic decisions were those concerned with the long-run performance of the firm. This basic normative agenda of examining the distinctive competencies of firms in relationship to competitive markets, as well as the positive agenda of explaining variation in firm performance, has continued to define and guide the field's development (see Ghemawat 2002).

relegate strategy to a low-paradigm status (Pfeffer 1993). We believe, however, that recent developments, many of which have been manifest in *Management Science* itself, begin to delineate a coherent, unified conceptual apparatus for treating the field's defining question of understanding the bases of performance variation across firms.

In this spirit, we first illustrate the intellectual diversity of the strategy field as demonstrated by important contributions that appeared in *Management Science*. We will not enumerate the large number of individual contributions, but rather focus on select ones that helped define and illustrate the historical development of the field. Although the formal strategy department of *Management Science* is of relatively recent vintage, dating from 1999, *Management Science* has featured strategy research from early on.

To provide a structured overview of such efforts, it is useful to rely on an overarching framework in which the numerous contributions can be located. We do so in Figure 1, which offers two basic dimensions reflecting two critical assumptions underlying research contributions to the field. One dimension relates to the researchers' assumptions about the nature of individual choice behavior. We observe enormous variation in the field along this particular dimension. Strategy research runs a wide gamut,

Figure 1 Mapping of Strategy Research



from work that characterizes behavior as being highly rational, involving the sophisticated game-theoretic logic of Nash equilibrium and sequential rationality (Harsanyi 1967), to work that treats actors as boundedly rational, whose intentional rationality is at best quite local (Mintzberg 1971, Levinthal 1997). The other dimension pertains to the level of analysis at which the research operates, with research ranging from a situational focus on the highly specific qualities of the setting in which actors are embedded, to research that is more structural and examines more aggregate patterns of behavior.

We then show that strategy research has moved in recent years toward the “middle ground” of this figure, and that this movement represents an opportunity for more unified treatments in strategy research. Recent perspectives on performance differences among firms have embraced a view that treats choice as intendedly rational, but not necessarily adhering to the strong rationality assumptions of neoclassical economics. Such perspectives, therefore, lie in the middle of our horizontal axis that delineates assumptions on choice behavior. Furthermore, these analyses of firm performance differences are increasingly being placed more centrally in a market context, where questions of consumer demand and market competition are now more salient than had been the case for earlier, more purely firm-centered accounts. We suggest that the framework of evolutionary economics (Nelson and Winter 1982, 2002) rests on a conceptual apparatus that is quite consistent with the nature of this movement. More specifically, we view it as an emerging archetype, a paradigm, which has the potential to unify this growing middle ground and provide the coherence that is key to the cumulative development of any field of intellectual inquiry. Evolutionary economics builds on both the behavioral theory of the firm as developed by the “Carnegie School” (March and Simon 1958, Cyert and

March 1963) and the classic traditions of industrial organization within the economics literature (Bain 1956). This middle ground between behavioral perspectives at the firm level and economic viewpoints at the markets level not only represents a movement in the dimension of assumptions regarding individual behavior, but also provides stronger linkages across levels of analysis ranging from the microfoundation of organizational routines to broad patterns of industry entry, exit, and profitability.

However, the evolutionary economics framework, particularly its microfoundation, as currently constituted must overcome important limitations if it is to become a more inclusive platform for strategy research and practice. In particular, the representation of Simon’s (1955) argument regarding bounded rationality, especially in formal modeling, has typically taken the form of myopic hill-climbing, or quasi-Skinnerian bases of action. Strategic action clearly involves greater degrees of intentionality, so fuller representations of cognition would need to be incorporated into such theoretical efforts. In addition, although evolutionary economics has addressed the linkage between the firm and markets, it has not similarly developed the linkages within the corporation across levels of the organizational hierarchy. Finally, we explore the capacity for evolutionary arguments to account for periods of apparent rapid change. We briefly flesh out these issues in the third part of this essay and lay out some possible avenues for future research.

2. Looking Backward: The History of the Field Through the Lens of *Management Science*

Early writing in the strategy field was of what we term the “engineering” variety. Authors such as Ansoff (1965), Ansoff and Brandenburg (1971), Mason (1969), and Mitroff et al. (1977, 1979, 1982) addressed the practical concerns of strategic management, considering in particular how to design planning processes that would enhance the quality of strategic decision making. These authors implicitly evoked a world of intended rationality, in which individuals were not assumed to be able to derive the optimal actions. Rather, this line of work attempted to define mechanisms that would enhance the intelligence of strategic decision making. In this spirit, we use the label “engineering approach,” because such work is rationalistic in its intent, involving the conscious manipulation of strategic processes, but it is also grounded in the phenomena of actual corporate decision making and quite practical in its orientation. However, this general line of research was grounded more on the normative tools of decision theory and

systems analysis than the traditional social science disciplines of economics and sociology. In later years, many issues posed and addressed by this “engineering” approach influenced the research agenda of more self-consciously social science-based examinations of strategy questions.

Management Science was also the home of Harsanyi’s (1967) seminal contribution on games with incomplete information. It took a considerable period of time, however, for this work to link with the strategy field. The flowering of game-theoretic treatments of questions of industry entry and strategic choices involving such variables as pricing, advertising, and R&D came only in the late-1970s and early-1980s—a body of work ultimately codified in Fudenberg and Tirole’s textbook (1991). Ironically, although Harsanyi’s Nobel prize-winning contribution was published in *Management Science*, relatively few strategy-oriented game-theoretic articles appeared subsequently in the journal (see Conner and Rumelt 1991, Cohen and Levinthal 1994 for exceptions), let alone those modeling games of incomplete information (see Amit et al. 1990, Ryall 2003 for exceptions). Game-theoretic treatments of business strategy questions obviously presume that individual actors possess high levels of rationality. Hence, we locate such approaches in the right-hand side of our diagram. A less obvious facet of such modeling efforts is that they are highly situational, depending on very specific assumptions regarding information sets, timing of players’ actions, and the like.

Early traditions within economics stemming from Bain’s work on industry analysis and the development of the structure-conduct-performance paradigm had a different ambition: to characterize the broad and systematic differences in profitability (or social welfare) across industries. Such work, coming out of the economics discipline and applied to questions of business strategy by Porter (1980), clearly presumed actors engaged in rational behavior but it developed arguments that were structural and did not specify individual behavior precisely. Although the journal’s contribution to the application of game theory to the field of business strategy was rather modest beyond Harsanyi’s foundational work, *Management Science* has been an important outlet for efforts centered on the application of the structural analysis of industries to questions of business strategy. A particularly significant contribution was Schendel’s early work with various collaborators, using large-sample econometrics to estimate performance differences among firms (Schendel and Patton 1978) and the identification of strategic groups in particular (Cool and Schendel 1987). The work of McGahan and Porter (2002) represents an important renewal of this lineage, although their work follows most directly from Schmanlensee’s

(1985) and Rumelt’s (1991) attempts to systematically partial out business unit, firm, and industry determinants of profitability.

Management Science has also been home to some of the most salient contributions to the resource view of the firm. In particular, Barney’s (1986) work on strategic factor markets is the paradigmatic statement of what Levinthal (1995) termed the “high-church” variant of the resource view.² Barney sets up a recursive logic that implies that, in a competitive market for resources, only initial differential information sets or beliefs, or ex post “luck,” can account for resources differences and therefore profitability differences among firms. Only firms with superior insight, or superior luck, can acquire resources for a price that is below their rent-generating capacity. Thus, the work takes the capability view of the firm that Rumelt (1984) and Wernerfelt (1984) had begun to articulate, and applied the dual logic of rational choice at the level of individual firms and market equilibrium at the collective level to derive the core argument.

Dierickx and Cool (1989) responded to Barney with their response constituting a “low-church” expression of the resource-based view. Within their perspective, actions were not necessarily rational and there was not a presumption of equilibrium. Indeed, they postulated that there are unlikely to be markets for the kind of resources that provide an enduring source of advantage. Although not explicitly linked to the Carnegie School tradition of characterizing organizations as engaged in problemistic search, Dierickx and Cool drew a picture of the firm’s capability development that was certainly compatible with such a viewpoint. To the extent that superior profitability is explained by heterogeneity at the resource level, such heterogeneity has its roots in the processes underlying the formation of such resources—a view that has been developed in recent years under the label of dynamic capabilities (Teece et al. 1997). Thus, in our mapping of the field, Dierickx and Cool (1989) represented a significant leftward shift within the resource view along the behavioral-rational axis.

It is also important to note the vertical location of the resource-view literature. The resource view puts forth a highly situated perspective. A critical part of the argumentation is the contrast between relatively generic resources that are readily available on factor markets with highly firm-specific capabilities that emerge with time and highly context-specific interactions—interactions along the value-chain with

² Levinthal (1995) distinguished between what he termed “high-church” variants of the resource view of the firm, which maintain the postulates of rational choice and market equilibrium from neo-classical economics, and so-called “low-church” variants that reject, at least implicitly, these dual assumptions and offer more behavioral perspectives on individual and firm action.

buyers and suppliers, but most importantly interactions within the firm that create some distinct organization-specific capability. However, as a prime impetus of the resource view of the firm was a shift in the locus of analysis from the industry to the firm, a by-product of this shift in emphasis in early writings on firm capabilities was to excessively decontextualize firms from market processes. As Levinthal (1995, p. 22) notes, "to the extent that market considerations enter the discussion, they tend to be factor markets not product markets." *Management Science* has, however, been home to important work that links these ideas of firm capability differences to industry-level outcomes and dynamics. In particular, Wernerfelt and Montgomery (1986) provide an insightful analytical treatment of what is implied by recognizing heterogeneity in firm capabilities in our models of industry structure.

Perhaps surprisingly, *Management Science*, despite its orientation to formal and quantitative approaches, has been the home of many important contributions to process perspectives on strategic management. Among the salient contributions in this vein are some of Mintzberg's early empirical work on the emergence of strategy (Mintzberg 1971) and Miller and Friesen's related work on the evolution of firms' strategies (1978, 1984). These authors argued that a firm's articulated strategy was often an *ex post* construction, occurring subsequent to the emergence of patterns of behavior that had, *de facto*, already configured the firm's strategy. Clearly, this line of work should be placed rather far to the left on our "map" of the strategy field. Furthermore, with respect to the vertical dimension, the perspective is highly situated. The context in which actors operate can greatly influence what they observe and come to learn. However, although strongly behavioral, the organizational aspects of strategy making, particularly the hierarchical ones, are less developed in this line of work. An important exception is Burgelman (1983), which extends Bower's (1970) model of the resource-allocation process and examines the influence of organizational structure and context on the making of strategy.

3. Movement Toward the "Middle"

Recent years have witnessed a sizable body of work in *Management Science* that falls roughly in the middle of our strategy field map. Researchers have shed light on issues of firm learning (Argote et al. 1990, Pisano et al. 2001, Adler and Clark 1991, Denrell et al. 2004), the conscious, path-dependent development of firm capabilities (Kogut 1991, Cohen and Levinthal 1994, Helfat 1994), the flow of knowledge across firms (Von Hippel 1986, 1994) and other institutional actors (Almeida and Kogut 1999, Agrawal

and Henderson 2002, Cohen et al. 2002, Gittelman and Kogut 2003, Zucker et al. 2002), technology and industry evolution (Christensen et al. 1998; Adner and Levinthal 2001; Shane 2001a,b; King and Tucci 2002), firm adaptation in dynamic environments (Romanelli and Tushman 1986, Tushman and Rosenkopf 1996, Baum and Ingram 1998), and diversification activity (Shaver 1998, Silverman 1999).

Some of this work is explicitly rooted in the behavioral theory of the firm, such as Lant's (1992) examination of the dynamics of aspiration levels. Other work takes a self-conscious evolutionary framing such as Helfat's (1994) examination of the emergence and persistence of firm capabilities in the oil and gas industry, Rosenkopf and Almeida's (2003) consideration of search in the space of technical knowledge, Sorenson's (2003) assessment of the role of complexity of organizational form for organizational adaptation in the workstation industry, and Lee's (2003) examination of industry dynamics in the history of the pharmaceutical industry. This more self-conscious evolutionary perspective has also had expression in several papers that adopt the framework of fitness landscapes and the use of computational modeling to consider the emergence of firm heterogeneity via path-dependent search (Levinthal 1997), the degree to which firm heterogeneity is sustained through imperfect imitation of practices (Rivkin 2000), how alternative organizational designs influence the process of organizational search (Rivkin and Siggelkow 2003), and the impact on organizational adaptation of modular structures that vary with respect to some true, but possibly unknown, modular form (Ethiraj and Levinthal 2004).

It is important to note that this "movement towards the middle" is not idiosyncratic to *Management Science*, but is a pattern of research activity for the field more broadly. Work on dynamic capabilities (Teece et al. 1997, Eisenhardt and Martin 2000, Winter 2003) and knowledge-based views of the firm more generally (Cohen and Levinthal 1990, Kogut and Zander 1992) are strongly rooted in an evolutionary viewpoint, whether the source roots are Penrose (1959), Nelson and Winter (1982), or March and Simon (1958). The same can be argued for empirical and theoretical work on the replication of routines and transfer of best practices (Szulanski 2000, Winter and Szulanski 2001, Zollo and Winter 2002). Many early applications of the resource-based view of the firm had a similar flavor, as they focused on how an existing set of capabilities might usefully be applied in new domains (Prahalad and Hamel 1990, Collis and Montgomery 1995). A growing body of research examines the link between the adaptive dynamics of firms and industry (Barnett and Burgelman 1996, Barnett 1996, Henderson and Mitchell 1997). Researchers have studied, for instance, how different

kinds of technological change affect an organization's capabilities and its chances of survival (Tushman and Anderson 1986, Henderson and Clark 1990); analyzed the kinds of capabilities incumbents should possess to limit the probability they will be displaced by a technological change (Teece 1986, Mitchell 1989, Tripsas 1997, Henderson et al. 1999); inquired about the role of entrants' backgrounds on the likelihood these entries will succeed (Carroll et al. 1996, Klepper and Simons 2000b, Helfat and Lieberman 2002); studied industry life cycles (Abernathy and Utterback 1978, Klepper 1997, Klepper and Simons 2000a); explored how firms' context influences their development of capabilities through market feedback and firm incentives (Levinthal and Myatt 1994; Christensen and Rosenbloom 1995; Malerba et al. 1998; Adner 2002; Adner and Zemsky 2003, 2004; Ethiraj 2003); and issues of firm boundaries (Silverman et al. 1997, Karim and Mitchell 2000, Zollo et al. 2002, Jacobides and Winter 2003).³

Work in this middle ground treats actors as boundedly rational—limited both spatially and temporally in their ability to evaluate the consequences of their choices. It also grapples with the challenge of adaptation in the presence of rival firms and shifting bases of competitive advantage. As suggested earlier, these two central traits of the middle ground map well to the conceptual apparatus of evolutionary economics (Nelson and Winter 1982, 2002), which indeed has been adopted (either implicitly or explicitly) in much of the research just noted. The framework of evolutionary economics, therefore, can be taken as representing an emerging archetype, or paradigm for this growing body of work, whose density has increased sufficiently to make it a significant, perhaps dominant, force in the strategy field.

4. The Foundations of Evolutionary Theory

We now consider the nature of the evolutionary framework in more depth. As a first step, we need to consider the precise location of evolutionary economics on the two-dimensional map of the strategy field that we have laid out. Evolutionary economics is perhaps most readily defined by its location on the horizontal axis, which reflects assumptions on choice behavior. Nelson and Winter (1982) frame their work as a reaction to the prevailing body of neoclassical economics research, which, at its core, assumes strong forms of rationality at the individual level

and the related notion of equilibrium at the system level. Particularly troubling for Nelson and Winter, as industrial organization economists interested in issues of technical change, was the gap between the image evoked by these assumptions and the enormous observed heterogeneity in capabilities across firms and the extraordinary persistence of such differences over time. These facts make the presumption that firms optimize input choices in the face of common production functions problematic, thus motivating Nelson and Winter (1982) to consider a different characterization of firm behavior. They found inspiration in the work of the Carnegie School (March and Simon 1958, Cyert and March 1963), as well as in their own direct observations of practice in the context of the RAND Corporation's early studies of technical change.

Their choice of the carrier of this persistent firm-level heterogeneity was the organizational routine, a bundle of coordinated activity that evolves slowly through local learning and typically involves substantial elements of tacit knowledge and context dependence (Cohen et al. 1996). They conceived of routines as the "DNA" of the firm and made them the basis in their evolutionary framework of the observed continuity of firm behavior. This choice was a radical methodological step for at least two reasons. First, they were foregoing the postulate of rationality that, through the coercive and mimetic pressures of institutional forces (DiMaggio and Powell 1983), had achieved paradigmatic stature in post-war economic research. In addition, they were countering strong norms within the social sciences for reduction to the individual level of analysis. Despite these challenges, the choice of routine as the central unit of analysis had clear advantages. First, by centering the theoretical apparatus on a higher unit of analysis than the individual, Nelson and Winter were able to address the key property of behavioral continuity. Second, this choice also had an important function in that it facilitated linking the characterization of firm-level processes of search and adaptation to the competitive dynamics of industries (the vertical dimension in our map). In the spirit of the modern synthesis in biology, Nelson and Winter had connected a process of Mendelian genetics at the firm level with a Darwinian process of selection at the industry level.

Given the strong normative traditions within the field, this choice of unit of analysis is somewhat subversive for strategy research: The image of organizational action as largely driven by routines seems to negate the role of strategic choice. One response to this dilemma has been to suggest that routines may also express themselves as higher-order cognitive heuristics, thus preserving a notion of choice in the context of routinized action (Cohen et al. 1996).

³Given our focus on the organization and industry levels of analysis, we neglect evolutionary work focusing on the broader institutional level (see Rosenberg and Nelson 1994 or Mowery and Nelson 1999).

However, the strong sense in which firm capabilities are suggested to be path dependent (Dosi et al. 2000, Helfat 2003) sharply restricts the domain of choice. Winter (1987, p. 162) summarizes this general point quite powerfully.

A key step in strategic thinking is the identification of the attributes of the organization that are considered subject to directed change and the implicit or explicit acknowledgment that some attributes do not fall in that category. As the Alcoholics Anonymous serenity prayer puts it, "God grant me the serenity to accept the things I cannot change, courage to change the things I can, and wisdom to know the difference." Substitute for *courage* the words *managerial attention and related resources supporting strategic decision-making* and you have here the beginnings of a paradigm for strategic analysis, its role being to help with the wisdom part.

The implications for strategic analysis are twofold: (i) it is important to discriminate between what is and is not controllable, and (ii) the relevance of managerial cognition is limited not only because individual rationality is constrained, but also because the role of strategic choice is limited. This perspective underlies much of the work falling in the middle ground of our map of the strategy field. Despite its power, as testified by its ability to connect with many of the diverse strands of strategy research and by the growing density of the middle ground, we believe this perspective contains a number of limitations that constrain the capacity of evolutionary economics to serve as a foundation for both the positive and the normative research agendas of the strategy field.

5. Expanding the Boundaries of the Evolutionary Perspective

We explore two such limitations that constrain how broad a "footprint" an evolutionary perspective might establish in the space of the foundational assumptions laid out in Figure 1, and conclude with a brief comment on how evolutionary theory is better able to address processes of nonincremental change than might be thought.

First, and perhaps most importantly, empirical accounts of decision making in organizations suggest that while behavior does not adhere to assumptions of full rationality, actors do engage in logics of consequences (March 1994), engaging in behaviors that are often cognitive, proactive, and deliberate (Porac et al. 1989, Weick 1995). Furthermore, while choice is highly constrained relative to a hypothetical world of full rationality and extreme plasticity in organizational action, such constraints still provide ample room for managerial decision making to impact the variation in performance among actual, as opposed to hypothetical, firms. Indeed, the theory of bounded rationality does not preclude behavior premised on a

logic of consequences (Simon 1955, March and Simon 1958). In contrast, much theorizing within the tradition of the behavioral theory of the firm, particularly formal modeling, tends to address those aspects of Simon's arguments regarding the need for search, but are relatively silent on the issue of how alternatives are to be evaluated or issues of cognition more generally. The result is a view in which deliberation and strategic choice are relegated to a modest corner. The question then arises as to what extent the evolutionary framework can incorporate elements of deliberation and cognition into its canonical microfoundation—i.e., to what extent can work under the rubric of evolutionary economics "move" towards the middle of the horizontal dimension of Figure 1.

How can one think about synthesizing both routinized and cognitive forms of behavior into a coherent microfoundation? To remain faithful to the evolutionary logic, the property of firm-level behavioral continuity must be maintained. Routines were originally chosen to represent the genes of the organization, and local forms of search, the mechanisms of variation; the current challenge is to identify other genetic traits, as well as search mechanisms operating on those traits, consistent with more deliberate forms of thinking.

Some traces of such an argument are beginning to appear. Gavetti and Levinthal (2000) represent bounded rationality not only in routinized behavior, but also in the form of imperfect cognitive representations that actors have of their environment; these representations, in turn, inform "off-line" evaluation of choice alternatives which provides direction to local forms of experiential learning and the emergence of routines. Ghemawat and Levinthal (2002) consider the degree of a priori cognition required to achieve intricate activity systems (Porter 1996). Tripas and Gavetti's (2000) account of Polaroid's failed transition from instant to digital imaging illustrates the power of cognitive representations as carriers of behavioral continuity, thus giving them a similar status as routines as part of the organization's genetic material. Gavetti (2003) explore how cognitive representations affect reasoning by analogy or case-based reasoning. Such reasoning allows managers to transfer potentially useful strategic wisdom they have obtained from prior contexts that their representations suggest are similar to their current strategic context. Denrell et al. (2004) examine the emergence of belief structures and organizational routines that develop via a process of credit assignment in which actions that lead to states that are viewed as being of value (stage-setting actions) are themselves rewarded, apart from any direct feedback from the environment.

Another important strand of research linking cognition with evolutionary perspectives is work

examining the relationship between codification and the subsequent reapplication of that knowledge (Zollo and Winter 2002, Narduzzo et al. 2000). The re-enactment of routines is not without important elements of cognition (Feldman 2000) nor, more generally, are “mindful” and “less-mindful” forms of organizational intelligence necessarily discordant (Rerup and Levinthal 2003).

Second, within the evolutionary economics tradition, the image of routines and capabilities developing “from below,” in a local and semiautomatic manner has dominated the quest for a theory of capabilities’ genesis and evolution. As a consequence, current evolutionary theory largely ignores the role of the organizational hierarchy. Ironically, hierarchy, particularly the notion of decision premise (Simon 1976), was central to the Carnegie School (March and Simon 1958, Cyert and March 1963), and, as previously mentioned, has been shown to be critical to strategy making processes (Bower 1970, Burgelman 1983). We believe evolutionary accounts of firm capabilities and strategies ultimately need to pay more attention to the linkages across actors within the organizational hierarchy (see Baum and Singh 1994, Ocasio 1997, Gavetti 2003, Dosi et al. 2003 for suggestive efforts along these lines).

A final dilemma concerns how the evolutionary perspective, whose microfoundation rests on the postulate of behavioral continuity, can account for moments of seemingly discrete change. We believe this dilemma is, to a large degree, only apparent. Indeed, in this regard, it is important to distinguish between the relatively fixed quasi-genetic traits that inevitably underpin an evolutionary argument with the manifestation of those traits. For instance, a firm may have relatively fixed rules about when it engages in major capital expansion based on market trends and its current capacity utilization; the capital investment is a significant discrete event, but it emerges out of a set of relatively fixed and stable routines. Burgelman’s (1994) account of Intel’s exit from the memory business is indicative of this sort of argument. Nonetheless, radical changes even at the level of quasi-genetic traits do occur (Tushman and Romanelli 1985), although their adaptive properties need not be taken for granted, as witnessed by the term biologists chose for them, “hopeful monsters” (Goldschmidt 1940). Similarly, it is important to recognize that although business enterprises can go through periods of rapid change, rapid recombinations of assets may take place without requiring major transformations in underlying routines and capabilities (Teece et al. 1994, Kogut and Zander 1992). Indeed, critical to the notion of punctuated change in the biological context (Gould and Eldridge 1977) is the notion of speciation, which, as Levinthal (1998) argues in the context of technological development, is quite

compatible with incremental evolutionary development of technologies themselves.

6. Conclusion

Management Science has played, and will continue to play, an important role in business strategy research. It is a journal with high academic standards that is not captured by a particular discipline. Rather, the tradition within *Management Science* is to engage important practical problems with rigor and little “d” discipline, the discipline of careful social science, but not necessarily the big “D” discipline of sociology and economics. These underlying attributes and values make *Management Science* a natural home for pioneering research in business strategy.

The journal has encompassed all four corners of our map of the field. Perhaps, most importantly, it has been a home for the growing body of research in the middle of this “map”—research that characterizes actors as boundedly rational, recognizing strategic action as highly situated, and, increasingly, linking across intraorganizational and market processes. One by-product of this middle-ground research is that the schisms that have historically divided the field, such as process versus content research and design versus implementation can be more readily bridged; indeed, seen in this light, such partitioning of the field does not seem sensible. There seems to be considerable promise of a more cohesive field of inquiry, perhaps ultimately a paradigm, emerging from this growing body of research. We look forward to seeing how this promise is fulfilled in the following decades’ contributions.

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