

## **Foundations and Futures of Strategic Management**

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### **Abstract**

Research in strategic management has burgeoned in the last few decades. This growth and specialization not only reflects impressive progress but raises questions about the boundaries and future direction of the field. In an effort to promote the accumulation and integration of insights regarding strategic management we trace research in the field through a series of four generations of scholarship. In so doing, we highlight some of the most important challenges and contributions of each of these generations and note the tensions and opportunities presented by the field's focus on fundamental issues and frontier research topics. We also note the various integration mechanisms that have been called for and discuss their merits and demerits. We conclude by discussing the role of the *Strategic Management Review* as a complement to existing strategy journals.

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## Introduction

By almost any measure, the development and progress of the strategic management field has been a remarkable success story. The affiliated division of the Academy of Management (AoM) constitutes over 5,000 members—making it the 2<sup>nd</sup> largest division within the AoM in terms of membership. In the span of some thirty years, membership in the Strategic Management Society has tripled to over three thousand members. The *Strategic Management Journal*, the field's flagship journal, attracts well over fifteen hundred submissions per year and is cited over thirty thousand times per year. Today the field enjoys legitimacy in many business schools and universities. Strategic management is now closely associated with MBA and executive education programs across the globe.

Along with this progress and impact, it is useful to reflect upon some of the unique accomplishments across several “generations” of strategy scholarship. Such a brief retrospective identifies some of the key challenges each generation has faced and provides inspiration on ways that scholars might address some of the key opportunities present in the field today. The generations of scholarship follow in rough chronological sequence, but there are overlaps. We provide illustrations to make sense of the field today and do not attempt a comprehensive review. Rather, we intend to capture and convey, however imperfectly, some of the broad themes and characteristics of strategic management research over the years. We also direct those new to the field to several excellent histories and reviews for more in-depth treatments of streams of literature and the field's intellectual foundations and antecedents (e.g., Mintzberg, 1990; Rumelt, Schendel, and Teece, 1991, 1994; Mahoney and McGahan, 2007; Ghemawat, 2016; Drnevich, Mahoney, and Schendel, 2020). In developing this essay, we have also carried out a brief survey involving senior scholars in the field and will call upon some of their ideas and insights as well

as other data to suggest opportunities for research in coming years. We flag the integration of our research efforts and the construction of a robust, cumulative body of knowledge as key opportunities facing the field, and highlight a number of different avenues that could promote this aim. In particular, we discuss the role that the *Strategic Management Review* can play and lay out its unique positioning among the field's journals.

### **Generations of Strategic Management Scholarship**

**Generation 0.0: Birth.** Defining the origin of strategic management is not as straightforward as it might seem. At one extreme, MBA students and faculty members outside the field sometimes express the mistaken view that it all began in the 1980s with the popular Five Forces analysis that is featured in early sessions of many strategic management courses. At the other extreme, Freedman (2013) sees the origins of strategy in the Old Testament, evolutionary theory and animal interaction, ancient Greece, and the proverbs of Sun Tzu, among other sources outside of business. So it is difficult to depict an orderly, linear history. It also quickly becomes evident that the term “strategy” – an admittedly amorphous construct and rather esoteric concept – and “strategic management” are not coterminous, even if the former is often used simply as a shorthand for the latter. In our view, a more nuanced examination reveals the field's rich history and historical willingness to draw on the worlds of management practice, teaching, and research in several allied fields.

Reflecting on the history of the field, Rumelt, Schendel, and Teece (1994) discuss the importance of the policy course in business schools as well as early writing on strategy by consulting firms going back to the 1930s. Ultimately, these authors settle on Chandler's (1962) landmark book on strategy and structure as providing the foundation of modern strategic management. In so doing, they emphasize the central roles played by organization (e.g., structure

and various processes such as formulation, implementation, evaluation, and control); they underscore the importance of “management” in strategic management. If the 1960s provided birth to this field, early conceptual foundations were also provided at that time by Ansoff’s (1965) volume on corporate strategy and the Learned, Christensen, Andrews, and Guth (1965) textbook that is closely associated with Kenneth Andrews. The role of management practice, as well as the centrality of the general manager in pursuing organizational effectiveness (i.e., doing the *right things*), therefore became hallmarks of strategic management research from the beginning.<sup>1</sup>

For scholars of strategic management, a conference organized in 1977 by Dan Schendel and Charles Hofer provided an important catalyst for the field’s research agenda as well as the impetus for what would become the field’s main institutions a few short years hence. The volume that followed this event catalogued some of the most pressing research needs and opportunities, organized into eighteen different topical areas (Schendel and Hofer, 1979):

1. Strategy Concept
2. Strategic Management Process
3. Boards of Directors: The External-Internal Interface
4. General Management Roles in Strategic Management
5. Goal Formulation and Goal Structures
6. Social Responsibility
7. Strategy Formulation
8. Environmental Analysis
9. Strategy Evaluation
10. Strategy Content
11. Formal Planning Systems
12. Strategy Implementation
13. Strategic Control
14. Entrepreneurship and New Ventures
15. Multibusiness/Multicultural Firms
16. Strategic Management in Not-for-Profit Organization

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<sup>1</sup> At a panel session of the 2017 Strategic Management Society meeting in Houston, Joseph Bower of Harvard Business School aptly made the distinction between operations research and strategic management, noting that strategic management is “not about the optimal distance between telephone poles.” Rather, it concerns complex, interdependent, and ambiguous decisions that lead the firm to “do the right things.”

17. Public Policy and Strategic Management
18. Research Methods in Strategic Management

The research needs and opportunities identified in the volume emerged partly from a Delphi study involving a panel of 56 authors, discussants, and moderators who attended the famous “Pittsburgh conference” in May of 1977. A handful or more specific research questions were identified in each of these eighteen areas, providing ample fodder for many future dissertations and research careers. Examining the list today, some categories for research might appear anachronistic, as organizations have moved away from formal planning systems, for instance. However, we are also struck by the extent to which this list of topics has stood the test of time. Some areas such as the interplay between public policy and strategic management seem to be especially prescient as they have received far less research attention relative to the potential payoffs they present for research, management practice, and society at large.

Whereas recent scholars have tended to define their identities in terms of specific theories or phenomena rather than these broader categories of inquiry, Bettis and Blettner (2020) note that the scholars at the Pittsburgh conference were not merely interested in questions unique to the strategic management field, but they were committed to establishing a new department or discipline in their business schools, altering core requirements in MBA programs, and building new PhD programs devoted to strategic management. They were revolutionaries. It might be difficult for current students to imagine the importance of these individuals for the development of the strategic management field or to appreciate the stakes involved, given the status of the field today. Bettis and Blettner (2020) summarize the academic landscape and developments in this way:

Some of the scholars at Pittsburgh paid a substantial price for their academic apostasy in terms of stalled or delayed promotions and/or minimal salary increases for years, but by the early 1990s Strategic Management had largely won, not just

won, but had become wildly successful in firmly implanting the field of strategic management as a fundamental teaching and research discipline/department in business schools. Without the intense work of a few dozen academic revolutionaries just discussed in the Business Policy and Planning Division of the AOM, the participants in the Pittsburgh Conference, and Dan and Mary Lou Schendel we likely would not have a successful and highly respected business school discipline.

### **Generation 1.0: Crystallization of Canonical Problems of Strategic Management.**

The launch of the *Strategic Management Journal* in 1980 and Strategic Management Society in 1981 provided much-needed infrastructure for the aspirations expressed at the Pittsburgh conference. These early discussions made it clear that the field was not only pre-paradigmatic in Kuhnian terms, but its very nature would require that it draw upon multiple disciplinary traditions, ranging from economics to sociology, as well as psychology, political science, and others to solve the practical problems executives face. In addition to the need for additional conferences and journal space to meet the level of interest in the questions raised by the field, the attributes of the research needs and opportunities in the field implied that these outlets would need to be managed differently.

The next generation of strategy scholarship we mark using Rumelt, Schendel, and Teece's (1994) work on the fundamental issues of strategy. In this volume the authors argued that strategic management would not benefit from a single, unifying paradigm, but rather an articulation of fundamental issues that could help direct and refocus research. The premise is that a fundamental question could help to define a field and guide scholars in that domain. For instance, for Coase the fundamental question was, "why do firms exist?" and similar defining questions can be identified in other fields.

The enduring value of a fundamental question lies in its ability to point to a potential way forward that may orient and unify research effort. It can differentiate a field and potentially

challenge others (e.g., Coase's question challenged neoclassical economics). Rumelt, Schendel and Teece (1994) suggested that some questions are too general, and others are not easily connected to usable theory or methods to make them tractable. They puzzled about the nature of organizations and their decision-making; about how competition affects their nature; about the role of management in influencing the value or costs of organizations; and about the role of complex, global environments in influencing business dynamics. This led them to identify four fundamental issues or questions that uniquely define the strategic management field:

1. How do firms behave? (i.e., do firms really behave like rational actors and, if not, what models of their behavior should be used by researchers and policy makers?)
2. Why are firms different? (i.e., what sustains the heterogeneity in resources and performance among close competitors despite competition and imitative attempts?)
3. What is the function of or value added by the headquarters unit in a diversified firm? (i.e., what limits the scope of the firm?)
4. What determines success or failure in international competition? (i.e., what are the origins of success and what are their particular manifestations in international settings or global competition?)

While the fundamental issues in strategy are certainly related to theories available in the disciplines and the topics covered in other fields in the business school, they also retain distinctive properties. In an afterward, Rumelt, Schendel and Teece (1994) also discussed the importance of organizational inertia and how the policy process matters. They also covered several “also ran” questions (i.e., Are their strategies? How do industries evolve? How is organizational competence generated and sustained?), but did not highlight them as fundamental

issues given a lack of sufficient empirical research foundations at the time, or an inability to compel future research with adequate clarity.<sup>2</sup>

Over twenty-five years later, it is worthwhile to reconsider how these defining questions fueled research in strategic management and contributed to the field's identity. In the current era in which many studies discussed in strategy conferences and published in strategy journals are not clearly connected with these canonical problems in strategic management, the fundamental issues raise several important questions for individual researchers and the field more generally: (1) How, if at all, should these fundamental issues be revised twenty-five years later given changes in organizations and their environments (e.g., challenges to capitalism, the advantage of artificial intelligence, etc.)? (2) With the ever-widening topical scope of strategic management research, to what extent will strategy research risk becoming "second class" finance, economics, sociology, etc. research? Can we define what makes a topic strategic? Or, alternatively, does the field need to express its research and boundaries in more theoretical terms? (3) With the field's new emphasis on applications of disciplinary theory and methods, how are strategy scholars learning about (and from) managerial practice on these or other fundamental issues? (4) Ultimately, how can the identity of strategic management research best be articulated and preserved?

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<sup>2</sup> Classification schemes of practical topics and questions underlying managerial decisions have also guided executives and teaching in strategy courses. For instance, in developing and executing strategy, general managers are encouraged to think through four specific components of strategy within a coherent economic logic: (1) Arenas – in which markets will the firm be active (e.g., product markets and countries)? (2) Vehicles – how will the firm get there (e.g., through exports, licensing, wholly-owned plants, joint ventures, etc.)? (3) Differentiators – how will the firm win versus close rivals? and (4) Staging – what will be the pacing and sequencing of commitments? (Hambrick and Fredrickson, 2001). These questions are connected with fundamental questions concerning competitive advantage and the scope of the firm. They also address the twin problems of competitive strategy and corporate strategy. Competitive strategy concerns how firms gain and sustain a competitive advantage. Corporate strategy concerns the management of the multi-business firm and its associated boundary-of-the-firm choices in product, factor, and geographic markets (e.g., market entry and exit, restructuring, acquisitions, partnerships, foreign direct investment, outsourcing, etc.). More generally, research in the field addresses how organizations can create and capture value through their actions in markets and resource allocation decisions.



The topics addressed in strategic management research are not and should not be static, as the field ultimately is one of application and therefore reflects the current realities of organizations and their environments. However, clarity is also needed on the distinctive contributions and boundaries of strategic management, both in journals as well as for the sake of business school structures and programs. As we will see, in more recent generations of strategic management research, scholars began to make new and important advances in theory application and development, and this led to many new frameworks and tools for research that could also guide practice. Substantial progress was also made in research methods and the field's empirical base, but with these advances came new challenges and opportunities as the field grew and became more specialized within different research areas.

**Generation 2.0: Deepening Theoretical Foundations and Developments.** The development of a series of fundamental issues focused attention on a set of core questions that bounded the strategic management field. The existence of these questions also spurred efforts to more systematically develop and test arguments surrounding these questions. Prior to the 2<sup>nd</sup> generation of strategic management research, it was unclear whether the field enjoyed a widely-accepted unifying theoretical framework or even a series of widely-accepted theories or hypotheses. And, without a solid theoretical foundation, it was difficult to connect the empirical observations that were being generated as researchers flooded to the field and its empirical base expanded. The strategic management field at this time was akin to the field of biology prior to the development and acceptance of Darwin's evolutionary theory of natural selection—accumulating a series of facts that allowed categorization and labelling of phenomena but unable to connect the dots or make robust predictions.<sup>3</sup>

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<sup>3</sup> Muthukrishna and Henrich (2019) state, “without an overarching theoretical framework that generates hypotheses across diverse domains, empirical programs spawn and grow from personal intuitions and culturally

The development of a theoretical foundation was needed to better articulate the organizational systems we study, to “dimensionalize” the relevant parts of these systems, and to propose the relevant relationships between these parts. In its second generation, the strategic management field developed several theories that aimed to explain questions underlying the fundamental issues. These theories may be placed into three broad categories: (1) theories of competitive advantage, (2) theories of firm organization, and (3) theories of resource allocation and strategic investment. In the first category, seminal contributions such as Caves and Porter (1977) and Porter (1980) highlighted how mobility barriers and industry structure affect firm behavior and performance; contributions by Barney (1986) and Lippman and Rumelt (1982) suggested how factor market conditions might lead to competitive advantage across close competitors; and contributions by Wernerfelt (1984), Barney (1991), and Peteraf (1993) clarified how differential endowments of resources might be leveraged to generate temporary or persistent competitive advantage. Other competence-based perspectives highlighted important differences across firms in terms of their knowledge bases and capabilities (e.g., Kogut and Zander, 1992; Henderson and Cockburn, 1992), routines (e.g., Nelson and Winter, 1982; Helfat, 1994; Klepper and Simons, 2000), and dynamic capabilities (e.g., Teece, Pisano, and Shuen, 1997). Related research considered the implications of cognition (Porac, Thomas, and Baden-Fuller, 1989; Barr, Stimpert, and Huff, 1992; Tripsas and Gavetti, 2000), attention and dominant logic (e.g., Prahalad and Bettis, 1986; Ocasio, 1997), and organizational learning (Levitt and March, 1988; March, 1991; Levinthal and March, 1993).

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biased folk theories. By providing ways to develop clear predictions, including through the use of formal modelling, theoretical frameworks set expectations that determine whether a new finding is confirmatory, nicely integrating with existing lines of research, or surprising, and therefore requiring further replication and scrutiny. Such frameworks also prioritize certain research foci and, often, provide a natural means to integrate across the sciences.”

In the second category, concerns regarding conditions associated with adverse selection, moral hazard, and shirking (e.g., Alchian and Demsetz, 1972; Holmstrom, 1979; Hart, 1995) as well as specific investment, quasi-rents, and the potential for hold-up by potentially opportunistic partners (e.g., Klein, Crawford, and Alchian, 1978; Williamson, 1975) led to well-received theories regarding the way in which markets and hierarchies were influenced by, and worked to mitigate, opportunistic behavior (Williamson, 1985; Mahoney and Qian, 2013). These theories fostered work in strategic management relying on the “discriminating alignment” between the attributes of transactions and discrete forms of governance (e.g., Williamson, 1991).

In the third category, interest in why firms differed in their resource allocation and organizational decisions led to research regarding how aspirations and goals, information asymmetries, and power differences across firms and levels within an organization affected decision-making (Cyert & March, 1963; Simon, 1976), the organizational processes that guide investment opportunities (e.g., Bower, 1970; Burgelman, 1983). Other research considered how differences in processes may influence search (e.g., Katila and Ahuja, 2002) or how organizations can create real options to make further investments on possibly favorable terms (e.g., Myers, 1977; Kogut, 1991; Kogut and Kulatilaka, 1994). Related research on the timing of investment by firms highlighted when commitment rather than flexibility is valuable (e.g., Lieberman and Montgomery, 1988; Ghemawat and Ricart i Costa, 1993).

It is notable to consider the extent to which the papers listed above deepened our understanding of core strategy problems and were recognized with citations and research awards. Many of these papers continue to receive hundreds or thousands of annual citations. Moreover, they also continue to influence contemporary research. For instance, recent work has clarified and expanded on classic ideas regarding industry structure by suggesting how competitive

interactions across competitors, cooperators, and suppliers affect the creation and distribution of value across participants in a value net or ecosystem (e.g., Brandenburger and Stuart, 1996, 2007; MacDonald and Ryall, 2004; Chatain and Zemsky, 2011). Research highlighting the importance of capability identification, selection, and creation (e.g., Teece and Pisano, 1994; Teece, Pisano, and Shuen, 1997; Helfat and Peteraf, 2009) or underscoring the role of culture and organization in coordinating activities (e.g., Kogut and Zander, 1992; Grant, 1996) may be linked to early “resource-based” research. Efforts to refine our understanding of various theories of the firm (Gibbons, 2005), to link capabilities and organization (e.g., Poppo and Zenger, 1998; Foss and Foss, 2005), or to highlight the problem solving abilities of different forms of organization (Nickerson and Zenger, 2004; Felin and Zenger, 2016) may be seen as extending earlier research regarding the scope of the firm. Research integrating behavioral assumptions and real options models offers the potential to more rigorously explore how aspirations, bias, and various forms of uncertainty affect firm behavior (Posen, Leiblein, and Chen, 2018) and organization (Trigeorgis and Reuer, 2017). The above examples are merely illustrative of the theoretical contributions that characterized our second generation of strategic management research and how they are influencing current research.

The second generation of strategic management research advanced the field in at least three ways. First, the development of strong theories helped other researchers prioritize research activities and questions. Rigorous theory not only provides propositions for empirical tests, it suggests clear and falsifiable hypotheses that ease the burden on subsequent empirical research. While inductive research and careful observation also lead to fruitful insights (e.g., as in Alexander Fleming's discovery of penicillin in a Petri dish), it is difficult to imagine publishing a “negative” finding without the support of a clear theoretical prediction. Yet, in the presence of a

clear theoretical argument, a “negative” finding may have important ramifications for the assumptions underlying the theory, its boundaries, and/or opportunities to clarify why a result was not found. For instance, real option theory implies that firms may form international joint ventures (IJVs) in an effort to reduce downside risk by staging commitments (e.g., Kogut, 1991). Yet, early empirical research indicating that the formation of IJVs did not mitigate but actually increased measures of downside risk (e.g., Reuer and Leiblein, 2000) suggested the need to explore a firm’s ability to properly manage the flexibility ascribed by an option approach in a complex, multinational setting.

Second, given the use of aggregate concepts such as the “firm” in organizational studies, the development of theory helped the field to define the critical parts of the industrial system and to “dimensionalize” the important elements underlying these individual parts. For instance, transaction cost, property rights, knowledge-based and problem-solving theories of organization emphasize different dimensions or elements of the firm. Quite naturally, this raises questions regarding whether the overall effects of organization are due to incentive intensity, administrative control, and/or dispute resolution regimes as in Williamson (1991) or a broader set of issues emphasized in other theories such as loyalty (Simon, 1976) or identity and communication codes (Kogut and Zander, 1992; Monteverde, 1995).

Third, there are good reasons to believe that the theories developed in the second generation of strategic management research benefitted practice. While the negative externalities associated with atheoretical research are not always imposed on researchers (whose careers may actually be advanced by publications of erroneous results), they do affect practitioners who choose to apply practices based on these results. For instance, in the field of medicine, much has been made of the causes of false research findings (Ioannidis, 2005). Huded, Rosno, and Pradad

(2013: 30) cite several examples of how incorrect findings adversely affected patient care, including “the use of hormone replacement therapy for postmenopausal women, stenting for stable coronary artery disease, and fenofibrate to treat hyperlipidemia.” A more robust theory may lead to a more cautious dissemination of incorrect “findings” into practice. We see no reason to believe that the field of management is immune from similar challenges.

**Generation 3.0: Specialization and Identification.** We mark the third generation of strategic management research by changes in the way that strategy scholars collect and analyze data. In particular, we highlight the field’s responses to the identification revolution in the social sciences at large.<sup>4</sup> As is now familiar, the identification revolution drew attention to the limitations of employing statistical techniques such as multivariate regression to assess the causal structure underlying associations of interest. While scholars have long understood the tradeoffs associated with the use of various research methodologies<sup>5</sup> and the challenges of drawing causal inferences from regression estimates without the benefit of random assignment, research employing quasi-experimental research designs gained prominence in the 1990s (Angrist and Pischke, 2009, 2010).

In strategic management and allied fields that often rely upon the use of observational data, improvements in methodology led researchers to shift their research designs to those that helped to elicit causal inference (e.g., use of instrumental variables and two-stage models, natural experiments, difference-in-difference models, regression discontinuity models, field

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<sup>4</sup> Several related issues have been raised throughout the social sciences. These include the replication crisis—the challenge scholars in many disciplines are having replicating or reproducing existing findings (Ioannidis, 2005; Camerer *et al.*, 2016, 2018)—as well as other issues such as data mining, harking, ‘p-hacking,’ and other questionable research practices.

<sup>5</sup> Even more broadly, the trade-offs across various means of testing theory were evident. For instance, McGrath (1981) notes that lab experiments trade-off generalizability and realism for improved measurement, formal models trade-off measurement and realism for improved generalizability, and field studies trade-off generalizability and measurement for realism.

experiments). Often, the introduction of these tools helped to clarify inconsistencies between theory and existing evidence. For instance, Shaver (1998) noted that strategic choice is self-selected and therefore empirical models that regress performance measures on these choices are likely misspecified. Shaver (1998) demonstrates how accounting for self-selection in the choice to enter a foreign market via acquisition or green-field affects foreign direct investment survival—a finding that helped explain the fragility of prior findings regarding the overall consequences of entry modes into foreign markets. In so doing, Shaver (1998) spurred additional research that addressed inconsistencies between theory and empirics and clarified the consequences of endogenous strategic choices such as the make or buy decision (Leiblein, Reuer, and Dalsace, 2002).

Another notable paper that demonstrates the benefits of a good identification strategy is provided by Natividad and Rawley (2016). Their study leverages an external shock in the Peruvian fishing industry to provide differences-in-differences estimates of the link between organizational scope and productivity. This approach allows Natividad and Rawley to demonstrate how a reduction in organizational scope affects firm level productivity and enabled these authors to partially reconcile conceptual discussions regarding the positive and negative effects of organizational interdependencies. As they state, “firms appear to design their organizational systems to take advantage of positive interdependencies between activities, by replacing markets with hierarchies,” yet in so doing, “the firm also creates negative interdependencies that will tend to persist over time.” (Natividad and Rawley, 2016: 29).

Chatterji, Findley, Jensen, Meier, and Nielsen (2016) provide two excellent examples of the use of field experiments to advance strategic management research. In their first field experiment, the authors test whether an indicator of the willingness of US cities to allocate

financial incentives offered to a corporation promising to relocate to their city varied with the firm's (presumed) country of origin. In addition to finding limited evidence of discrimination against foreign firms, the study helps overcome measurement and selection challenges that confound many studies of the liability of foreignness using observational data. In the same paper, Chatterji *et al.* (2016) present a process experiment that explores whether and how organizational culture influences firm performance. This experiment tests the *process* underlying why we think culture might affect performance by manipulating employee identity and exploring its impact on cooperation. In both studies, Chatterji and colleagues demonstrate how the creative use of field experiments can help us address important strategic management problems with complex underlying causal structures.

While the arguments underlying the identification revolution are plainly correct from a technical perspective, a singular focus on causal identification might also have unintended consequences in altering the research questions we study and our understanding of the strategic management of organizations. In our view, there can be two adverse byproducts of the “identification revolution.”

1. A singular focus on identification may lead scholars and editors to pass on research questions less amenable to tight identification strategies (e.g., due to the unavailability of a ready experiment or instrument) in favor of studies that have identification pinned down.
2. The focus on identification may lead scholars to overstate the importance of findings employing research designs that are in and of themselves imperfect. The fact that one is able to identify *exogenous* variation is independent from the question of whether the study is informed by prior research, informs managers, or even if that variation is *useful* or *interesting* for the study.

The first effect concerns the breadth of questions we study. We need to understand that many of the choices that define our field – or that make a decision strategic—cannot be



randomly assigned. Given the complexity of organizations and the nature of strategic management research, it is not always possible to identify an exogenous shock to support a differences-in-differences design or a viable instrumental variable research design (Helfat, 2018). The second effect concerns the quality of the insights generated from studies using quasi-experimental research designs—it recognizes that the identification strategies we employ are partial solutions to the problem. The phenomena we study are innately embedded in dynamics of reciprocal causation. It is only through our theories that we are able to impose a structure on data—to label one variable as a “dependent” variable and others as “independent” variables (Bettis and Blettner, 2020). The existence of a tight identification strategy does not mean that the paper advances the theory or practice of strategic management.

While taking causal identification seriously is important, we must recognize that many instrumental variables are imperfect, many of our efforts to match observations are imperfect, and our causal methods are imperfect. It is not always possible to develop a tight identification strategy to address a question of central importance to the strategic management field. Thus, we need to think more broadly about generating theory on canonical questions and carefully testing our theories as a collective. We need to recognize the importance of developing clear theory to set the context for empirical research and value how the insights gained from demonstrating the existence of empirical associations shapes the way we think about strategic issues. We hope the *Strategic Management Review* can complement other journals by allowing scholars to publish provocative essays and theoretical ideas on canonical strategic management questions, that this work will inform managers of interesting ways to think about important issues, and anticipate that the best of our conceptual work may be tested by other papers published in other journals using careful causal identification strategies.

## **Opportunities for the Future: The Voice of the Field**

In May of 2017, we distributed a brief, informal survey to a convenience sample of 573 senior scholars in the strategic management field. The goal of our survey was to identify and evaluate some of the primary contributions of strategic management, as well as to help surface perceptions regarding the most important challenges and opportunities for scholars today. 142 responses were received, providing quantitative evaluations as well as qualitative responses to more open-ended questions. To obtain information on potential trends and opportunities, we also augmented this survey data with an analysis of the keywords and full texts of papers appearing in the *Strategic Management Journal* over the thirty-five year period from 1980-2015.

The first question in our survey asked respondents to allocate 100 points across several possible areas of contribution by the field: providing insights to general managers or other leaders of organizations, developing theory that illuminates and informs competitive behavior in strategic settings, providing an inter-disciplinary approach to strategic decision-making, using good econometric techniques, showing how a realistic model of decisions (that is more nuanced than the assumptions provided by other social science disciplines) affects strategic decision-making, and through other means. Results appear in Figure 1.

Survey respondents exhibit a broad consensus that much of the field's contribution lies in providing rigorous insights to general managers and other leaders of organizations. The interdisciplinary approach of strategy, while certainly a feature of the research, received less emphasis, as did other characteristics of the field's research, including rigorous econometrics or more realistic models of decision-making. In written responses, one respondent summarized the field's contribution simply as "helping organizations thrive in uncertain times." Another respondent stated that the strategic management field contributes "by explaining in a rigorous

way how and why firms behave as they do and the competitive consequences of their actions and behavior.” Another scholar was more expansive, considering the broader societal impact of strategic management research: “the field's work helps organizations to create and sustain value in the services and products they offer and through such help improve the welfare of society as well as help preserve and increase the assets that enable organizations to survive and grow.”

*\*\*\*Insert Figure 1 about here\*\*\**

The second question of the survey asked respondents to indicate their perceptions of the importance of several challenges, using a five-point scale ranging from “not important” to “very important.” The potential challenges we prompted respondents with were the following:

1. The field becomes more topically diverse/fragmented (and it becomes harder to converse with others in interest group specialties).
2. The field connects less with the canonical problems that have historically defined its domain (e.g., competitive heterogeneity, organizational scope, managerial process, etc.), emphasizing instead new topical domains.
3. The field loses its distinctiveness versus disciplinary traditions (e.g., strategy vis-à-vis economics, psychology, sociology, etc.).
4. The field loses its distinctiveness versus other applied fields in business schools (e.g., strategy vis-à-vis finance, marketing, organizational behavior, etc.).
5. The field emphasizes theory development and becomes less focused on informing management practice or achieving statistical identification in large-scale, empirical studies
6. The field emphasizes statistical identification in large-scale, statistical studies and becomes less focused on theory development or informing management practice.
7. The field emphasizes management practice and becomes less focused on theory development or statistical identification in large-scale, statistical studies.
8. Other

Of these potential challenges, two were rated as important (4) or very important (5) by over 50 percent of respondents: fragmentation (#1; 53%), and an emphasis on statistical identification at the expense of theory development or informing management practice (#6; 54%). Following closely behind was #5, an emphasis on theory development at the expense of informing management practice or achieving statistical identification (46%). The results also suggest that

some of these potential challenges are less important, in particular an overemphasis on management practice (#7, 19%). At the same time, some concerns were also raised about the potential loss of connection with canonical problems (#2; 37%), as well as the potential loss in distinctiveness vis-à-vis disciplinary traditions (#3; 35%) and other applied fields in business schools (#4; 31%).

The potential fragmentation of the field echoes sentiments expressed by others in conference sessions, special issues, and papers over recent years. Overall, it appears that some scholars believe that researchers in the field are losing contact with the field's historical core and with each other. One respondent to our survey put it this way: "there is not enough interdisciplinary dialogue, which occurs at the reviewer level." Another respondent concluded, "the leaders in the field are aging and retiring, and newer scholars appear to be unable to place their work and contributions in an historical perspective." While outside the scope of our informal survey, it would be interesting to compare these results and perceptions held by senior faculty with the reactions of mid-career and junior faculty to these potential challenges going forward.

At the same time, it is also evident that there are divergent views on the potential implications of focusing on theory development or statistical identification at the expense of the field's historical emphasis on informing management practice. Comments by individual respondents provide some texture to the diverse sensibilities in the field today. One commented that the "field needs to develop a taxonomy of paradigmatic theories, not a potpourri of contrived theories." Another reflected, "a principal challenge is overcoming the tyranny of theory, which gives preeminence to the production of boxes and arrows models of management decision-making."

Others expressed reservations with some of the broader implications of the current identification revolution. One worried that “economists gain excessive influence and the field becomes obsessed with endogeneity,” and another expressed concern that the field “privileges rigor over interesting work.” Given the research efforts currently undertaken that rely on certain widely available datasets (e.g., SDC information on external corporate development activities, data on patents and individual mobility, etc.), one respondent opined that there is “too much focus on huge existing databases and THAT resource defines research problems.”

In sum, we see these diverse responses as partly the natural outcome of strategic management prizing at once relevance and rigor, with the two being in dynamic tension, whether in individual studies, literature streams, or the field more broadly. These different perspectives also likely reflect the unique and diverse interests and skill sets of scholars in the field.

It might also be useful to view these opinions in light of the aspiration of “Pasteur’s quadrant,” which captures research that simultaneously reflects basic research and a quest for fundamental understanding as well as an appreciation for the use of theory (Stokes, 1997). Early generations of strategic management research prized very applied research closely connected with management practice, and more recent research has focused on theory development. At the field level, we believe that there are multiple pathways to achieving theoretically rigorous and managerially relevant research, even if individual studies focus on one or the other. This suggests that there are multiple ways for individual scholars to advance the field. However, a potential risk is that the field’s research more and more becomes not only “lost in translation” to managers, but also “lost before translation” (Shapiro, Kirkman, and Courtney, 2007) – a type III error of failing to address primary problems in favor of problems that are of secondary or minor relevance to management practice (Drnevich, Mahoney, and Schendel, 2020). Overall, there are

views expressed in our survey results that the strategic management field could develop still more relevant and rigorous insights to managers<sup>6</sup>. One respondent suggested, “The field is not where managers look to provide insightful, actionable ideas” and another noted, “The findings are largely ignored in legal cases whereas economics and finance are not.” In one challenging response, one individual averred, “We no longer tackle ‘big strategy’ questions in journal articles.”

To get a sense of how strategic management scholars define their own work, we also obtained all of the keywords from EBSCO for articles appearing in the *Strategic Management Journal* (1980-2015). EBSCO lists 1733 distinct “author supplied keywords” and 1712 distinct “subject terms.” We note that 1118 (65%) of the “author supplied keywords” and 698 (41%) of the “subject terms” appear only once in the dataset. There are numerous explanations for the relatively high frequency of distinct terms, and some are more innocuous than others. For instance, the frequency of distinct terms likely reflects the breadth of contexts in which strategic management theory has been tested, the changing topics appearing in the journal over the years, and even authors’ own efforts to differentiate their work from previously-published articles by developing new concepts and terms. However, these small numbers also likely speak to the topical and theoretical diversity of the field and accord with some scholars’ concerns about the implications of fragmentation in the future.

Finally, we also conducted a topic modeling exercise to determine whether and how the key subjects of the field’s research are changing over time.<sup>7</sup> Figure 2 presents the phenomena appearing in the *Strategic Management Journal*, where topics near the origin are those that are

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<sup>6</sup> This point is echoed in the 2016 Five Year BPS Division Review Report conducted for the Academy of Management. This report indicates that the greatest concern reported by members was the need to increase relevance to managerial practice and public policy.

<sup>7</sup> We thank Normand Peladeau and Amanda Robinson of Provalis Research for their assistance.

shared or common over time, and topics further out are unique to certain periods of time. The proximity of a line drawn from the origin to a topic indicates the time period with which it is most closely associated. For instance, strategic planning systems were the focus of research in the 1980s but appear to be less core over the entire history of the journal that also covers more recent periods. More recently, topics such as stakeholders, founder CEOs, CEO succession, and boards of directors have enjoyed greater currency in the journal. Similar plots might be done in the future for theories, methods, measurement, contexts of research, and the like to obtain an independent assessment of the strategic management literature in order to understand changes in the field at large or in particular areas such as competitive strategy, corporate strategy, international strategy, and so on. Such modeling exercises could be used, for instance, to evaluate topical trends or drifts in streams of research, to appraise the theoretical plurality of interest areas, or to determine whether and where certain methodological approaches are taking root in the literature.

*\*\*\*Insert Figure 2 about here\*\*\**

### **Addressing Fragmentation in Strategic Management Research?**

A field as topically and theoretically diverse as strategic management does not have, and will likely never have, a single or well-developed paradigm, in comparison with other business school disciplines. Indeed, the multidisciplinary perspectives called upon by individual strategy studies as well as the field as a whole can be seen as a strength and virtue, not to mention evidence of the field's development and success in addressing complex and changing management problems. The theories used in strategic management span a wide range of disciplinary traditions, and the topical diversity of strategic management research is also substantial, making it hard to imagine otherwise. That said, one might also ask questions as to

how individual studies relate to the field's core; what the distinctive contributions, boundaries, and even identity of the field are; and how scholars will continue to have the same kinds of meaningful exchanges as the field grows and becomes increasingly specialized within certain sub-areas of research. Each of the field's veins of research are becoming deeper and deeper and often draw upon different starting assumptions about organizations as well as their leaders and decision-making.

We are hardly the first to make such observations or to consider potential ways to respond to the growth and diversity of strategic management research. For instance, the Strategy Research Initiative (SRI) provides a valuable reader that provides a listing of seminal articles and commentaries to provide doctoral students and others new to the field with an appreciation of the core challenges tackled by strategic management research (i.e., <http://strategyresearch.net>). The SRI has also developed a set of criteria for high-quality strategy research, offering sub-criteria for (1) all research, (2) all theoretical research (e.g., theoretical claims are clear, rigorous, measurable, and plausible), (3) all empirical research, and (4) theory testing research (e.g., use of reliable data conforming to theoretical constructs, alignment in the unit of analysis, consideration of alternative interpretations for valid proxies, accounting for endogeneity). As an illustration, to promote progress and greater coherence in all strategy research, they recommend that individual studies appreciate and build upon prior high-quality research (Oxley *et al.*, 2010). These suggestions are highly relevant and important for strategy research, and research in management more generally as well as in the social sciences. Most of these recommendations are concerned with the field's potential vertical differentiation rather than horizontal differentiation, as these recommendations address the quality of strategy research rather than the topics, theories, and approaches that potentially distinguish strategic management from other fields *per se*.



Durand, Grant, and Madsen (2017) begin with the provocative question, “is everything ‘strategy’?” and note that fragmentation might warrant greater consolidation, integration, and redirection to address some of the challenges we have mentioned in the foregoing sections. They suggest that fragmentation exists in multiple dimensions in strategy research – in theory, in the expansion of phenomena and analytical issues, and in the development of specialized sub-fields in the research community. They suggest that literature reviews of strategic management research can play an important role in integrating the field in five different ways:

1. Identifying and promoting theoretical developments and empirical analyses that subsume and integrate multiple theoretical streams,
2. Reconciling different theories and results that address the same phenomena but with different, sometimes conflicting, predictions and outcomes,
3. Applying a single theoretical approach to a range of separate but related phenomena,
4. Extending and combining existing concepts, theories, and empirical results of strategic management research to analyze novel phenomena, and
5. Building more precise, systematic theory comprising clearly defined concepts, explicit assumptions, and logically derived causal relationships to support cohesive empirical progress.

Each of these opportunities for integration respond to specific sources of fragmentation in the field – a lack of cumulative or accretive theoretical development and empirical analyses, multiple theoretical streams and empirical findings addressing the same phenomena, narrow-range theories addressing particular contexts, new concepts needed for new business problems, and ambiguity and imprecision concerning theoretical constructs and their relationships, respectively. We believe that there are valuable opportunities to build upon their special issue and to encourage literature reviews for these purposes.

Leiblein, Reuer, and Zenger (2018) suggest that a specific source of fragmentation in the field is potential confusion or limited appreciation of what makes decisions strategic. Often scholars resort to the perceived importance of a decision for an organization, but this approach defies *ex ante* reasoning, and other fields justifiably claim to be studying “important” decisions.

They identify three dimensions of interdependence that together make decisions more strategic – interdependence across contemporaneous choices and activities, interdependence with decisions of other economic actors (e.g., competitors, suppliers, etc.), and interdependence across time. These characteristics of strategic decisions give rise to a number of important tensions in strategic decisions. For instance, when a decision is costly to reverse and subject to a high level of uncertainty, it can pay to be flexible and defer a decision (i.e., interdependence across time), yet the risk of preemption by a competitor can prompt an early commitment instead (i.e., interdependence across other economic actors). Strategic management concerns a number of paradoxes that these unique characteristics of strategic decisions highlight, including not only whether to invest in a commitment-intensive versus flexible manner (Trigeorgis and Reuer, 2017; Posen *et al.*, 2018), but also whether to compete or collaborate (Hoffmann *et al.*, 2018), whether to make or buy (e.g., Williamson, 1985), explore or exploit (e.g., March, 1991), or organize for innovation in a closed or open manner (e.g., Felin and Zenger, 2014), among others.

We would note that such decision tensions or paradoxes underlie each of the fundamental issues in strategy and have important consequences for how strategic problems are framed and theorized upon. At a broad level, the strategic management field has avoided universalistic thinking and general “rules to riches” in favor of alignment and equifinality as general principles for assessing competitive advantage or guiding organizational scope decisions. For example, there is value to committing and value to being flexible in different decision-making contexts, so the correct answer to the right course of action is “it depends” on the situation. Should the firm outsource some R&D activity or perform it in-house? Again, it depends, this time on the characteristics of a transaction or economic exchange. Thus, strategic management research forces comparative analysis and thinking on alternative decisions that make up these paradoxes,

and so strategy research often invokes the principle of alignment in one way or another.

Attributes of economic exchanges need to be matched with attributes of alternative organizational forms (Williamson, 1991). As a second example, organizational activities need to be consistent and supportive of a business' overall competitive positioning, whether emphasis is given to developing a cost advantage or offering sources of differentiation (Porter, 1985).

The fact that such decision tensions underlie the canonical problems of strategic management and in turn reflect the fundamental characteristics of strategic decisions provides new ways to promote integration of strategic management research (see Figure 3). As we have noted, a topical focus on the fundamental issues raises questions as to what is more or less central to strategic management, yet the criteria for defining the boundaries of scholarly inquiry in strategic management are left open. Of course, it is also the case that decision tensions exist in all business school disciplines. However, the three facets of strategic decisions are concrete and are well-grounded in the theory being used in the field today, so there is promise in using these criteria to help scholars connect their work with the core of the field. For instance, when one approaches contemporary or frontier topics -- whether they might be corporate social responsibility, stakeholders, artificial intelligence, digital platforms, or others -- questions such as the following are prioritized: (1) What decisions do the phenomenon or management challenge present, and how are they closely intertwined with others made contemporaneously by the organization? (2) How might these decisions meaningfully influence, and be influenced by, the decisions of other economic actors, whether competitors, suppliers, complementors, etc.? (3) What intertemporal considerations are implicated (e.g., how do they force future decisions, and what uncertainties are present in making commitments or staging investments)? The scholar can therefore use the three types of interdependence to make the case that a decision is strategic and

can usefully build on the field's historical core, regardless of the method used in approaching new phenomena or the disciplinary tradition employed (e.g., economics, sociology, political science, psychology, etc.). Leiblein, Reuer, and Zenger (2018) suggest that a focus on these dimensions of strategic decisions is also valuable because they can inform the theory of competitive advantage as well as the theory of the firm, so there is a basis for theoretical integration, particularly among theories with a shared emphasis on uncertainty as a core determinant of firms' resource allocation, competition, and governance decisions.

\*\*\**Insert Figure 3 about here*\*\*\*

There are likely to be additional ways to offer the integration potential and pathways for doing so along the lines that Oxley *et al.* (2010), Durand *et al.* (2017), and Leiblein *et al.* (2018) highlight. For instance, given the sheer depth of work occurring within the various interest areas within strategic management, there is value in synthesizing literatures and using integrative reviews as a means of developing understanding of, and building bridges across, increasingly specialized streams of research. Essays that go beyond reviews *per se* also offer integration potential for strategic management research in the future to spur on research that addresses foundational issues.

### **Introducing the *Strategic Management Review* (SMR)**

The idea to launch a new strategy journal was informed partly by the challenges and opportunities noted in the foregoing sections, but it also was based on three main starting premises and beliefs. First, not only has the field grown enormously in a few short decades as evidenced by the ratio of strategy scholars to top strategy journals (e.g., *Strategic Management Journal*, *Strategy Science*, etc.) and their capacity for handling scholarly output, but there is a perception that the field has lost consensus on its purpose and identity. The issues addressed by

research in the field are broad and drifting, and we see considerable promise for research on foundational issues of strategic management. Second, the field of strategic management has historically maintained strong interdisciplinary ties, and we aim to build on this legacy. For instance, our core theories related to the resource allocation process, the scope of the firm, and the pursuit of competitive advantage are of continuing, critical importance to managers, just as there are opportunities to help scholars with new and specialized skills connect with the field's core. Finally, we believe that the core issues of strategic management are of increasing importance to contemporary managers, even if business practices and the environment of strategy is changing. Current challenges such as AI, Big Data, or IP challenges in China, among others, require system-level approaches, and the strategic management perspective is critical to address such issues. Examination of these and other changing phenomena also holds the potential to enrich understanding of the canonical problems of strategic management as they provide new proving grounds for the field's ideas. The mission of the journal follows from these basic beliefs:

The *Strategic Management Review (SMR)* publishes ideas that matter for strategic management research. Specifically, the *SMR* features provocative essays and forward-looking reviews to guide the questions tackled by research in the field. The journal also aims to promote integration of strategic management research by encouraging research closely connected with the field's canonical problems as defined by management practice. The *SMR* complements existing strategic management outlets that emphasize empirical research.

The *SMR* aims to promote insights on core questions in the strategic management field through impactful essays. These essays can take many forms, including (a) essays on the theoretical foundations of strategic management and the theoretical perspectives emanating from the field, (b) scholarly exchanges, (c) thought pieces dealing with managerial practice or public policy, (d) methodological primers on advances relevant for strategic management research, (e) state-of-the-art reviews or research retrospectives, and (f) forward-looking literature critiques.

The journal therefore provides a platform for creative essays employing multiple disciplinary perspectives to address topical issues within the strategic management field. Topical coverage is intentionally broad, including the following areas as examples of the field's intellectual footprint:

- Behavioral strategy
- Competitive strategy
- Collaborative strategy
- Corporate strategy
- Entrepreneurship and strategy
- International strategy
- Knowledge, innovation, and technology
- Micro-foundations of strategy
- Stakeholder strategy
- Leadership and governance
- Organization and strategy
- Strategy process and practice
- Strategic decision-making
- Research methodology in strategic management

Empirical articles or other pieces of original research that could be published in traditional strategy and management outlets fall outside of the scope of the *SMR*. An aim of the *SMR* is to complement those outlets by focusing on developing and curating thought-provoking essays.

The *SMR* therefore aims for a unique position, as summarized in Figure 4. Traditional strategy journals have increasingly been pursuing excellence within a normal science paradigm that emphasizes disciplinary precision and empirical rigor. This emphasis has been accompanied by less attention to canonical questions, pre-paradigmatic work, engagement with practice and (possibly) thought-provoking ideas. Because the *SMR* does not publish empirical research and has the flexibility and freedom to relax some of the constraints that naturally accompany good discipline-based empirical research, it can allow for more provocative essays, encourage earlier stage work and speculation, provide room for opinion, and foster engagement with practice and a tighter focus on canonical strategy problems while encouraging creativity. Some of the essays are expected to foster ideas that can be tested and further elaborated upon in other journals. If

such essays serve the purpose of “pre-research” to spur on inquiry, others will fulfill the function of “post-research” to take stock of streams of research in order to encourage cumulative knowledge building in the field.

\*\*\**Insert Figure 4 about here*\*\*\*

We envision that these objectives will be met in part by devoting ourselves to learning from practice. In earlier years, strategy scholars often had substantial work experience, engaged heavily in consulting, and attended conferences that consultants and businesspeople also frequented, but this less so today, particularly among junior scholars (Drnevich, Mahoney, and Schendel, 2020). While the *SMR* is by design an academic journal that it intended to complement other academic strategy outlets, it will foster learning from practice in a few new ways. To begin with, the journal has developed a Business Practice Advisory Board (BPAB) consisting of senior consultants and business practitioners who appreciate and are interested in the enterprise of strategy research. The inaugural BPAB is chaired by Hugh Courtney (Northeastern University) and aims to address the growing disconnect between strategy research and practice and the often insular approach to research in the field. To do so, the BPAB will provide counsel on the content of regular and special issues in order to shape strategy research, and opportunities will also exist for informal data gathering as well as editorials and commentaries in the future. Members of the inaugural BPAB include the following individuals:

- Fernando Chaddad, Managing Director and Head of Analytics Strategy Practice, Latin America, Accenture
- Leslie Donato, VP Strategy, Bayer Pharmaceuticals
- Neil Kalvelage, Senior Managing Director, Centerbridge Partners, L.P.
- Jane Kirkland, SVP and Head of Strategy for Global Services, State Street
- Max Michaels, AI entrepreneur, former Global Head of IBM’s Network Services Business
- Cynthia Pols, VP Strategy, Change Healthcare
- Michael Raynor, Director, Deloitte Services LP

- Martin Reeves, Senior Partner, Managing Director, and Director of the Henderson Institute, BCG
- Rebecca Roth, VP Strategy and Planning, GateHouse Media
- Scott Snyder, Partner, Digital Transformation and Innovation, Heidrick & Struggles
- Tom Stewart, Executive Director, National Center for the Middle Market, former editor of *HBR*
- Scott Wells, CEO, Clear Channel Outdoor Americas

In addition, the *SMR* will sponsor special conferences on contemporary topics, and these events will afford scholars the opportunity to interact with consultants and executives, including members of the BPAB and/or consultants and executives assembled by special issue editors.

*SMR*'s first conference, on Open Innovation at UC Berkeley, featured keynotes by David Teece and Henry Chesbrough, and an *SMR*-sponsored conference in London on M&A Strategy and Practice featured presentations by M&A heads at McKinsey UK, Philips Lighting, SAP, and Unilever. An *SMR* conference on Corporate Renewal at Columbia Business School featured a keynote by James Gorman, CEO of Morgan Stanley, as well as a panel of private equity and turnaround consultants. These events will drive content for essays for some upcoming issues of the *SMR*. Finally, in order to foster engagement and the sharing of ideas on contemporary strategy topics, the *SMR* will also develop and maintain a LinkedIn community of like-minded scholars as well as practitioners interested in strategic management research.

In order to support this positioning, the *SMR* will also have a tailored governance process that will foster the development of thought-provoking essays. A key objective of the review process is to support authors' voice, by which we do not just mean avoiding imposing a particular format or avoiding reviewers taking responsibility for the content or style of an essay. We wish to provide authors room for opinion, creativity and imagination, and illustration that are not possible at other outlets. When submitting final papers for peer review, authors will be asked to nominate a set of 4-5 potential reviewers presenting no conflicts of interest, the editors will



select one reviewer, and the review process will be handled in a single blind manner.

Opportunities to engage with the ideas in essays will also exist within the *SMR* LinkedIn community. The unique features of *SMR* and its governance would not be appropriate for all journals, to be sure, but we believe that the *SMR* can offer unique value as a complement to other strategy outlets in the field.

In developing the *SMR*, we received support from many individuals, and we would like to close by acknowledging their input and advice, subject to the usual disclaimer. Ideas contained in this essay were presented along with a panel of editors considering the future of the field at an SMS Keynote Plenary (Houston 2017), and we would especially like to thank Rich Bettis (UNC), Connie Helfat (Dartmouth), and Dan Schendel (Purdue) for sharing their experiences and insights. We also appreciate the input of our Engagement Editors – Don Hatfield (Virginia Tech) and Sotirios Paroutis (Warwick) – for their ideas on ways to take advantage of our developing digital platform. Many of the characteristics of the *SMR* are also due to a great number of conversations with our editorial board, whose members have also been active in suggesting special issues and essays to develop as we launch the journal. The initial board of the *SMR* follows:

- Juan Alcacer (Harvard University)
- Africa Ariño (IESE Business School)
- Richard A. Bettis (University of North Carolina)
- Aaron Chatterji (Duke University)
- Gary Dushnitsky (London Business School)
- Nicolai Foss (Bocconi University)
- Melissa E. Graebner (University of Texas at Austin)
- Ranjay Gulati (Harvard University)
- Constance E. Helfat (Dartmouth College)
- Glenn Hoetker (University of Melbourne)
- Riitta Katila (Stanford University)
- Michael J. Lenox (University of Virginia)
- JT Li (HKUST)

- Jeffrey T. Macher (Georgetown University)
- Tammy J. Madsen (Santa Clara University)
- Joseph T. Mahoney (University of Illinois)
- Xavier Martin (Tilburg University)
- Kyle J. Mayer (University of Southern California)
- Brian T. McCann (Vanderbilt University)
- Rita G. McGrath (Columbia University)
- Jackson A. Nickerson (Washington University St. Louis)
- Subramanian Rangan (INSEAD)
- Melissa A. Schilling (New York University)
- J. Myles Shaver (University of Minnesota)
- Harbir Singh (University of Pennsylvania)
- Kulwant Singh (National University of Singapore)
- Margarethe F. Wiersema (University of California – Irvine)
- Edward J. Zajac (Northwestern University)

In closing, we would like to encourage engagement by the strategic management community as this new journal launches. We very much welcome your ideas for special issues and essays, and we look forward to your involvement in developing and publishing ideas that matter for strategic management research.

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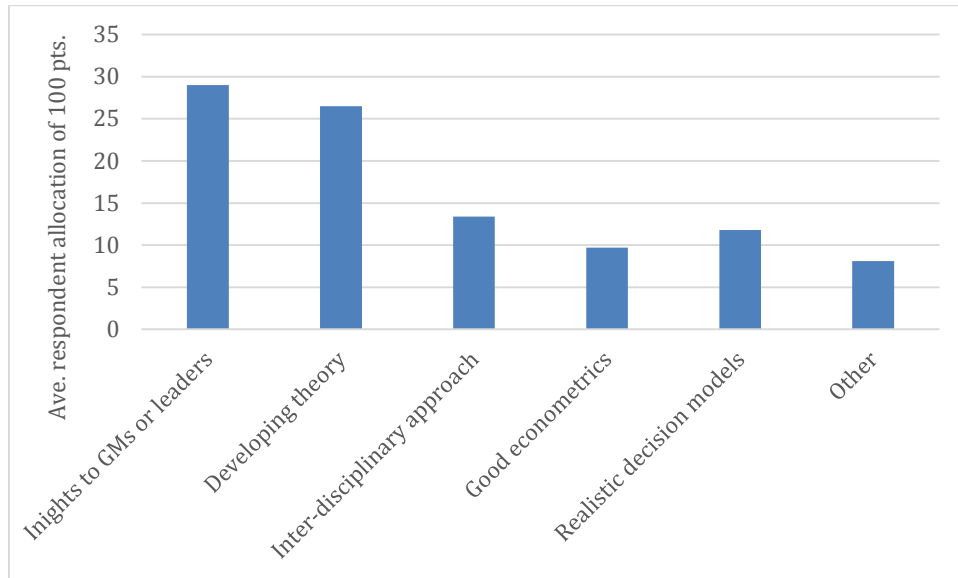
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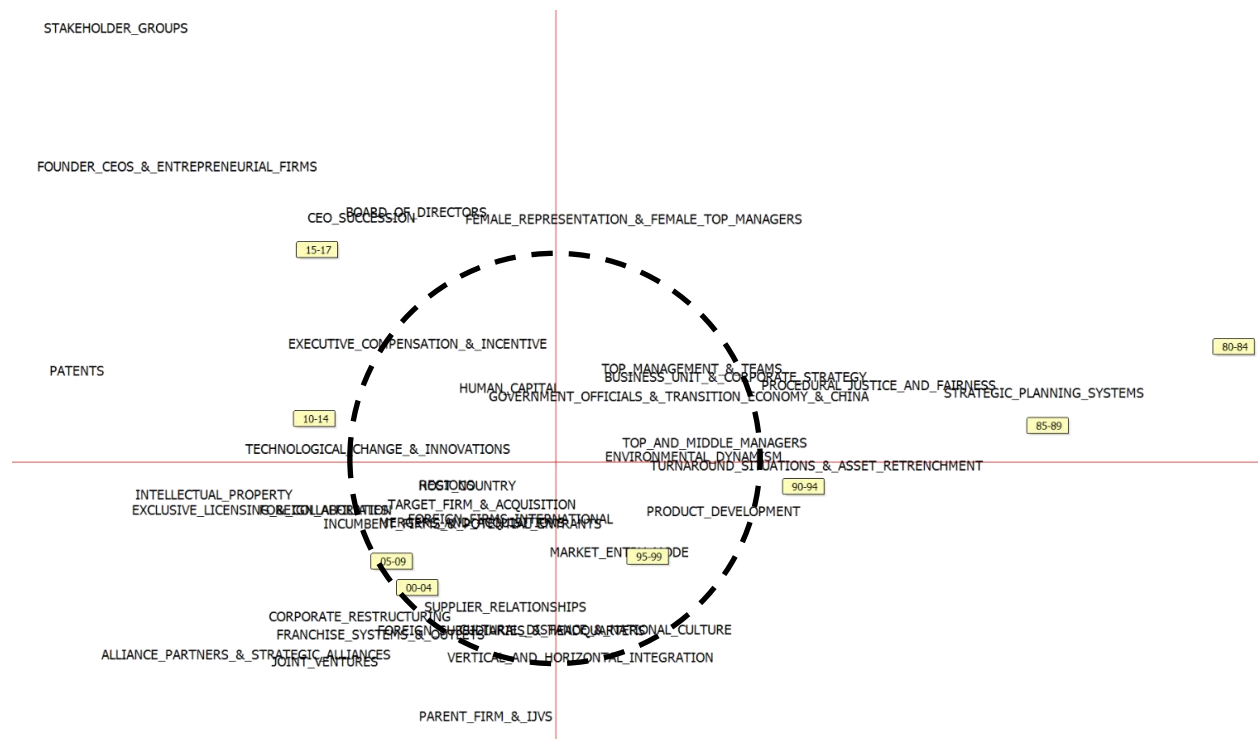
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**Figure 1**  
**Perceived Distinctive Contributions of Strategic Management**

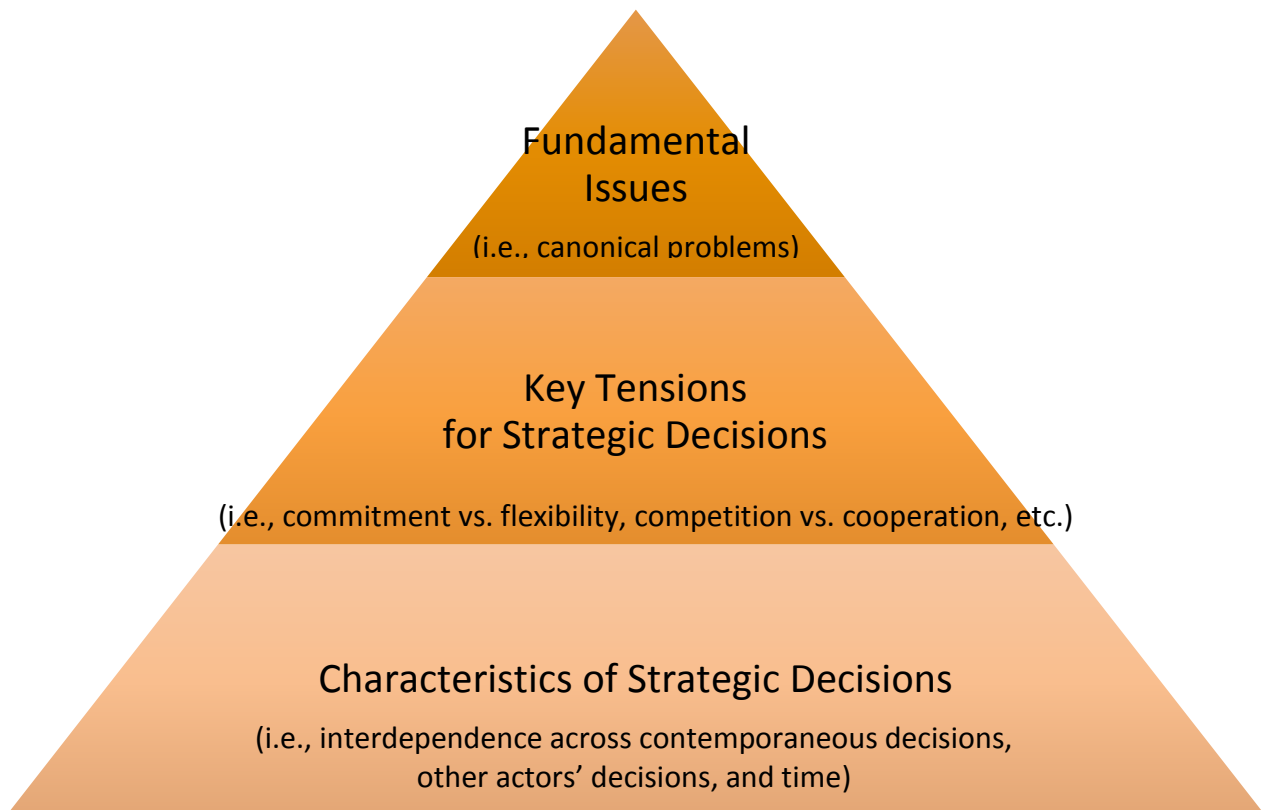




**Figure 2**  
**Topic Modeling Evidence for Phenomena**



**Figure 3**  
**Strategic Decisions as Building Blocks**



**Figure 4**  
Positioning of *Strategic Management Review*

