System dynamics and strategy

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Abstract

System dynamics research has made numerous contributions to a range of management subfields, including operations, organization behavior, marketing, behavioral decision making, and strategy. In this paper, we focus on the role for system dynamics research in making important progress on the defining issue in the field of strategy: why are some firms more profitable than others? Strategy researchers are eager for dynamic theories that explain the evolution of performance differences among firms and are increasingly looking to managerial decision making as the source of dynamics. This interest in dynamics and decision making creates an enormous opportunity for system dynamics researchers as carefully grounded behavioral theories of dynamics flow naturally from the research methods of system dynamics. Building and testing theories that explain longitudinal patterns of performance differences among firms would be an enormous step forward where mainstream strategy approaches have struggled. We identify four promising research paths along these lines for system dynamics research in the field of strategy. Copyright © 2009 John Wiley & Sons, Ltd.

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Introduction

Why are some firms more profitable than others? This question is of great importance to investors, public policy makers, and general managers. It is also a challenging one for researchers given the wide range of factors influencing firms, from the actions of individuals to the demands of broad social institutions. Whether due to its practical importance or theoretical richness, the question of why some firms are more profitable than others has attracted researchers from many disciplines. In the process, these researchers have made the journals, conferences, and faculties comprising the field of strategy into a vibrant research community where a wide range of ideas and evidence about firm performance are investigated and debated. We believe system dynamics (SD) researchers can make unique and important contributions to this central question of strategy scholarship and that making stronger connections to this community will be valuable for SD researchers.

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We begin by describing trends we see in the nature of strategy research. We argue that strategy scholars are becoming more and more interested in understanding the dynamic processes that give rise to performance differences among firms. In addition, strategy researchers are increasingly investigating managerial decision making as a source of dynamics. We believe that one of the most vibrant areas of strategy research over the next decade or more will focus on the role that managerial decision making has in creating performance differences among firms over time. This trend—one which we see as very positive for the strategy field—presents a great opportunity to leverage and expand on the strengths of system dynamics research to make important and unique contributions in the field of strategy.

We also argue that SD researchers interested in contributing to strategy scholarship will benefit from making stronger connections to the field of strategy. Strategy is richly multidisciplinary, serving as a home for economists, sociologists, psychologists, and historians, as well as researchers grounded in behavioral decision making and other research traditions. This diverse range of strategy scholars have all found common ground in their interest in explaining performance differences among firms and bring an accumulated wealth of concepts, evidence, methods, and perspectives on which SD scholars can build. In this paper we identify opportunities for SD researchers to draw on and contribute to this community and discuss four threads of ongoing strategy-related SD research we believe hold the most promise. We review existing research in each of these threads and highlight opportunities for extensions focused on explaining longitudinal patterns of performance differences among firms.

The paper is organized as follows. In the next section, we discuss in more depth the trends we believe will shape the future of strategy research. We then identify four promising paths through which SD research can explain longitudinal differences in firm performance. Our intent is not to be exhaustive, but rather to focus on a few paths that we know best and believe will be useful for those who will help shape the future of SD research in the field of strategy.

Towards a dynamic and behavioral strategy field

While there has been considerable progress in developing frameworks that explain differing competitive success at any given point in time, our understanding of the dynamic processes by which firms perceive and ultimately attain superior market positions is far less developed. (Porter, 1991, p. 1)

As strategy researchers we . . . have a number of well-developed theories as to why, at any given moment, it is possible for some firms (and some industries) to earn supranormal returns. As of yet, however, we have no generally accepted theory—and certainly no systematic evidence—as to the origins or the dynamics of such differences in performance. (Cockburn *et al.*, 2000, p. 1123)

As the preceding quotes highlight, strategy researchers are well aware that existing strategy theories and frameworks are far better at explaining performance differences among firms at a particular point in time rather than the dynamics of such performance differences. Research is needed to build compelling explanations for how performance differences among firms arise, persist, and disappear over time. In their summary of the current state of strategy research, the editors of a recent special issue in *Management Science* argued that "the challenge of fully incorporating dynamics into how we think about strategy is a major one, perhaps the biggest one that the field faces going forward" (Ghemawat and Cassiman, 2007, p. 535).

In an attempt to rectify this imbalance, strategy scholars are increasingly seeking to understand the dynamic processes that lead to performance differences (Cockburn *et al.*, 2000; Ghemawat and Cassiman, 2007; Porter, 1991). Work in the strategy field has characterized possible sources of differences and dynamics among firms into two broad classes: luck and differences in purposive decision making (Barney, 1986; Dierickx and Cool, 1989).

Luck, unsurprisingly, is a potentially problematic starting point for explanation in the strategy field. Where luck is the dominant part of an explanation, the success and failure of individual firms become idiosyncratic to the point of being inexplicable and frameworks based on them become "roughly equivalent to ex post accounts of the way in which a winning gambler chose to put her money on red rather than black at the roulette table" (Cockburn *et al.*, 2000, p. 1124).[1]

Focusing on purposive managerial decision making as a source of dynamics and performance heterogeneity is consistent with the fundamental assumption in strategy that managerial decisions and actions play an important role in determining firm performance. Managers make large commitments (Ghemawat, 1991), and they make sequences of more subtle decisions that cause the evolution of resources and competitive positions to differ among firms over time (Dierickx and Cool, 1989). Strategy scholars have started to look more closely at the role of managerial cognition and decision making in firm dynamics (Gavetti and Levinthal, 2004; Ocasio, 1997; Zajac and Bazerman, 1991; Camerer, 2003). The goal of this research is not only to understand how modal patterns of managerial decision making lead to stylized norms of firm dynamics, but critically how heterogeneity in decision making explains heterogeneity in firm performance.

Over the last several decades, a huge literature has developed around strengths, limitations, and empirical regularities in managerial decision making (e.g., Camerer and Lovallo, 1999; Hogarth, 1987; Kahneman and Tversky, 2000; Sterman, 1989b). Given the interest in performance heterogeneity at the center of the strategy field, further research is required to gather systematic evidence of heterogeneity in patterns of decision making and to link these differences to observed patterns in the trajectories of firms over time. Research is needed to address why some managers and not others invest in resources or strategies that will ultimately be associated with competitive success and how

decision-making regularities and heterogeneity among decision makers lead to the distributions of firm performance that exist in our economy.

We believe the growing interest in firm dynamics and managerial decision making presents a great opportunity for SD research to make important contributions to the field of strategy. SD researchers have long been interested in connecting diversity in decision making to performance differences among firms over time. As Forrester noted, we need to understand "what exists in the policies, practices, and attitudes of one group of managers to produce one life history, while other managers, in the same industry with similar products, can have an entirely different . . . behavior" (Forrester, 1963, p. 5). Building and testing theories that explain longitudinal patterns of performance differences among firms would be an enormous step forward in an area where mainstream strategy approaches have struggled and where leveraging the strengths of SD could make unique contributions. We have argued that the strategy field has always been interested in understanding differences in performance among firms. We have also argued that the strategy field is increasingly interested in how firm dynamics and managerial decision making contribute to differences among firm. In the following section we show that the close alignment between these three elements—firm dynamics, managerial decision making, and differences in firm performance—provides a clear opportunity for SD research within the strategy field.

Opportunities for SD research in strategy

Managerial decision making and firm dynamics have always been fundamental to system dynamics research, and—while we believe the potential has been underexploited—explaining differences among firms is hardly a new idea to our discipline. Models of individual firms were long intended to produce, with slight adjustments, the range of behavior observed within broad classes of organizations (Forrester and Senge, 1980). This point was made clearly in an early paper in system dynamics, where Forrester (1964) presented a diagram showing four stylized patterns of firm growth and stated that models should be able to generate all four alternative paths. Forrester has made this point repeatedly, noting that SD models should represent general theories rather than explanations of special cases:

> The primary utility of a theory lies in its generality and transferability. Ohm's law in electricity would have little usefulness if it applied only to one specific electrical circuit, and another law had to be discovered for the next circuit. (Forrester, 1983, p. 6)

In Figure 1, we have adapted Forrester's (1964) diagram of four stylized patterns of firm growth to illustrate how theories explaining longitudinal patterns of performance differences among firms connect with and extend

Fig. 1. Connecting explanations of longitudinal and cross-sectional performance differences among firms
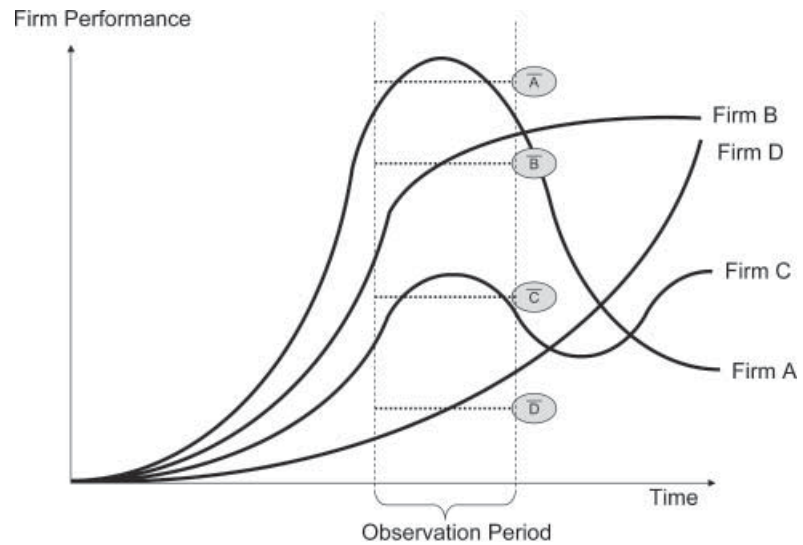
the type of cross-sectional analyses commonly employed in strategy research. The longitudinal patterns are the kinds of results we might expect from an SD model with multiple competing firms or an SD model focused on one firm where we run the model multiple times with changes in managerial policies or other key influences. In addition, we have added two vertical lines indicating the period covered by a hypothetical panel dataset (incorporating multiple observations of a few firms) of the kind often used in strategy research. Panels overcome some of the limits of pure cross-sectional data but, as illustrated by the horizontal lines associated with each firm, most strategy empirical studies use the short longitudinal component as a proxy for static differences among firms that are not observed (so-called fixed effects or random effects) rather than for trying to understand rich dynamic patterns. The findings of such panel data studies can be misleading for systems that are not in equilibrium, as the relationships observed between firm attributes and firm performance will depend not only on when the sample is taken but also on differences in the developmental stages among firms during the period of observation. The clear limitations of such studies certainly help explain their low explanatory power and mixed empirical findings (Agarwal and Gort, 2002). There is a big opportunity for theories explaining the different performance trajectories of different firms over time to reconcile mixed empirical findings from cross-sectional studies and to enhance overall explanatory power. Such theories would naturally explain both longitudinal and cross-sectional performance differences among firms—an enormous step forward where mainstream strategy approaches have struggled.

Those seeking to explain dynamics and heterogeneity in firms as an outcome of managerial decision-making processes have a rich literature to build on. To start with, there is a substantial SD literature on boundedly rational decision making and misperceptions of feedback (e.g., Forrester, 1961; Morecroft, 1983, 1985a, 1985b; Sterman 1989a, 1989b; Sterman *et al.*, 2007). There is also an expansive literature on decision-making processes including studies of organizational routines (e.g., Cohen *et al.*, 1996; Nelson and Winter, 1982; Bowman, 1963; Cyert and March, 1963), institutional logics (e.g., Thornton, 2002), managerial mental models and cognition (e.g., Huff, 1990; Ginsberg, 1990), dominant logic (e.g., Prahalad and Bettis, 1986), and decision biases and heuristics (e.g., Kahneman and Tversky, 2000; Gigerenzer *et al.*, 1999). These research threads span micro-individual, macro-organizational, and supra-organizational levels of analysis and draw from sociology, economics, and psychology. Nevertheless, we remain far from the systematic and exhaustive understanding of human decision making that Herbert Simon envisioned nearly five decades ago:

> Within the very near future—much less than 25 years—we shall . . . have acquired an extensive and empirically tested theory of human cognitive processes and their interaction with human emotions, attitudes and values. (Simon 1960, p. 22)

Research on many fronts is needed to understand both the commonalities and variations in decision-making that shape firm performance. In the following pages, we discuss four broad research paths we believe hold the most promise for making progress on this important agenda. The first is laboratory experiments of individual and team decision making; the second is bootstrapping decision rules using field data; the third is variation in resource accumulation and implementation strategies; and the fourth is dynamics of competitive rivalry. We note excellent prior research and more recent efforts in order to highlight how these paths help us gain insight into the ways decision makers vary and how such diversity leads to longitudinal performance differences.

Laboratory experiments of individual and team decision making

SD researchers have already established a rich tradition of experimental work on managerial decision making. SD research on dynamic decision making has focused on identifying and documenting systematic misperceptions of feedback between decisions and the environment (Diehl and Sterman, 1995; Moxnes, 1998; Paich and Sterman, 1993; Sengupta and Abdel-Hamid, 1993; Sterman, 1987, 1989a, 1989b; Langley and Morecroft, 2004). These experimental studies explore decision making and performance utilizing experimental tasks that consist of management flight simulators or microworlds encompassing an underlying system dynamics model coupled with a "friendly" user interface. To study decision making some feedback loops are cut in these microworlds and individual subjects or groups make one or more decisions each time period based

on the information available in the user interface. This research has contributed to our understanding of widespread deficiencies in individual and team decision heuristics in environments characterized by dynamic complexity.

SD is particularly well suited for such research. Experiments using management simulation microworlds that incorporate feedback, delays, and nonlinearities more closely approximate the decision-making environments of executives than the experimental tasks typically employed in psychological and judgment and decision-making research. The findings suggest that dysfunctional macro-organizational behavior (e.g., poor or puzzling firm performance) can be caused by systematic misperceptions of feedback at the micro-individual level. The outlets and audience for this research beyond the system dynamics community have primarily been the field of judgment and behavioral decision making, though we see a huge potential for extending this work to speak directly to the central questions of strategy.

For example, recent experimental work examines how differences in mental model accuracy, decision rules, and strategies lead to differences in the performance of simulated firms (Gary and Wood, 2008). Results show there is substantial variation in mental model accuracy and that decision makers with more accurate mental models achieve higher performance levels. In addition, estimates of information weights indicate considerable variation in participants' decision rules for managing the simulated firm, with more accurate mental models leading to more effective decision rules. The findings also show patterns in participants' decision rules clustered into a small number of distinctive managerial strategies. There were significant differences in mental model accuracy across these different strategies, and these different strategies accounted for significant variation in performance outcomes. Overall, these findings help explain why some managers and not others adopt strategies that are ultimately associated with competitive success and underscore the potential for future research on mental models.

SD researchers studying managerial decision making in the laboratory can also connect with other strategy scholars who have recently started exploring the extent to which managers reason by analogy (Gavetti and Rivkin, 2005; Gavetti *et al.*, 2005). The potential to transfer insights from commonly recurring archetypes or generic structures has long been a topic of discussion and research in system dynamics (Senge, 1990; Paich, 1985; Lane, 1998). In general, decision makers typically have great difficulty in transferring knowledge from one problem to another, even when the structures underlying the target and the source problems are very similar (Gick and Holyoak, 1983; Markman and Gentner, 1993; Bakken *et al.*, 1992). However, there is evidence of significant heterogeneity in decision makers' ability to recognize and combine deep structural information common to analogous problems and to apply insights and solutions across classes of problems. Studies show that experienced high performers (i.e., true experts) use high-quality mental models of the key principles of the deep structure to solve analogous problems across a variety

of problem domains, including general management (Barnett and Koslowski, 2002), physics (Chi *et al.*, 1981), medicine (Schmidt and Boshuizen, 1993), and many others (Ericsson *et al.*, 1993).

Recent research suggests enhancing the development of high-quality mental models of the key principles or deep structure may facilitate disciplined analogical reasoning and help executives overcome many decision biases on commonly recurring management problems (Gary *et al.*, 2008). A number of high-level, simplified causal models of common management problems and challenges already exist in the SD literature that can be developed into learning platforms. This includes launching new products (Paich and Sterman, 1993; Nord, 1963; Bass, 1969), project management (Abdel-Hamid, 1989; Cooper, 1980; Rodrigues and Williams, 1998; Roberts, 1978; Ford and Sterman, 1998), inventory management in supply chains (Sterman, 1989b; Forrester, 1961), managing commodity production cycles (Meadows, 1970), and others.

Research into managerial decision making and cognition must also be embedded in the broader competitive and institutional context. For example, a recent experimental study explored the role of team decision making in a competitive context. Kunc and Morecroft (2007) used the Fish Banks gaming simulator to study decision making and rivalry among competing teams. While the "tragedy of the commons" depletion of the overall fish stocks is a typical result of the game, the results show there is substantial heterogeneity in firm performance. Team performance varied as a function of the team's own decisions as well as the decisions of other teams. In other words, the competitive context determined whether a given decision rule or strategy would result in superior or poor performance. For example, a highly aggressive team was successful in fisheries where most other teams sold their fleets, but was very unsuccessful when other teams followed the same strategy. The results demonstrate that the effectiveness of any given strategy depends on the strategies adopted by competitors in the market. This shows an even more nuanced way that the distribution of decision rules among competitors affects heterogeneity in firm performance.

Continuing along this path, we see numerous promising directions for future experimental work on decision making. Further laboratory research can help us better understand and document heterogeneity in mental models, decision rules, and strategies across a wide range of decision-making contexts. We also need to identify the conditions in different competitive contexts (e.g., industries with long time delays, nonlinearities, powerful increasing returns mechanisms) that influence heterogeneity in decision making and whether such differences have performance implications. There are also opportunities for further research to examine the formation of mental models and decision rules. How and why do managers develop different mental models, decision rules, and strategies? Experimental studies can also examine the extent to which managerial insight or intentionality explains performance differences among simulated firms. To address the normative objectives of strategy and system dynamics, research is

also needed to design and test the efficacy of interventions targeted at improving mental models and decision making in dynamically complex environments. For example, we need empirical research investigating interventions that facilitate transferring insights across multiple analogous management problems. Finally, we also need research testing and connecting the insights and findings from laboratory experiments with field data on firm performance.

SD models and microworlds provide excellent platforms for identifying important decisions and eliciting the information cues managers use in making those decisions. In the following section, we turn our discussion to the use of field data to estimate decision rules, across a population of firms to examine the impact of these rules on performance heterogeneity.

Bootstrapping decision rules using numerical data from the field

Managers' decision rules can be estimated from data on decisions and the information available to managers at the time they made those decisions. This estimation process, known as bootstrapping, has a long tradition both within and beyond the system dynamics research community (Bowman, 1963; Dawes, 1979; Huber, 1975; Dawes and Corrigan, 1974; Camerer, 1981; Sterman, 1989b, 1988). Most of the bootstrapping research within the system dynamics community involves estimating decision rules from experimental data (Paich and Sterman, 1993; Sterman, 1987, 1989a, 1989b) or a very limited number of cases (Hall, 1976). Bootstrapping, however, has promise for highlighting the generality of case-based modeling efforts and experimental findings by estimating the policies of different firms and analyzing how those different policies produce different patterns of behavior and thus heterogeneity in performance.

For example, recent research applied the decision rules from Hall's (1976) model to a larger panel of women's consumer magazines (Rockart and Mitchell, 2007). The process involved extensive work to assemble longitudinal data on a panel of magazines from 1991 to 2004. Data from the panel were used to estimate decision rules at the various magazines to assess heterogeneity in policies and heterogeneity in the way firms adhere to those policies. This research focused on identifying the rules for each magazine and the performance consequences of loose or tight adherence to simple decision rules. Findings showed that when firms fail to follow their own decision rules closely growth is less and the likelihood of subsequent failure rises.

Further work is underway to tie the rules more closely to variation in firm performance. The estimated decision rules for each magazine are to be inserted back into Hall's (1976) original model for each magazine. The goal is to evaluate how closely changes in model behavior due to different decision rules mimic differences in the performance of the actual magazines. This analysis is highly stylized, but is perhaps the first attempt at a full use of the "family-member test" (Forrester and Senge, 1980). More importantly, tests of this kind build a natural bridge between the models built to explain specific and counterintuitive

firm behavior and general arguments about the causes of heterogeneity in firm performance.

There are many opportunities for future research to contribute to the strategy field by bootstrapping decision rules using field data. Roger Hall's (1976) model of magazines is only one of a dozen or so potential models to which we could apply these methods. Project models (Abdel-Hamid, 1989; Cooper, 1980; Rodrigues and Williams, 1998; Roberts, 1978; Ford and Sterman, 1998), supply chain models (Sterman, 1989b; Forrester, 1961), models of growth and underinvestment, commodity cycle models (Meadows, 1970), service quality models (Oliva and Sterman, 2001), models of punctuated change (Sastry, 1997) and many others could be used as a basis for future research. Existing SD models in these domains all include formulations for decision rules that have been grounded in the empirical context. If studied in industries where the variables in these models have a large chance of influencing firm performance, the work could quickly show that managerial decision making explains far more of the variance in firm performance than other factors such as technology or industry structure. Similarly, there are opportunities to test the decision rules identified in laboratory experiments through bootstrapping decision rules using field data to see whether the rules explain variation in actual firm decisions and whether this variation is an important source of performance heterogeneity among firms. For example, the decision rules identified in experiments for multi-stage supply chains (Sterman, 1989b) and new product launch boom-and-bust dynamics (Paich and Sterman, 1993) could be tested using an appropriate panel dataset from the field.

While bootstrapping research relies on numerical data, SD research has always relied on the numerical as well as the written and mental databases to formulate models of firm dynamics (Forrester, 1992). SD researchers draw on all of these sources of information to identify the system structure, including the stocks and decision-making policies driving the related rates of flow, responsible for the dynamic behavior of interest. Bootstrapping research presupposes such an effort has already been undertaken. In the following section, we discuss a stream of SD field-based research, using the full range of data sources, which is building on the resource-based view (RBV) in strategy to examine how variation in resource accumulation and implementation strategies explains longitudinal performance differences among firms.

Variation in resource accumulation and implementation strategies

System dynamics shares some of the same behavioural and process assumptions as the 'low church' form of the Resource Based View (RBV) in strategy (Levinthal, 1995). This literature originated at almost exactly the same time as the first statements of system dynamics, and is based on case studies that revealed the importance, and some general properties, of the dynamics of resource accumulation (Selznick, 1957; Penrose, 1959). As with system

dynamics, 'low church' RBV emphasises the importance of tangible and intangible firm-specific resource stocks, the associated accumulation processes, and the bounded rationality of managers (Penrose, 1959; Selznick, 1957; Prahalad and Hamel, 1990; Rubin, 1972; Dierickx and Cool, 1989).

However, as the strategy field developed through the 1970s by incorporating ideas from industrial organization economics, industry-level factors became the center of attention (Porter, 1980) and research on intra-firm resources and capabilities waned. The movement back to the RBV firm-level analysis in strategy was driven partly by empirical evidence that industry factors account for only a modest fraction of the variance in firm performance (Rumelt, 1991). Over the last 15 years the RBV has re-emerged as one of the primary strategy theories for explaining persistent performance differences among firms by identifying conditions whereby resources remain valuable, rare, and hard to imitate or substitute (Barney, 1991; Wernerfelt, 1984; Peteraf, 1993; Dierickx and Cool, 1989).

Despite a large and growing body of work, resource-based explanations have left several important issues unresolved. First, RBV scholars acknowledge that it has proven very difficult in complex organizational settings to identify which resources, individually or in combination, account for a firm's success (Foss *et al.*, 1995). Second, much of the empirical RBV research has attempted to identify the handful of very high-level critical resources responsible for superior performance, and has largely ignored the interdependencies and complementarities of a firm's system of resources that typically make them valuable. Third, RBV theory is largely silent about why firms come to possess different endowments of resources and capabilities, and scholars adopting an RBV perspective have struggled to explain how competitive advantages arise and evolve over time. Dierickx and Cool (1989) highlight and theorize about the crucial role of managerial decision making in guiding resource investment flows, but they do not discuss the cognitive models or decision-making assumptions driving managerial investment decisions. In fact, in their own logical analyses, Dierickx and Cool (1989) assume leading firms start with initial superior resource profiles and working from that starting point identify several mechanisms through which leading firms sustain those leadership positions.[3] Similarly, the vast majority of empirical RBV studies have concentrated on the comparative static effect of a firm's resource configuration at a particular point in time rather than examining resource accumulation processes. Such analyses neither explain how differences in resource profiles and performance originate nor why leading firms at one point in time have lost their leadership positions at a later point in time.

A growing stream of SD research has sought to resolve these issues and has drawn on and made contributions to the "low church" RBV in strategy. SD research along these lines has explored a range of strategy topics including start-up firm growth and resource allocation (Morecroft, 1999b), unrelated diversification (Morecroft, 1999a), related diversification (Gary, 2002, 2005),

industry rivalry (Kunc and Morecroft, 2005; Kunc, 2007), revisited the case of the rise and fall of People Express (Morecroft, 2002), and many others (Warren, 1999, 2002; Mollona, 2002).

SD research in this area has leveraged the strengths of the SD approach to develop explanations about the dynamics of firm resource profiles and performance. Good SD practice employs a wide range of techniques for identifying and grounding, in extensive and rich field data, the interdependent system of tangible and intangible resource stocks and associated rates of flow that account for a firm's success (Morecroft, 2007). To address the question of why some, and only some, managers and firms develop valuable resources, SD researchers have sought to identify the decision policies that managers use in actual organizational settings for investing in and allocating resources. Drawing on ethnography, sociology, organization theory, judgment, and decision making, SD has developed a rich set of guidelines and procedures to uncover a management team's dominant logic (Prahalad and Bettis, 1986), elicit mental models, determine the cues managers use in making decisions, and formulate policies for resource investment and allocation (Sterman, 2000).

Recent SD research in this area focuses on identifying variation in resource accumulation strategies among firms through developing resource maps capturing the resource profiles and investment strategies of competing firms (Kunc and Morecroft, 2005). Resource mapping is a form of content analysis in which written documents and reports (e.g., company annual reports) are used to identify top managers' conceptual representation of the firm and likely performance consequences of various actions. For example, a recent study compared resource maps developed from annual reports of two radio broadcasting firms from 1998 to 2000 and found that managers in the rival firms had different views of the resources needed to compete successfully in the same industry and were following different resource accumulation strategies (Kunc and Morecroft, 2005). Prior SD researchers have used content analysis techniques to develop models of influential theories about organizations (Sastry, 1997), and other organization and strategy scholars have studied managerial mental models and decision making using cognitive maps (e.g., Eden and Spender, 1998; Hodgkinson *et al.*, 2004; Huff, 1990). The resource mapping approach builds on both of these traditions, while focusing on identifying differences in resource accumulation strategies among firms.

Another recent study examined the performance consequences of different implementation strategies in related diversification moves (Gary, 2005). Field data from in-depth case studies was combined with existing diversification theory to develop a system dynamics model capturing the process of implementing a related diversification move. Simulation experiments explored the performance consequences of six different implementation strategies using multiple runs of the single-firm model. All of the implementation strategies appear to create value for the first several years, but the failure to invest in shared resources to keep up with rising workloads ultimately undermines

many plausible implementation strategies. The results demonstrate how a firm can destroy value through poor implementation in a related diversification move even when there are significant potential synergy benefits. The simulation experiments comparing different implementation strategies illustrate the importance of investing in adequate shared resources to prevent overstretching as the new business grows. The findings suggest the equivocal empirical results found in the expansive strategy research on this topic may be due to variation unaccounted for in implementation strategies among diversifying firms.

We believe there are many opportunities for future work along this path investigating variation in resource accumulation and implementation strategies. More research is needed to document heterogeneity in resource accumulation strategies among populations of firms, and to identify widespread strategy implementation difficulties. Over the last three decades, much of the scholarly strategy work has been focused on strategy content; that is, identifying *what* strategy or strategies would provide competitive advantage in a given environmental context. A number of well-defined, "big commitment" strategic choices have attracted the attention of strategy content researchers, including mergers and acquisition moves, international expansion, and joint ventures. Research is needed to explore the consequences of different resource accumulation and implementation policies on performance heterogeneity for any of these "big commitment" strategic choices. Theories of what strategies should be adopted also need corresponding theory guiding the implementation process, and such research may well help explain the mixed empirical results of cross-sectional strategy studies that have focused just on strategy content issues. Much more research is also needed to document the degree to which our theories of implementation difficulties are widespread within organizations, and to what extent our models explain observed differences in firm resource profiles and performance trajectories. While a lot of classic RBV work is too far from specific settings, the SD work related to RBV often leverages in-depth fieldwork at a specific setting enabling us to ground our rich dynamic theories by focusing on the successes or failures of resource management at a particular firm. However, we also need to develop these into more general hypotheses about when resources will and will not be managed well and show that our models explain not only the path of a particular organization but also heterogeneity in the paths of various organizations.

There are also opportunities to connect with, draw on, and contribute to other leading strategy perspectives beyond the RBV. In the next section, we discuss the potential for SD research to enhance our understanding of inter-firm competitive rivalry, which has been a topic of much game-theoretic research in strategy.

Dynamics of competitive rivalry

A prominent line of strategy inquiry uses game theory models of competitive rivalry to examine the performance consequences of interdependent decision

making among rival firms (for a review of this work see Saloner, 1991). Game theory analyses have highlighted the importance of information and beliefs about competitive reactions among rivals for understanding the equilibrium consequences of competitors' strategic choices. However, game theory models suffer from several weaknesses which have limited their impact on strategy thinking and practice. One weakness is that the models typically incorporate only a small number of variables in order to remain analytically tractable, and therefore fail to capture the simultaneous choices over many variables that characterize competition in most industries. The models also hold many variables fixed, again to maintain analytical tractability, that in reality would be changing over the relevant time horizons. Finally, the rationality requirements and common knowledge assumptions imposed on the agents of the game are usually optimistic (Porter, 1991).

Recent SD work has started to investigate and focus on the competitive interactions between a firm and its rivals (Rockart, 2000, 2007; Oliva *et al.*, 2003; Lenox *et al.*, 2006, 2007; Sterman *et al.*, 2007). Research along this path directly addresses heterogeneity in performance outcomes generated through rivalry. For example, a recent paper examines the performance consequences of decision making among rival firms through simulation experiments to evaluate the efficacy of "get big fast" strategies compared with more conservative strategies in a new, emerging duopoly industry with strong increasing returns (Sterman *et al.*, 2007). Sterman *et al.*'s analysis explores the conditions under which disequilibrium dynamics and limited information-processing capability result in different conclusions from models assuming full rationality. The results demonstrate that when capacity adjustment is instantaneous or managers have perfect foresight to maintain capacity utilization at the desired level, boundedly rational firms reach the equilibria predicted by neoclassical economic models and the aggressive "get big fast" strategy outperforms a conservative rival. However, when market dynamics are rapid relative to capacity adjustment, forecasting errors lead to excess capacity and the costs of excess capacity exceed the benefits of increasing returns in the aggressive "get big fast" strategy; the conservative strategy is preferred.

Another example study explores heterogeneity in performance and practices among competing firms in the investment banking industry (Rockart, 2007). Reinforcing feedback effects for social learning were found to underpin stable differences among competing firms' capabilities. The results suggest the typical normative advice regarding learning curves—to set prices low and get big fast—does not apply to quality-driven social learning processes. Unlike quantity-driven learning, social learning implies that firms cannot develop or maintain capability advantages through aggressive pricing and must carefully restrict market share. The contrast between the implications of quantity- and quality-driven reinforcing feedback processes highlights the importance of considering context when analyzing reinforcing feedback processes. These results emerged from comparing the development trajectories of competing

firms in a multi-firm model grounded in case research of the kind common in system dynamics.

Overall, we see a huge opportunity for future SD work in the strategy field to identify and document general insights into settings where heterogeneous actors compete with one another and where competition amplifies heterogeneity. Such work leverages the great strength of the SD method to span multiple levels of analysis. More research is needed to explore interdependent decision making among rival firms to understand the conditions under which disequilibrium dynamics and bounded rationality may lead to different normative conclusions from those of traditional game-theoretic models (Sterman *et al.*, 2007). A sustained stream of research along these lines could help build a dynamic, behavioral game theory that overcomes many of the limitations of traditional game-theoretic analyses. Settings with high dynamic complexity (i.e., long time lags, powerful positive feedbacks, and nonlinearities) are potentially fruitful contexts where SD results are most likely to differ from those of standard equilibrium analyses. Research in this area is likely to benefit from starting out exploring competitive interactions in duopoly cases, as did Sterman *et al.* (2007), before expanding to include more competitors. In addition, research is needed to understand how the combination of managerial decision making and resource accumulation leads to the emergent dynamics of inter-firm competition and performance observed in cross-sectional studies. Lastly, there is an opportunity for future research on this path to explore the structures within firms that amplify small chance differences among rival firms into substantial and persistent differences in firm performance.

Conclusions

In this paper, we have argued that the strategy field is increasingly interested in understanding the dynamic processes that give rise to differences in firm performance over time, and how heterogeneity in managerial decision making influences those processes. We argued that this created a great opportunity for system dynamics research to draw on and contribute to the rich intersection of disciplines represented in the strategy field by developing explanations for longitudinal patterns of performance differences among populations of firms. Table 1 provides an overview of the four promising research paths we have discussed and highlights the big opportunities we see for future research contributions to the strategy field in each path. This includes laboratory experiments of individual and team decision making to ground behavioral assumptions in our models and inform propositions about how differences in decision making lead to performance heterogeneity among firms. It also includes field research that estimates decision rules using panel data gathering systematic evidence of the diversity and distribution of managerial decision making in organizations and the associated impact on performance. We also

Table 1. Overview of four promising paths for SD research contributions to the field of strategy

Research stream	Opportunities to make important strategy contributions
Lab experiments in individual and team decision making	• Identify microstructure decision-making processes responsible for macro performance differences in representative management simulations as testable propositions for fieldwork
Bootstrapping decision rules using panel data from the field	• Document and explain heterogeneity in decision rules of different firms across a wide range of industry contexts and show important connections to performance differences
Variation in resource accumulation and implementation strategies	• Investigate and document the role of heterogeneous resource accumulation and implementation strategies in explaining performance differences among firms
Dynamics of competitive rivalry	• Build a dynamic, behavioral game theory to overcome many of the limitations of traditional game theoretic analyses

believe there are many opportunities for investigating variation in resource accumulation and implementation strategies; this variation is unaccounted for in much of the strategy content research and may help explain mixed empirical results in several areas. We also see great potential for far more work investigating competitive rivalry, where interdependent decision making among rival firms leads to heterogeneity in outcomes.

We propose that SD is not only well positioned to identify how decision making drives dynamics and leads to performance heterogeneity, but also that it is better positioned to do so than many alternative research methods. Think about the task facing researchers who have traditionally focused on equilibrium explanations and cross-sectional evidence. They must develop dynamic explanations for firm heterogeneity almost from scratch. In contrast, the SD researcher has a long tradition of rich, dynamic explanations of firm behavior to draw on and established methods for developing new theories to leverage. As SD researchers, what we need is to extend the scope of our research to explicitly and rigorously investigate differences among decision makers and firms. Admittedly, building and testing generalizable dynamic theories is not an easy task whichever direction one is coming from, but for those interested in making these connections the direction facing SD researchers appears easier and faster.

We also emphasize that SD researchers interested in making contributions to the field of strategy should also draw on and connect with relevant existing strategy theories and empirical evidence of performance differences among firms. Our SD colleagues have reminded us to pay attention to the questions, literature, and language of the scholars we seek to influence (Repenning, 2003). Knowledge of the related strategy literature for our specific research

topic helps us to understand the areas where contributions are most needed, benefit from decades of cumulative research focused on explaining performance differences among firms, frame the novelty of new insights, and communicate SD findings more forcefully and efficiently to a strategy audience.

Although we did not discuss this in the text, we also reiterate the message other SD scholars have already highlighted regarding the benefits of simplifying SD models and theories to more powerfully and persuasively communicate the key insights (Repenning, 2003). In addition, we suggest there may be similar benefits to subjecting our dynamic theories to simplified tests. Most other strategy researchers empirically test highly abstracted models even when they begin with rich theories. With full expectation that these abstractions will leave a great deal of variance unexplained, they develop datasets with observations of many organizations and rely on large-sample statistical inference tools to discriminate among competing abstracted explanations. Similarly, we argue that SD scholars can leverage our rich models and theories by identifying important relationships and behavior patterns that can serve as a basis for statistical evaluation in numerical datasets that are available for larger numbers of firms. This requires a willingness to abstract key ideas from our models and test those ideas independently of our models in a manner that we have rarely chosen to do as a field, but for which there are no serious impediments. Testing the generality of important relationships and behavior patterns will not only be useful for refining our models, but also as compelling evidence of their relevance to investors, public policy makers, and general managers.

It is our hope that this paper does more than call for more good work in SD. We see a great opportunity for SD research to develop explanations for the observed longitudinal patterns of performance differences among firms. Such work addresses an important issue for policy makers, shareholders, and general managers, and would make enormous contributions to the strategy field. We hope that the avenues discussed in this paper will spark ideas for leveraging existing SD research and conducting novel studies.

Notes

1. Over the past several decades researchers have identified many processes that amplify small and often unintentional differences (i.e., luck) among firms into very consequential differences in firm performance. Amplification happens through search over time (Levinthal, 1997) as well as through a variety of reinforcing processes at both the firm and industry level (Sterman, 2000, Ch. 10). We believe continued research into processes that amplify small differences will be fruitful, but we focus on the opportunities for research on differences in purposive decision making where we see even greater potential for contributions.

2. SD researchers will likely find it helpful to distinguish between the "high church" and "low church" RBV perspectives (Levinthal, 1995). The "high church" RBV adopts assumptions of objectively rational economic man and efficient equilibria, and handles resources with very much the same static approach that industrial organization economics research takes in theorizing and analyzing market positions (Barney, 1991, 2001).
3. Dierickx and Cool (1989) highlight the same conceptual distinction between stocks and flows (or levels and rates) found in SD, but do not build formal models of resource accumulation processes. Instead, they present highly stylized arguments about the features of stock and flow accumulation structures that help maintain competitive advantages of leading firms. This work has been widely recognized in strategy.

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