*Strategic Management Review, 2020, 1: 35–73*

# Has Strategic Management Research Lost Its Way?

Paul L. Drnevich[1], Joseph T. Mahoney[2] and Dan Schendel[3]*

[1] *Culverhouse College of Business, University of Alabama, Tuscaloosa, AL, USA; dren@ua.edu*
[2] *Gies College of Business, University of Illinois at Urbana-Champaign, Champaign, IL, USA; josephm@illinois.edu*
[3] *Krannert School of Management, Purdue University, West Lafayette, IN, USA; schendel@purdue.edu*

ABSTRACT

Strategic management research has strayed from its primary focus on efficient and effective management practice. We suggest a refocusing of strategic management research, based on the logic of discovery of real-world phenomena and strategic problems, with the goals of better enabling scholars to refine existing and develop new management theories. We demonstrate that historically, such a pragmatic, engaged, and problem-focused process helped produce many of the seminal works, groundbreaking concepts, and core theories in the strategic management field. We express our concern that many scholars are no longer developing or practicing sufficient skills to generate such impactful research. We propose that re-adopting this logic of discovery within the knowledge production process of engaged scholarship can produce both scientifically rigorous and practically relevant research. Such a research approach both develops new theoretical contributions and bridges the theory-practice gap by addressing real-world problems (and thereby creating economic value). We discuss some of the existing impediments to doing so and suggest changes in incentive systems

*We thank Guatam Ahuja, Richard Bettis, Seth Carnahan, Lillian Cheng, Felipe Csaszer, Don Hambrick, Bruce Kogut, Michael Leiblein, James Mahoney, Cathy Maritan, Will Mitchell, Arvind Parkhe, Olga Petricevic, Jeff Reuer, David Teece, Alain Verbeke, and Sidney Winter for their thoughtful comments on earlier drafts of the paper. We also thank those who provided comments at our presentation at the 2016 annual Academy of Management meeting, and at subsequent seminars at Temple University, the University of Calgary, and the University of Michigan. Finally, we thank *Strategic Management Review* for the editors' and reviewer's helpful comments and suggestions for improving this manuscript through the review process. The usual disclaimer applies.

ISSN 2688-2612; DOI 10.1561/111.00000004

to reward scholars for producing impactful research that better serves the collective welfare of the school, the field, and society.

*Keywords:* Collective action problem; logic of discovery; problem-formulating and problem-solving approach; theory-practice gap

## Introduction

The study of management, including strategic management, is ultimately about the efficient and effective practice of management in organizations, as well as the organizational challenges arising from an ever-changing environment. The concern raised here is that strategic management research has strayed from this focal purpose. Why is this so? While there are numerous structural and incentive reasons highlighted below, we maintain that the availability of data and/or fashionableness/novelty of the methodology employed often drive current management research. While such a research approach is understandable for individual scholars with institutional pressures to publish, the unintended consequence collectively for the field is an ever-narrowing set of research questions posed (Alvesson and Sandberg, 2013). Such a *collective action problem* (Gulati, 2007; Olson, 1965) touches at the heart of the management profession in academia. It is unsatisfactory, if not unsettling, that much of our current research fails to receive much attention from either business executives (Pfeffer and Fong, 2002) and policy makers, or even other academics (McGrath, 2007). Such a lack of practical relevance of management research threatens our field's very legitimacy (Hitt and Greer, 2012).

The academic field of management is one of application, as a source of thinking and experience that creates value for society by making organizations work better. This role is simple, perhaps even mundane, but very important. Society has an incredible array of organizations, almost all of which are certain to fail eventually due to some unmet challenge. Within this field, the domain of strategic management finds its value neither in knowledge for its own sake, nor as a fundamental academic discipline, which meets the lofty standards of uncovering basic truths in understanding static and universal phenomena. Indeed, it would be unwise for the management field to seek a reconstructed logic of how core academic disciplines such as mathematics, chemistry, and physics ostensibly progress (Kaplan, 1964). Quite conversely, the domain of strategic management deals with a dynamic array of ever-changing variables and vexing uncertainties, which create unceasing challenges and an ever-changing set of perplexing problems to solve. While there may be some duration to the problems and the challenges they pose to organizations, there is nothing universal or necessarily long continuing about their solutions.[1] Like medicine, it is not the body (read organizations) that changes so much as the phenomena (read diseases) encountered. That is why we practice medicine for organizations, and why the ultimate test of the relevance and value of our work resides in the practice of management.

Unfortunately, what has resulted from current research processes is a (re-)doubling of research on problems that are at best of secondary importance and are at worst not "grounded in reality" (Van de Ven, 2007, p. 13). To facilitate much needed change, we submit that management research should be grounded in a *logic of discovery*, with a primary focus that begins and ends with real-world phenomena and addresses strategic problems of managerial practice. The beginning means understanding extant practice and its problems, and the end means employing theoretical reasoning that is capable of increasing the efficiency and effectiveness of executable routines for solving substantive managerial problems (Simon, 1982). If not executable, then to repeat a view attributed to the mathematician Poincaré: "such solutions are very little solved" (Simon, 1957, p. 203). Management research, thus, must focus either on the patient's illness, as in medical practice (Rousseau, 2006), or on problem solving, as in engineering (Nickerson and Zenger, 2004), and its prescriptions must be as applicable in helping, as the problems are in hindering, increased efficiency and effectiveness.[2]

To address these concerns, we suggest a refocusing of strategic management research that joins a problem-formulating and problem-solving approach (Felin and Zenger, 2016; Nickerson and Zenger, 2004) with engaged scholarship (Van de Ven, 2007) and a philosophy of pragmatism (Dewey, 1938), which holds that the validity of an argument depends upon the consequences of acting upon it (Rorty, 1979). Historically, such a problem-focused, engaged, and

[1]The economist, Sir John Hicks noted that, "Since it is a changing world that we are studying, a theory which illuminates the right things now may illuminate the wrong things another time. This may happen because of changes in the world (the things neglected may have grown relative to the things considered), or because of changes in the sources of information (the sort of facts that are readily accessible to us may have changed) or because of changes in ourselves (the things in which we are most interested may have changed). There is, there can be, no economic theory which will do for us everything we want all of the time" (1976, p. 208). We concur and add that the same reasoning holds for the strategic management field.

[2]Simon (1982, p. 475) states: "It is a vulgar fallacy to suppose that scientific inquiry cannot be fundamental if it threatens to become useful, or if it arises in response to problems posed by the everyday world. The real world, in fact, is perhaps the most fertile of all sources of good research questions; calling for basic scientific inquiry." For the purpose of achieving rigor and relevance, the evidence we seek moving forward is that more published works in the strategic management field exhibit engaged scholarship by identifying various stakeholders (e.g., users, clients, sponsors, and practitioners) informing their research questions to establish that the problems identified are grounded in reality (Van de Ven, 2007).

pragmatic scholarly discovery process facilitated many of the seminal works and core theories in the strategic management field.[3] We submit that scholars (re-)adopting this logic of discovery within the knowledge production process of engaged scholarship can produce scientifically rigorous and practically relevant research that both develops new theoretical contributions and bridges the theory-practice gap by addressing real-world problems, and thereby creating economic value.[4]

The structure of this monograph proceeds as follows: First, we develop and support our view that a substantial research–practice gap has arisen, which presents a problem for the relevance and impact of the field. Further, because many scholars are no longer developing sufficient skills to conduct impactful research, we are unlikely to close this gap by pursuing the field's current activities and directions. We suggest these issues represent a collective action problem, which, if resolved, would provide substantial benefits to the field. Second, we review the scale and scope of the problem and the impediments to its resolution. Third, we provide a discussion of how we may address these impediments and suggest changes needed to incentivize scholars to pursue research that is more impactful for the collective welfare of the school, the field, and society (Gulati, 2007; Khurana, 2007). We conclude by offering suggestions for future research that may yield such value.

[3]The strategic management field needs to return to its earlier foundations of problem formulation as a key activity in strategic decision-making (e.g., Ackoff, 1970; Andrews, 1971; Cooper and Schendel, 1976; Delbecq and Van de Ven, 1971; Hax and Majluf, 1984; Hofer and Schendel, 1978; Koontz and O'Donnell, 1964; Learned *et al.*, 1965; Lyles, 1981; Lyles and Mitroff, 1980; Mintzberg *et al.*, 1976; Newman and Logan, 1971; Pettigrew, 1973; Quinn, 1980; Rumelt *et al.*, 1994; Schendel and Hofer, 1979; Shrivastava and Grant, 1985; Simon, 1973; Smith, 1951; Volkema, 1986).

[4]To bridge the theory-practice gap, engaged scholarship approaches include: (1) informed basic research; (2) collaborative research; (3) design science; and (4) action research (Van de Ven, 2007). *Informed basic research* resembles a traditional form of basic social science research, but also solicits key stakeholder advice (e.g., users, clients, sponsors, and practitioners) (Van de Ven, 2007; Van de Ven and Johnson, 2006). *Collaborative research* requires academic researchers to co-produce knowledge with stakeholders (Burgelman and Groves, 2007; Louis and Bartunek, 1992). *Design science* concerns how things should be done for solving practical problems (Romme, 2003; Simon, 1996; Van Aken, 2005) through evidence-based knowledge (Rousseau, 2006; Tranfield *et al.*, 2003). *Action research* takes a clinical intervention approach to diagnose and treat a problem of a specific client (Argyris and Schön, 1978; Lewin, 1951; Schön, 1983). Concerning these four approaches, Van de Ven (2007, p. 28) states: "which is most appropriate depends on the research question and the perspective taken to examine the question. Pragmatically, the effectiveness of a research approach should be judged in terms of how well it addresses the research question for which it was intended (Dewey, 1938)." Bridging the theory-practice gap is typically beyond the skills and scope of any one individual, which warrants the creation of intermediary organizations, such as the Network for Business Sustainability (Bansal *et al.*, 2012). Further, Schwab and Starbuck maintain that small and egalitarian groups, which they call "collegial nests," can become powerful generators of innovative ideas that are both rigorous and relevant (2016, p. 167). The current paper emphasizes *informed basic research* (Van de Ven, 2007).

### The Theory–Practice Gap Problem and Benefits of its Closure

The current rigor versus relevance debate and the theory–practice gap problem, we posit, would be more effectively framed and addressed through understanding tensions and dualities to be recognized and overcome (Bartunek, 2003; McGahan, 2007; Vermeulen, 2005). Therefore, in this section, we focus on the logic of discovery for identifying problems facing managers for the purpose of refining and developing *theory*[5] that attains both scientific rigor and practical relevance (George, 2014; Mahoney and McGahan, 2007; Stokes, 1997).[6] We submit that the field's current deficit and challenges in refining and applying existing theory to address practical problems, as well as its limited development of new theory in recent years (Bennis and O'Toole, 2005), are likely due to a disconnection from the logic of discovery (Hanson, 1958). We further maintain that joining a problem-formulating/problem-solving approach (Lawrence, 1992; Nickerson *et al.*, 2012), engaged scholarship (Van de Ven, 2007), and a philosophy of pragmatism (Dewey, 1938) facilitates the logic of discovery. We submit that renewing scholarly efforts to adopt such a problem-focused, engaged and pragmatic scholarly discovery logic can produce scientifically rigorous and practically relevant research that both develops new theoretical contributions and bridges the theory–practice gap (Aram and Salipante, 2003; Bartunek and Rynes, 2014; Mintzberg, 1994; Rynes *et al.*, 2001).

A problem-formulating and problem-solving approach to strategic management (Nickerson *et al.*, 2012) embraces the philosophy of pragmatism. The word "pragmatism" comes from a root that means act or deed. The focus of pragmatism is to serve the purposes of various stakeholders effectively, and to mediate between the push of our past and the pull of our insistent future. Essentially, it focuses primarily on the process (beyond the content and context). Data and solutions are concrete and contextual, and the pragmatic

[5]We follow Kerlinger in defining theory as "a set of inter-related constructs (concepts), definitions, and propositions that presents a systematic view of phenomena by specifying relations among variables, with the purpose of explaining and predicting phenomena" (1964, p. 11). In this usage, theoretical *propositions* provide statements of relationships between concepts and constructs (i.e., between abstract theoretical terms), while *hypotheses* are defined as relationships between variables (i.e., among concrete observable terms) (Bacharach, 1989; Van de Ven, 2007). The current paper considers the logic of discovery of theoretical propositions and their corresponding testable hypotheses (Godfrey and Hill, 1995; Lam, 2010; Miller and Tsang, 2011).

[6]Augier and March (2011) provide an exemplar in Holt *et al.* (1960) on "a set of problems in inventory management. The inventory control project sought to develop theory and contribute to science, but it was fundamentally problem driven. . . . The work was conducted around the Pittsburgh Plate Glass Company. The purpose was to develop analytically based tools that would improve the performance of the factory. In the course of solving the practical problem involved, the book made important technical contributions to the theory and practice of dynamic programming" (2011, p. 138).

method is an historical, evolutionary, and functional approach (Bourgeois, 1984; Kaplan, 1961). Pragmatism is above all concerned with the usefulness of an idea and the consequences of acting upon it (Dewey, 1938; Rorty, 1979).

For asking fundamental questions, the strategic management field need not pursue only those problems externally imposed by other disciplines that it has found useful in application, but rather it should pursue problems intrinsic to its own aspirations, based on the principle of the *autonomy of inquiry* (Dewey, 1938; Kaplan, 1964).[7] Such autonomy of inquiry should apply not only to questions raised, but also to methodologies deployed. Embracing theoretical and methodological pluralism (Bowman, 1990; Mahoney, 1993; Mahoney, 2005; Pettigrew, 2001) in the strategic management field, has advantages for staying connected to various disciplines (e.g., economics, finance, psychology, sociology), and an interdisciplinary approach is needed for addressing problems of obtaining strategic coherence in a global and complex interconnected world (Ghemawat, 2007; Porter, 1996).

The requirements of these external disciplines typically focus on the logic of justification (e.g., research design and rigorous methodologies), which is vitally important. However, because bridging the theory–practice gap is an intrinsic aspiration of the strategic management field, we focus more on *social science as process* rather than *social science as product*, and we thus emphasize the *logic of discovery* (Hanson, 1958) rather than the *logic of justification* (Reichenbach, 1938).[8] The logic of discovery is an abductive process, which considers the reasons for entertaining hypotheses (Hanson, 1958; Peirce, 1940), in contrast to the logic of justification, which deals with the reasons for accepting a hypothesis through the rational procedure of *conjectures and refutations* (Kaplan, 1964; Popper, 1963).[9] Failing to apply the logic of discovery and the identification of

---

[7]Kaplan states: "The conflict between freedom and control is an existential dilemma for science, whatever it may be for society at large. Yet for science, at any rate, it seems to me that reason requires that we push always for freedom, freedom even for the thought which we enlightened ones so clearly see to be mistaken" (1964: 377). To achieve effective communicative action requires ethical rules of conversation (Habermas, 1973; McCloskey, 1985).

[8]Bettis (1991) suggests connections among pragmatism, hypothesis generation, and the logic of discovery, and referenced Dewey's (1938, p. 508) forceful statement: "[I]n the social disciplines, law, politics, economics and morals . . . . Failure to encourage fertility and flexibility in formation of hypotheses as frames of reference is closer to a death warrant of a science than any other one thing."

[9]Peirce (1940) maintained that induction and deduction are each incomplete and pioneered the concept of *abduction* or hypothetical inference. Abduction is a form of inference comprising the generation and selection of a (tentative) hypothesis as to the cause of something observed by creative insight in the interpretation of data and a coherent resolution of an anomaly. Hanson's (1958) *patterns of discovery* further developed the concept of abduction. Van de Ven notes: ". . . Hanson [1958] held that a major defect of logic positivism was that it confines attention only to the finished product of scientific reasoning and gives no attention to the process of reasoning whereby laws, hypotheses, and theories receive their

practically relevant problems can lead to addressing minor or irrelevant issues, rather than a primary problem, or what mathematician John Tukey referred to as a Type III error (Mitroff and Featheringham, 1974). Tukey states: "Far better an approximate answer to the right question ... than an exact answer to the wrong question, which can always be made precise" (1962, pp. 13–14). With Type III error, the relevance to management practice is lost *before* translation of the theory (Pettigrew, 2001; Shapiro *et al.*, 2007). Therefore, it is critical to consider the logic of discovery, which is concerned with generating relevant questions and with the sources and generation of useful hypotheses in the knowledge-creation process (Kieser *et al.*, 2015; Nonaka and Takeuchi, 1995).

To attenuate the risk of Type III error, discovering and formulating the primary relevant managerial problem requires accurate and effective problem formulation and hypothesis generation, which are creative processes. Further, in terms of the questions we ask, we need to possess greater sensitivity to the possibility that what we fail to find in the research literature may be more important than what we find (Ladd, 1987; Weick, 1989). Effective problem formulation is often the first and most important task in the research process (Volkema, 1983; Whetten, 1989), and needs to be clearly defined and corroborated by practitioners (Markides, 2007; Van de Ven, 2007).[10] Inquiry from inside the organization(s) is critical for the logic of discovery and effective problem formulation, and requires that the strategic management field have a balance between "inquiry from the outside," and "inquiry from the inside" (Bartunek and Louis, 1996; Evered and Louis, 1981). Generalized theories of researchers and contextual theories of managers can interactively evolve in a new model of double-loop learning (Argyris and Schön, 1978; Balogun *et al.*, 2003; Mahoney and Sanchez, 2004). Part of the reason for a widening of the theory-practice gap in the strategic management field, we suggest, is that with limited exceptions,[11] qualitative- and process-research (i.e., inquiry from the inside) has largely atrophied in the past three decades, and relatedly meaningful interactions between researchers and practitioners

tentative first proposal" (2007, p. 46). Thus, our focus is not on theory using, but on theory finding; our concern is not with the test of hypotheses, but with their discovery. Eisenhardt and Graebner (2007) and Ketokivi and Mantere (2010) provide insights on the abductive process for theory building in management research. Exemplars of abduction and engaged scholarship include Bingham *et al.* (2007), Maritan (2001), and Shah (2006).

[10] This notion is akin to saying, "If you want the right answer, you need to first ask the right question." Relatedly, American engineer, inventor, and founder of General Motors' Research Laboratories, Charles F. Kettering suggested: "A problem well stated is a problem half-solved."

[11] For example, work by Eisenhardt and her students and co-authors serve as an exception and offer some exemplars of the type of much needed high quality qualitative- and process-research that has been in decline in recent decades.

are less frequent. Bowman (1990) makes the following case for the importance of such interactions:

> There is always the risk that the professor would rather interact intellectually with other professors and doctoral students than with executives. While the first interaction is obviously worthwhile, to miss the second is folly. Most of us exist in professional business schools that, as with all professional schools, exist to help the professions — the worldly managers and managers-to-be . . . The practitioner and the researcher are *doubly linked* [italics added]: the researcher supplies insights, relationships, and theory for the practitioner; But the practitioner supplies puzzles, ideas, judgments, and priorities for the researcher (1990, pp. 25, 27).

Thus, effective problem formulation, aided by improved interactions between researchers and practitioners as Bowman (1990) suggests, is perhaps the most important means of bridging the theory–practice gap (Hambrick, 1994; Hambrick, 2007; Hodgkinson and Rousseau, 2009). The path towards such effective problem formulation is arguably through a *triangulation process*[12] (Denzin, 1978; Jick, 1979). Basic social science is grounded in practical experiences drawn from insights of key stakeholders (e.g., researchers, users, clients, sponsors, and practitioners) and/or the practical experiences of the authors themselves in attempts to better understand managerial problems (Van de Ven, 2007; Van de Ven and Johnson, 2006). Thus, the theory–practice gap is not simply the product of a *knowledge transfer problem* (e.g., *converting or translating* knowledge from theory to practice to facilitate its transfer) (Carlile, 2004; Tranfield *et al.*, 2003) as many in the field incorrectly presume. Rather, this gap is a much more fundamental *knowledge production problem* (i.e., a problem in how we create knowledge and what knowledge we create). Thus, to effectively ground theory-driven knowledge in practical problems, more practitioners of management must be consulted and involved in the research process (Huff, 2000; Rousseau, 2006). Consultation with such highly informed stakeholders can better define problems, and during the course of both qualitative and quantitative research, can shed light on the interpretation/meaning of the data (Diesing, 1971; Jick, 1979).

[12] *Data triangulation* uses multiple ways to collect and analyze data. *Investigator triangulation* uses multiple rather than single observers. *Theory triangulation* uses multiple theoretical lenses to interpret phenomena. *Methodological triangulation* uses more than one quantitative and/or qualitative methodology. *Multiple triangulations* refer to combining in one study data, investigator, theory, and/or methodological triangulation (Denzin, 1978; Jick, 1979). The pluralist position we take here leaves room for the role of specialization; in other words, not all scholars need take the path of a triangulation process and engaged scholarship, but rather we suggest the vital *complementarity* of this path to basic social science research (Van de Ven, 2007).

## Exemplars of Problem-focused, Engaged, and Pragmatic Scholarly Discovery Logic

Historically, the theory–practice gap was far less prominent than it is currently in the management field because a large number of our seminal works gave greater attention to the logic of discovery. These works employed engaged scholarship by grounding their problem formulation and their solutions in pragmatic real-world contexts (see e.g., Schelling, 1960; Selznick, 1957). Exemplars of such impactful research[13] include:

- *Administrative Behavior* by Herbert Simon (1947), which was influenced by his consultations with Chester Barnard of New Jersey Bell Telephone, as well as informed by Barnard's (1938) *The Functions of the Executive.*

- *The Theory of the Growth of the Firm* by Edith Penrose (1959), which drew on practical insights that she gained from immersion in the operational and strategic (diversification) problems of General Motors-Holden Ltd. and the Hercules Powder Company (Penrose, 1960).

- *Strategy and Structure* by Alfred D. Chandler (1962), which drew on the historical records of practitioners and managerial problems of Dupont, General Motors, Standard Oil, and Sears.

- *A Behavioral Theory of the Firm* by Richard Cyert and James March (1963), which was informed by their careful examination of routines and pricing within a retail department store.

- *Corporate Strategy* by H. Igor Ansoff (1965), which drew on and built from Ansoff's extensive work experience and discussions with managers at the Rand Corporation and Lockheed.

- *Markets and Hierarchies* by Oliver Williamson (1975) and his path-breaking research on vertical integration, which was grounded in his involvement in the mid-1960s at the Antitrust Division of the U.S. Department of Justice, and was coupled with insights gained from a transaction cost economics perspective, both of which highly influenced this seminal work.

- *An Evolutionary Theory of Economic Change* by Richard Nelson and Sidney Winter (1982), which drew on their experiences at the Rand Corporation and at President John F. Kennedy's Council of Economic Advisers.

[13] *Google Scholar* citations (as of December 13, 2019) are as follows: Simon (1947): 30,661 citations; Penrose (1959): 34,466 citations; Chandler (1962): 22,934 citations; Cyert and March (1963): 30,485 citations; Ansoff (1965): 14,055 citations; Williamson (1975): 43,567 citations; and Nelson and Winter (1982): 43,279 citations.

To illustrate the logic of discovery research process within the knowledge production process of engaged scholarship that underpins these seminal works in the strategic management field, we offer a more detailed exploration of four of these exemplars: Penrose (1959), Chandler (1962), Ansoff (1965), and Williamson (1975).

*Penrose (1959)*

Edith Penrose's (1959) seminal work, *The Theory of the Growth of the Firm*, drew on practical insights from many primary and secondary sources. These sources included: (1) interviews with managers pragmatically rooted in real-world problems (see Richardson, 1964); (2) research on annual reports and studies of business history[14]; and (3) field studies and extended company visits and observations, such as those at General Motors-Holden Ltd. in Australia, and the Hercules Powder Company in Delaware. Penrose's inductive case study of the Hercules Powder Company during the summer of 1954 — and published in Penrose (1960) — informed many of Penrose's (1959) deductive arguments. Kor and Mahoney (2000) summarize Penrose (1960) as follows:

(1) There is a close relationship between resources and the ideas, experience, and knowledge of a firm's managers (1960, p. 2).

(2) Managers' references to their own firm's resources shape their expectations (1960, p. 3).

(3) Complementary assets enhance a firm's capabilities (1960, p. 5).

(4) Specialization can lead to diversification (1960, p. 7).

(5) Scarcity of firm-specific managerial talent can limit firm growth (1960, p. 21).

(6) The entrepreneurial nature of a firm largely determines how imaginatively and rapidly it can create and discover opportunities (1960, p. 23).

Penrose's classic work involved an iterative process of induction, abduction, and deduction, informed by multiple levels of pragmatic inquiry, which helped link the dynamic interactions of resources, capabilities, and entrepreneurial imagination (Kor *et al.*, 2016). It illustrates substantive research outcomes generated from a process of problem formulation and engaged scholarship.

[14] Kor and Mahoney (2000) provide a list of 15 historical business works that Penrose (1959) acknowledged as influential to her seminal book. For example, Penrose (1959) reveals her engaged scholarship approach by noting that: "Charles H. Wilson's *History of Unilever* is a model of what good firm histories can be. I have leaned heavily on this type of work (and there are some others), as well as on direct discussions with businessmen, for insight into the processes of firm growth" (1954, p. 3).

*Chandler (1962)*

*Strategy and Structure* by Alfred Chandler (1962) drew on historical records of practitioners and their managerial problems (e.g., Dupont, General Motors, Standard Oil, and Sears). Chandler's focus on business organization and strategic management began with his dissertation and first book, which was a study of his great grandfather, Henry V. Poor, whose name and legacy continue to this day as one half of Standard and Poor's, one of the big three credit rating agencies. This early work by Chandler led to an invitation to teach at the Naval War College in Rhode Island and to collaboration opportunities with William Reitzel of the Brookings Institution, which is one of Washington DC's oldest think tanks. These experiences in working through detailed case studies on major structural changes in large-scale organizations provided the foundations for Chandler (1962), which emphasized the fundamental change from centralized, functionally departmentalized organization structures to a multidivisional structure with a number of product or geographic divisions. Greater requirements of complexity led several pioneering organizations to adopt this multidivisional structure, which enabled partitioning of tactical and strategic decisions. The choice of the multidivisional structure was an adaptive response by:

(1) DuPont to their rapid product diversification strategy into several industries;

(2) General Motors to their strategic decision to move into a number of different lines of automobiles, trucks, and parts;

(3) Standard Oil Company to their increased geographic scope through operations in international markets; and

(4) Sears to the increased demands, which followed their strategic move from a mail-order house to a leading retail chain.

From these insights, Chandler defined *strategy* as "the determination of the basic long-term goals and objectives of an enterprise, and the adoption of courses of action and the allocation of resources necessary for carrying out those goals" (1962, p. 13). Chandler likewise defined (organizational) *structure* as "the design of organization through which the enterprise is administered" (1962, p. 14). The primary thesis of *Strategy and Structure* is that growth and diversification strategy led to the adoption of the multidivisional structure. Indeed, Chandler articulated that "unless [the multidivisional] structure follows [the diversification] strategy, inefficiency results" (1962, p. 314). Further, the multidivisional structure then "institutionalized this strategy of diversification" (1962, p. 394). In summary, these seminal ideas in the history of thought in the strategic management field introduced in Chandler (1962) are a byproduct of pragmatic inquiry, which helped produce a well-grounded formulation of managerial problems, deep knowledge of organizational and institutional details, and path-breaking theory development (see also Chandler, 1977; Chandler, 1990).

*Ansoff (1965)*

*Corporate Strategy* by H. Igor Ansoff (1965) builds on his experiences with the Rand Corporation and Lockheed Aircraft Corporation where he served as a senior corporate planner before moving to academia in 1963 and penning this seminal work. Ansoff acknowledged the conversations with, and contributions of Robert Gross, former Chairman of Lockhead, along with many other associates at Lockheed. Ansoff's research involved the complex decision process of strategy formulation and was decidedly pragmatic in seeking "to develop a practically useful series of concepts and procedures, which managers can use to manage" (1965, p. vii). Specifically, Ansoff articulated that: "the central purpose of the business firm is to maximize long-term return on resources employed within the firm" (1965, p. 38) and identified the allocation of resources among product-market opportunities as a fundamental strategic problem facing many business enterprises. Ansoff (1965) also introduced the strategic tools of *capability profiles* (1965, p. 76) the *grid of competences* (1965, p. 92), the *product–mission matrix* (1965, p. 128), and championed evaluation of various types of synergies including sales, operational, investment, and management. In summary, this pioneering pragmatic work in engaged scholarship by Ansoff (1965), moved the strategic management field forward by providing key insights on processes for creating competitive advantage, and skillfully bridged theory and practice.

*Williamson (1975)*

The seminal book, *Markets and Hierarchies*, by Oliver Williamson (1975) was influenced by the grounded knowledge he gained at the Antitrust Division of the US Department of Justice in the mid-1960s. Beginning with an invitation by Carl Kaysen in the spring of 1966 to serve as the Special Economic Assistant to the head of the Antitrust Division, Donald Turner, Williamson became engaged in a wide set of antitrust issues. In this role, Williamson also worked with Stephen Breyer, another of Turner's Special Assistants, and with Richard Posner in the Solicitor General's office. As part of this role, Williamson was involved with the Schwinn case of vertical coordination (where Williamson advised Turner and Posner against proceeding with their market power line of argument, but in which the advice was too late to influence the brief), and the Ford-Autolite merger, where vertical integration issues were posed

(Williamson, 1986, p. xv). These experiences highly influenced Williamson's (1975) formulation of the canonical problem of vertical integration, and the subsequent development of transaction cost economics with an emphasis on the behavioral assumptions of bounded rationality and opportunism, and the environmental conditions of uncertainty and small-numbers bargaining. The catalyst for developing this new theory was Williamson's dissatisfaction with theories at the time (e.g., market power, variable proportions distortions, and technological interdependence), which did not adequately explain and predict the vertical integration decisions observed in the real world (Ketokivi and Mahoney, 2016). Williamson's strong grounding in real-world problems and his engaged scholarship illustrate a key pathway for formulating new problems, rationally reconstructing older problems, and developing new theory (Ladd, 1987; Weick, 1989).

The exemplars of Penrose (1959), Chandler (1962), Ansoff (1965), and Williamson (1975) illustrate the logic of discovery within the knowledge production process of engaged scholarship, which have highly influenced the development of the strategic management field since their publication.[15] Patterns that we identify indicate that multiple levels of pragmatic inquiry and engaged scholarship with various real-world stakeholders informed each of these seminal works. Such engaged scholarship enabled new problem formulation and path-breaking theory development. There is either dissatisfaction with how current theory addresses a traditional problem in the field, or a new question in the field is introduced. Typically, these researchers followed a logic of discovery through multiple triangulations and an iterative process of induction, abduction, and deduction.

[15] The positive effect of engaged scholarship (i.e., basic social science research, which solicits key stakeholder advice from users, clients, sponsors, and practitioners) is demonstrated not only by seminal research books, but also by impactful articles. We submit that all 29 (https://www.strategicmanagement.net/awards/smj-best-paper) recipients to date since 1993 of the *Strategic Management Society's* Dan and Mary Lou Schendel best paper prize, which recognizes some of the most impactful research published in the *Strategic Management Journal*, directly or indirectly qualify as engaged scholarship. Of these, we highlight five exemplars: (1) 2013 winner Barr *et al.* (1992) provides a highly informed analysis from a matched pair of US railroads to suggest that strategic renewal requires that managers link environmental change to corporate strategy and to modify that linkage over time. (2) 2001 winner Leonard-Barton (1992) considers core capabilities and core rigidities through engaged scholarship in drawing insights from managers at Chaparral Steel, Ford, and Hewlett-Packard. (3) 2010 winner Henderson and Cockburn (1994) offers empirical findings on component and architectural core competencies in the pharmaceutical industry that are informed by managers from multiple firms. (4) 2004 winner Szulanski (1996) analyzes internal stickiness of knowledge transfer and tests the resulting model based on 122 best-practice transfers in eight firms. (5) 2015 winner Tripsas and Gavetti (2000) shows the importance of managerial cognitive representations in directing search processes in a new learning environment, the evolutionary trajectory of organizational capabilities, and processes of organizational adaptation, which are informed by managers at Polaroid.

The main question emerging from the lessons in these exemplars is "how do we facilitate such a research approach in the modern age to produce relevant and impactful knowledge that improves the collective welfare of the business school, the management field, and society?" This challenge is a persistent one for business schools, and indeed for all professional programs, which have the "common problem of bridging the gap between the social system that produces scientific knowledge . . . and the social system where professional practice takes place . . . " (Simon, 1967, p. 16); (see also, Khurana and Spender (2012); and Rousseau (2012)).

## Bridging the Theory–Practice Gap: Identifying Issues, Problems, and Impediments

Part of the earlier statement from Bowman (1990) is worth repeating: "The practitioner and the researcher are doubly linked: the researcher supplies insights, relationships, and theory for the practitioner; But the practitioner supplies puzzles, ideas, judgments, and priorities for the researcher" (1990, p. 27). What Bowman (1990) sees is something like an old-time firefighting bucket brigade of people passing buckets of water from the water source to the fire, a double loop: buckets of water going to the fire and the emptied buckets going back to the water source. In such a process, both sides of the loop must be complete to extinguish the fire. In reference to strategic management research, such a doubly linked loop creates a symbiosis between advancing knowledge and its application to practice. In the applied field of strategic management, we see a number of reasons for this weakened symbiosis, which threatens the field in terms of the relevance of the research undertaken and thus as a worthwhile source of applications to real-world practice.[16]

We see gaps in this double-loop linkage on both sides. Problems that are grounded in reality are not reaching researchers with the capacity to undertake relevant research, and practitioners are losing (or have lost) connection to needed help in addressing the real-world phenomena they face, increasingly without a perception on their part that the potential to advance the level of management expertise is even available. As the gap widens, the symbiosis withers, and each side separates from the other. In the remainder of this monograph, we deal simultaneously with two significant questions: (1) "what conditions (i.e., "problems") are causing these gaps?" and, (2) "what can be

[16]Factors enabling research relevance include (1) engaged scholarship to ground the strategic problem in reality; (2) providing novel and useful theoretical concepts and constructs; (3) revealing concrete and measurable consequences of the variables; (4) showing a clear tradeoff among variables; (5) addressing variables under managers' control; and (6) triangulating through use of quantitative and qualitative approaches (Van de Ven, 2007; Vermeulen, 2007).

done to close the gaps (i.e., "solutions"), and by implication, improve both the theory and practice of strategic management?" To address these two questions, bear in mind that the double loop acts in both directions, and that we face not only a *knowledge transfer and application problem*, but also a more difficult *knowledge production or creation problem.* To solve each of these difficult challenges involves issues for both sides of the double loop. To simplify our problem and solution discussion, let us call one end of the loop, the *practice* side, (i.e., the side that feeds real problems, questions, challenges, and issues to the researcher). The other side of the loop is the *research* side, which has a responsibility to interact with practitioners directly and not only transfers concepts, ideas, theories, and suggestions to the practitioner, but also has the task of identifying theoretical and conceptual means of value to the practitioner. Such an undertaking effectively suggests that the research-side needs well-grounded, interdisciplinary research, which is a considerable challenge in its own right.

## Practice-Side Issues: Changing Roles for Academics and Consultants

The practice side is well defined and essentially related to organizations that are involved in providing the goods and services that some buyer segment values enough to allow generation of sufficient resources necessary for organizational survival. It is in managerial work that phenomena, problems, and puzzles arise, and for whose solutions, professional managers seek relevant knowledge. Earlier in this paper, we showed how some of the seminal works that underpin the strategic management field came about by researchers deeply embedding themselves in practice. There are institutional reasons why many of today's researchers no longer see such efforts as necessary, or even of value, an issue we examine later in this paper. However, neither do contemporary researchers have the same opportunities to have first-hand experience in practice given the doctoral program training protocols we examine later, nor are the practice problems quite as simple to observe or identify as they once were.[17]

In an earlier time-period, the most advanced and complex organizations, while growing ever larger, were still substantially single-business firms operating within a single technology and industry. Developing, however, in the conglomerate merger wave of the 1960s were more difficult organization problems as firms became more complex and their environments changed more rapidly (Williamson, 1975). Adding to the complexity and growing size, firms needed to operate globally across many, different sovereignties and in multiple, widely differing business environments comprised of different businesses, technologies, and industries (Ghemawat, 2007; Reuer and Leiblein, 2000). Further,

[17] It is also worth asking whether a reduction in case-writing activities, which was once more prevalent by research scholars than currently, has been a contributor to the gap between research and practice.

professional management teams were finding it harder to obtain the help they needed from the research side, just as the research side was finding it harder to understand the new scope of complexity these emerging conglomerates presented.

*Rise of the Large Consulting Firms*

Because practitioners were encountering new issues and challenges in managing these conglomerates, sovereignties, and competitors not before seen in practice, they began looking for help wherever they could find it. Apart from textbooks, formal education beyond textbooks, or even the knowledgeable academic professor, all of which practitioners found increasingly out-of-date, managers in all types of organizations looked to a new entity forming to help them meet these new complexities: today's very large consulting firms specializing in strategy. Perhaps the most notable example was 48-year-old Bruce Henderson's founding of the Boston Consulting Group (BCG) in 1963, and its several offshoots such as the founding of Bain & Company by Bill Bain in 1973, as well as the older firms such as McKinsey & Company, and Booz Allen Hamilton (Henderson, 1984; Kiechel, 2010). These consulting entities were able to marshal the size and scale of teams of research workers needed for interdisciplinary (consulting-type, quickly monetized) research. They could do the required volume of work in a timely way to meet new challenges that were no longer possible to accomplish by the largely fragmented capabilities of the academic-based research side of the loop. The academic side essentially became an indirect "supplier" by training research workers for "middleware" consulting firms, in addition to its traditional direct output of trained workers for practice.

Given the scale they could muster and their direct access to practice, consultants were now the ones with the opportunity to become steeped in practical experience and well-grounded problems — much more so than the academic side. Consultants were more prepared to do the work of finding potential concepts, perspectives, and tools capable of helping with growing practice complexities. The barrier that formed between the two sides of the double-loop linkage is due in part to the appearance and the manner of work characteristics of these consulting firms and, in part to the barriers arising from the large-scale movement away from practice-motivated research that conventional business school academics were undertaking. These barriers between practitioners and business school academics arose for several other reasons. The consulting firms, departing from the early BCG model, and following the pioneering proprietary business model of Bain & Company in the 1970s, largely focused on their competitive positioning (Semadeni, 2006), and typically kept their knowledge — e.g., market information, observational data, and best practices experiences — proprietary, thus impeding dissemination important to advancing knowledge. Because these consulting firms were loath, for competitive reasons, to widely share what they learned, these proprietary tendencies hindered the traditional academic researchers' work, whose public good incentives are open publication and wide circulation of knowledge, as well as lessened opportunities for academic researchers to ground their work in real-world problems.

Moreover, these consulting firms received substantial fees for their help with problem solutions, providing financial resources not directed toward funding academic research. Thus, a further important barrier was that academic researchers were without the resources to undertake the more complex work required because of the loss of connections to the practice side of the loop. Further, the practitioner side of the loop was no longer easily influencing the academic researcher side (and *vice versa*), which increasingly lacked the resources that allowed the creation of the scale of teams needed to maintain relevance and to create and transfer the theory and concepts required of the complexity arising in practice (Kiechel, 2010). In some ways, the double loop still exists with the larger consulting firms ostensibly taking on the researcher's knowledge creation and knowledge transfer function, the role held by what were largely academic-based researchers.[18] The consulting firms have been able to do so by monetizing the practice challenges in ways that permit them to fund the scale and complexity needed to deal with problems arising in the world of practice.

If there is a gap forming and growing that eliminates the traditional researcher's access to practice, then conventional approaches to the strategic management field's growth, in terms of conceptual and theory development by the traditional academic researcher, are at risk. Can medicine advance without access to patients and treatments for disease? Can civil engineering and structural mechanics advance without bridges to build and increased understanding of new materials? By this logic, the applied field of strategic management that examines and is devoted to improving the efficiency and effectiveness of organizations will not likely be sustainable without attention given to practice. Thus, reducing the current theory-practice gap requires direct access to the practice side as well indirect access through consulting entities. Yet currently, such access is decreasing and perhaps even disappearing altogether, which is clearly problematic for both the research and practice sides.

[18]We seriously question, however, whether consultants are taking on knowledge creation the same way that skilled academic researchers can take on knowledge creation through various engaged scholarship processes. Consultants typically do not have the interests, incentives, or skills to create such knowledge. Consultants have a lot of experiences, field data, and (sometimes-pertinent) war stories. However, they are far from advancing theories and knowledge at the level of basic managerial principles, which connect know-why with know-how.

Consulting firms, in effect, challenge the public goods role of the business school academician embedded within university institutions of higher learning and researchers who have been the conventional provider of theory, concepts, and knowledge needed for application at the practice part of the double-loop linkage discussed earlier. This creation of knowledge and especially its collection and dissemination have traditionally been in the hands of academic researchers directly incentivized to perform public goods work,[19] and are unlike consultants who have a different (private goods) system that creates economic incentives for them to be secretive and out of the loop by design.

Earlier we discussed *engaged scholarship* to highlight the idea of both learning from the real world the current problems lacking theory and concepts not yet developed to solve them, and the problems of the application of new theory and concepts. Engaged scholarship, without a public goods orientation, is what consultants do. So, who and how might the gap between academics and practitioners be bridged now that consultants are firmly established in the loop? First, it would help greatly to bridge the gap in the loop if the larger consulting firms were more open to participation in research that could be more widely distributed through academic channels, or even more disseminated through the business practice literature. Bruce Henderson's BCG *Perspectives*, which provides short essays of typically 800 (well-written) words on new concepts, ideas, and nagging business questions, serve as (over 400) examples of the latter (Kiechel, 2010, p. 21). The *McKinsey Quarterly* is another, similarly notable, longstanding example. Likewise, traditional researchers located in academe and possessing the norms and skills of both disseminating and advancing knowledge should make more of an effort to reach out and collaborate with consultants. At a minimum that means maintaining or renewing our consulting ties for academics with prior work experience in this domain, and for keeping in touch with our former students who pursue their post business-school careers in strategy consulting. One outcome of such outreach by academics to consultants (and vice versa) would be to seek funding opportunities for research projects through temporary (e.g., summer research) employment, financial grants, or just plain openness within the infrastructure that exists to share common

[19]Huff notes: "Research institutions, if sheltered from the immediate need to generate significant income from their knowledge production activities, can produce "public goods" that companies and consultants cannot credibly produce. Business schools also offer a desirable, neutral ground on which new, more synthetic knowledge can be generated from the interaction of individuals with diverse business, consulting, public, and university experience" (2000, p. 292). Huff (2000) goes on to argue that a pure disciplinary mode ("mode 1") driven by the theoretic agenda of academic elites, and which is detached from real-world problems will receive decreasing public support. In contrast, the market is the sole driver of the consulting mode ("mode 2"). Building on Huff (2000), we submit that through double-loop learning, the problem-finding and problem-solving approach, and engaged scholarship ("mode 1.5") business schools can redress limitations of both modes of knowledge production.

problems and/or disseminate created knowledge that is no longer a source of competitive advantage to the firm. For such collaborative/action/design science research to take place (Bartunek and Louis, 1996; Lewin, 1951; Simon, 1996), a real issue is funding. In addition to the inroads that consultants have made, there is also a growing institutional and professional disconnect between the research behaviors that scholars should pursue for the collective good of the field and those that scholars are incentivized to pursue by their academic institutions and professions.

## Research-Side Issues: Interdisciplinary Research and Institutions of Higher Learning

In this section, we address some of the structural and incentive issues that need to change in the business school/university and/or larger institutional environment for directing scholars to pursue impactful research more intensely for the collective good.[20] Such structure and incentive problems exist at multiple levels, such as, in research training, academic recruitment, promotion and tenure processes in business schools, and institutionally among academic journals and professional societies, associations, and accreditation bodies. On their own, but in different ways, these problems are influencing researchers to remove themselves from the double loop, which we have emphasized as critical to advancing the field.

### *Incentive Issues with Doctoral Training and Faculty Recruitment*

We begin our discussion of incentive issues with our approaches to management doctoral training, the source of academic research skills that provides an institutionalized and reinforced license throughout a modern scholar's academic career. A major issue in this area is that much of our doctoral training limits doctoral students by having them focus only on filling gaps in existing theory by use of a single research methodology (Bartunek *et al.*, 2006; Summer *et al.*, 1990; Vermeulen, 2005). Further, most current doctoral training is almost exclusively (extant) theory and methods driven, rather than phenomena- or problem-driven, further placing relevance at risk. In contrast, as a solution to enable scholars to be more capable of executing impactful research, doctoral students need to be trained to use triangulation approaches (Allison, 1971; Denzin, 1978; Jick, 1979) mixing qualitative and quantitative methods to ground social science research in practical experiences and insights drawn from

[20]Structural, incentive, and institutional factors are key sources of a collective action problem leading to an under-supply of engaged scholarship in the management field (Gulati, 2007; Van de Ven, 2007). Thus, encouraging a knowledge production process of engaged scholarship requires changes in these factors.

key stakeholders (e.g., researchers, users, clients, sponsors, and practitioners). Methodologically, triangulation approaches are typically more time consuming and may impose higher risks of failure for doctoral students who are often less concerned about the application of findings, and are more concerned with meeting standards to help launch their own careers within the current incentive system. All these factors conspire to limit a doctoral student's ability to acquire the training and capabilities to design and execute impactful research via engaged scholarship.

Further, while exceptions may be found, structurally, there is an expectation that successful interview selection, and university employment of a doctoral candidate, depends significantly on having published in top-tier academic journals.[21] Thus, doctoral programs (and/or observant Ph.D. students) mindful of this "proxy" for candidate quality and employability, pursue doctoral training and research programs that they perceive as more likely to result in published research. The problem is therefore one of both training and incentives where we cannot simply change doctoral program training without also changing the related employment incentives for publication.

To examine this latter incentive problem, we first ask, what do top-tier journal publications have to do with the logic and process of creative discovery and the production of impactful research? We suggest that the answer is "very little," as evidenced by numerous practitioners and consultants who complain that they cannot understand the published journal articles (or conversely the numerous academics not pursuing research questions based on topics, or producing knowledge, that is practically relevant, and/or effectively translating their research results [when they do] to better facilitate such knowledge transfer). Three key factors often drive such narrow gap-filling, publication-focused research: (1) the set of recent research questions and conversations occurring in the top journals (i.e., the source of the "gaps" perceived in need of filling); (2) the availability of (and popularity of certain types of) data (i.e., easy data accessibility); and (3) the novelty/fashionableness of the methodology(ies) currently in vogue in top-tier journals. The resultant (largely closed-loop) research output from such drivers of these three factors is a collective knowledge product that is often irrelevant to contemporary practice (even if translated effectively) and of little significance in contributing to the field's knowledge base.

The collective consequence for the field of this type of doctoral training and publication focus is a closed-loop, self-reinforcing process of scholars with an ever-narrowing set of research skills and output (Hambrick, 2007). This

[21]Part of the problem might be that doctoral programs do not significantly emphasize recruitment of experienced students in the admissions process. The incentives for doctoral students and faculty to focus exclusively on top-tier journals may result in lower variation in knowledge creation, which inhibits evolutionary advancement in scientific knowledge. We thank an anonymous reviewer for this insight.

incentive-driven process over time has resulted in the "doubling and re-doubling of research" on problems that are often not "grounded in reality" (Van de Ven, 2007, p. 13), and has led to the strategic management field's current predicament. To attenuate this relevance problem, doctoral students could leverage executive education settings to get closer to their basic phenomena of interest and to test their ideas by interacting with managers on campus. Tushman and O'Reilly suggest that: "engaged scholarship and relationships anchored in joint respect for research as well as relevance are associated with virtuous cycles whereby knowing affects doing and doing, in turn, affects knowing" (2007, p. 771).

The incentive-driven, publication-focused, training problem is so pervasive that several prominent scholars observed that the last several annual meetings of the Academy of Management (AOM) had at least a dozen or more workshops or sessions for doctoral students and young scholars on "how to publish" in top research journals, but virtually no workshops or sessions on "how to do research." One might expect that scholars would concur with the proposition that it is far more important to first train doctoral students on how to do relevant research, and then on how to publish it. Yet, as a field, we now seem to have the "cart" firmly fastened before the "horse." Given the current incentive problem in doctoral student training, only a small handful of doctoral students at more enlightened business schools are somewhat unencumbered (initially) by such structural impediments, and hence genuinely have the option of pursuing more impactful avenues for dissertation research, which would allow and even encourage them to pursue triangulation approaches (Denzin, 1978; Jick, 1979). The former Business Policy & Strategy (BPS)[22] Division's Kauffman Foundation Supported dissertation award program within the AOM has recognized several mixed method and/or more practically relevant dissertations in recent years. This type of recognition, as well as the Strategy Research Foundation's (SRF) funding initiatives[23] for multi-year dissertation research grants, both offer current examples of efforts towards addressing the doctoral training incentive problem. The field would benefit from many more such positive incentive changes, as it would from the previously mentioned need for more practitioner input.[24]

Structurally, on the supply side, doctoral-granting institutions may need to increase the length of time for support of doctoral students in their

[22] Now known as the Strategic Management (STR) division as of August 2017.

[23] The SRF's Research in Organizations (RIO) funding program also serves as an example of the type of much needed programs for facilitating practitioner input and academic-practitioner research interaction for faculty beyond their doctoral training.

[24] We are pleased to note that the SMR has an advisory board of senior executives and consultants, with the goal for academics to learn from them and to identify pressing strategic challenges and research questions. Advisory board members will also be attending special mini-conferences, affording junior scholars with little or no business experience or consulting activities opportunities to learn from them.

Ph.D. programs (as many have done or are beginning to do). This change would allow doctoral students the time to both pursue, and have an opportunity to publish more impactful, relevant research, assuming current publication proxies for candidate quality remain the norm. The demand-side problem is far more challenging as there is not an easy, foreseeable alternative (or means of incentivizing institutions to adopt such an alternative) to using journal publications as a proxy for applicant quality in their faculty recruitment processes (Huff, 2000). Faculty hiring committees within a university that take the time to read a candidate's dissertation, and extend interviews, and make hiring decisions on the collective faculty's informed judgment of the potential of the candidate's dissertation research are far more the exception than the norm (Gulati, 2007). As such, short of major changes in the criteria and incentives for faculty promotion and tenure, or an imposition from an accrediting body (e.g., AACSB) for different faculty performance-assessment criteria, this is a deep structural challenge to overcome.

### *Incentive Issues with Faculty Promotion and Tenure Processes within Business Schools*

Structural and incentive issues in faculty promotion and tenure processes in business schools exacerbate the collective action incentive problem. The most prominent structural impediment in the business school is the "publish or perish" mantra. In this paradigm, which is prevalent in most AACSB accredited business schools in North America (and now much of the world), a young scholar typically has a probationary period varying from three- to seven- (or more) years to produce a quantity of high quality research publications, or face termination of their faculty position. One top journal publication per year is a commonly quoted metric at many ranked business schools for achieving promotion and tenure in the five- to seven-year probationary period (Certo *et al.*, 2010). Such high publication pressures[25] incentivize scholars to pursue the types of research most likely to result in publication (i.e., "narrow gap-filling data accessible research" stemming from their publication-focused doctoral training and usually closely related to, or extensions of, current research streams). Even more problematic, such high publication pressures may incentivize some scholars to engage in lapses from appropriate and ethical research practices (Byington and Felps, 2017; Honig *et al.*, 2014). Furthermore, the downsides of such incentives may include scholars sacrificing consummate quality in teaching and service activities (including much needed business and

[25] Certo *et al.* (2010) find that the majority of management scholars require more than five years to publish five top-tier journal articles, with the average time required to publish five articles taking 9.72 years by the end of their sampling period from 1988 to 2008. The result of this "audit society" is that instead of focusing on the logic of discovery, academics' incentives focus on quantity of publications in prestigious journals (Adler and Harzing, 2009). All but forgotten in this publication race is transcending the sense and non-sense of academic rankings and producing and communicating research findings for the public good.

community engagement) to allocate more time to research productivity, which may not be impactful.[26]

How might business schools orchestrate research productivity that is more impactful? Major changes in the criteria and incentives for faculty promotion and tenure are likely required. One step would be for business schools to count and to reward potentially more impactful scholarly activities such as research books and external grant funding in the promotion and tenure decision (as do some other Departments and Colleges within a University). Research books would allow for triangulation and engaged scholarship processes, as would perhaps practically oriented external grant funded and/or sponsored research projects. Another step may be to allow longer time-periods for tenure decisions, as well to incentivize more private-sector collaboration and funding opportunities for research interaction. However, in practice, the step that many schools appear to be adopting by default is to develop multiple tiers of faculty, which allow specializations for faculty to allocate the majority of their time to teaching, research, grant writing, or service (Gulati, 2007). One can observe this practice through use of clinical, adjunct, or visiting (non-tenure-track) faculty and the use of doctoral students to cover increasing teaching loads, while tenure-track or tenured faculty may increasingly specialize in research, grant writing, or administrative service, depending upon their research productivity and preferences. The risk of this practice is that it may lead to an entrenched two-tiered or dual-class faculty system that reduces a sense of collective purpose and shared responsibility, and may serve to undermine the effectiveness of the organization's norms.

## Research-Side Issues: Academic Journals, Societies, and Accreditation Organizations

Finally, perhaps the most serious incentive problems, due to the downstream implications for business schools and universities, are the structural and incentive issues among our academic journals, professional societies, and accreditation bodies. We discuss each of these, in turn.

### *Incentive Issues with Academic Journals*

Expectations are that our academic journals will disseminate and communicate the collective knowledge produced by the field, while at the same time ensuring its rigor and relevance. This knowledge management task is dependent upon a largely voluntary, often partially *ad hoc*, editorial review process. Yet, this

[26]There are issues raised also about the definition of a top-tier journal, and, however defined, whether there is sufficient capacity within this set of journals to allow scholars the opportunity to produce innovative research and to publish their work at the rates required by promotion policies.

knowledge product is also the "coin of the realm" for our profession. It plays a central role in recruitment, promotion, tenure, and career mobility, often serving as a generally accepted *de facto* proxy for faculty productivity and quality. This system is severely strained and is confronted by the current herculean task of managing and ensuring the quality of the knowledge product of the field. With the number of scholars producing research increasing, and the number of schools recruiting and assessing these scholars based on their research productivity, how do those administering our academic journals ensure the quality and impact of this research? Is the growth and capacity of top-tier journals keeping up with the capacity to generate knowledge? Moreover, if research published is to be impactful in the sense of application to practice, are the published journals accessible and readable by practitioners? Institutions increasingly incentivize their faculty to publish their research in top-tier journals, and thus to conduct the types of research they believe to be of interest to the editors and reviewers of those journals. Because of these motivations, there is a closed loop of incrementally extending less impactful research.

Further, a research journal must compete with a set of peer journals, for editors, reviewers, and manuscripts. The measure of such competitiveness is itself assessed through proxy measures, the most prominent likely being a journal's impact factor — essentially a measure of the relative average number of citations to the research previously published in a journal. This competition among academic journals creates another incentive problem. The need to maintain or increase a journal's impact factor incentivizes publishing research papers that cite articles the journal previously published, and to (tacitly or actively) encourage more citations to research previously published in that journal. Such incentivized coercive citation behavior raises serious ethical concerns (Martin, 2016).

### *Incentive Issues with Academic Professional Societies*

Similar incentive problems also exist with our academic professional societies and associations. First, these organizations are managed and led by many of the same scholars running the editorial and review processes at the journals, as well as their younger protégées and doctoral students seeking to advance in the profession. Second, while the journal impact-factor incentive problem is not operative, most of the other collective action problems affecting our fields' academic journals also affect our field's academic professional associations. However, the largest and most pervasive issue is perhaps the low participation of business practitioners ("B's") and consultants ("C's") in our professional societies.[27] The former BPS Division of the AOM noted this growing concern

[27] For example, already low rates of US based "executive" membership in the AOM's former BPS (now STR) Division declined by 9.2%, while non-US based "executive" membership declined by 4.1% from 2011 to 2015 (Bidwell *et al.*, 2016).

in its most recent five-year review (Bidwell *et al.*, 2016, p. 18), which reports that:

- "...executive membership [is] in sharp decline."
- "An increasingly academic membership squares with repeated concerns about the link between research carried out by division members and business practice."
- Practitioners and consultants see no benefits to their exposure to academics and they "...do not attend the meeting to gain and share information relevant to practice."

This report suggests that efforts need to be made by AOM leadership to connect "research presented at the conference with real-world issues" and that they should "take responsibility for fixing the increasing disconnect between research and practice" (Bidwell *et al.*, 2016, p. 18). It is positive to note that the report recognized the inherent "applied nature" of strategic management knowledge and that resolving the gap will likely require effort on both sides. It also advocates that the "researcher should be aware of what is going on in the real world, and the challenges that real firms are facing," while the "practitioner should understand the difference between research and consulting and appreciate the value that both can offer" (Bidwell *et al.*, 2016, pp. 18–19). While such acknowledgements by our professional societies are a significant step in the right direction, there remains a widely held misperception that the field is simply facing a knowledge translation and transfer problem. We maintain, however, that the primary source of the theory–practice gap is due to a much more serious *knowledge production problem* (Huff, 2000; Rousseau, 2006; Van de Ven, 2007).

*Incentive Issues with Accreditation Organizations*

Incentive problems preventing more impactful research also exist with our accreditation bodies (e.g., AACSB), which set curriculum guidelines, and quality assessment of college/university faculty and administration through numerous proxy measures. For example, suggested curriculum guidelines in terms of the number, type, topic, and scope of courses may serve to reinforce the type of instructors hired, their knowledge bases and/or approaches to research and teaching, as well as how doctoral students are trained, selected, and incentivized to learn and develop their knowledge and skills in a similar fashion. Further, rigid reliance on structured courses and content likely reinforces the reproduction of old knowledge rather than the production and dissemination of new and more impactful knowledge. Another accreditation-related incentive problem that may dampen engaged scholarship occurs in the

AACSB's assessment of faculty research productivity through a proxy of the average number of publications per faculty member in a set of approved and recognized journals over a five-year period, and individual faculty members maintaining a minimum level of publication output to remain qualified as a "scholarly academic."

Finally, accreditation-related incentive problems may have unintended negative consequences and/or encourage quasi-unethical behavior in business school hiring, research support, and administrative practices. Such consequences and behavior may include bifurcating faculties to increase specialization in teaching (quality) or research (productivity) in the attempt to improve a school's accreditation assessment metrics. Such increasing specialization also influences reinforcement in university hiring processes, whereby preferences may be made to hire *only* established highly productive research scholars (adept at publishing high quantities of un-impactful research in the assessed journals) or high quality teaching scholars (adept at teaching numerous course preps, multiple and/or large course sections, while maintaining high teaching evaluations). Accreditation-reinforced incentives for faculty bifurcation would likely result in a substantially diminished labor market for research faculty who are viewed by the accreditation metrics as "less productive" (a category which would likely include faculty pursuing higher quality/more impactful, but lower publication quantity research topics). The likely results of the accreditation incentives are for new and younger scholars to: attempt to publish a high "quantity" of research (i.e., reinforcing the aforementioned problems with un-impactful research); "give up" on research and pursue a lower paid and less academically prestigious (despite its importance) teaching career; or leave academia altogether to pursue higher paid consulting or industry work. Such outcomes of these incentive problems are problematic to various degrees for the university, our field, and society.

One potentially optimistic note is that the AACSB has expanded its previous bi-modal faculty classification scheme of either academically qualified or professional qualified to a four-category classification (Scholarly Academics, Practice Academics, Scholarly Practitioners, and Instructional Practitioners). These new classifications allow Ph.D. qualified faculty to alternatively be classified as either "scholarly" or "practice" academics and for non-Ph.D. qualified faculty to be classified as either "scholarly" or "instructional" practitioners (AACSB, 2017). Such classifications, if widely adopted by business schools, could result in opportunities for the production of more impactful research (e.g., through collaboration between scholarly academics interested in professional practice and/or professionally qualified practitioner faculty interested in academic scholarly research). Collectively, to address these structural impediments to impactful research, we must continue to improve incentive issues with accreditation organizations to encourage both the research and practice sides increase crossover and collaboration to work to reduce the theory-practice gap. The AACSB's latest emphasis on the need for graduates "to be fluent in the practice of theory in a less-than-ideal context" is an optimistic sign of a step in this direction (Gellman, 2016, p. B5).

## Research-Side Issues: Further Suggested Solutions

So what types of steps might we collectively pursue to find our way back to a problem-focused, engaged, and pragmatic scholarly discovery process more capable of producing impactful research that is rigorous, relevant, and narrows the theory-practice gap? How are we in the strategic management field going to encourage such a process? What are some executable algorithms, which provide managerial heuristics (Simon, 1982)? In this section, we offer suggested solutions to the issues and impediments we have raised in the paper. We recognize that institutional norms and organizational inertia undermine adaptive responses to the environment (Chandler, 1962; Hannan and Freeman, 1977). Thus, it is unlikely that some of the suggestions we offer are remediable in the short-run, but perhaps they will be when environmental pressures for change become great enough. Such environmental pressures may include even less public support for higher education, increasing pressure from knowledge production alternatives such as consulting companies and corporate university models, and greater competition from education alternatives such as *Coursera* (Whitaker *et al.*, 2015).

### *Further Suggested Solutions for Doctoral Training and Faculty Recruitment Issues*

To help find our way back to a problem-focused, engaged, and pragmatic scholarly discovery process to narrow the theory-practice gap, the incentive alignment and collective action problems in doctoral training and faculty recruitment processes are perhaps the most pressing issues in need of solutions. Some further suggestions in these areas include:

- Include philosophy of science courses in all doctoral training, covering the concepts and the ethics of the scientific method, cause and effect relationships, and the nature of explanatory and predictive theory.
- Use as part of the teaching curriculum a course led by tenured faculty that directs real-world interactions of student teams to encourage engaged scholarship processes.
  - For example, offer hands-on real-world experiences through team project work as part of entrepreneurship and "capstone" strategic

management coursework, in the undergraduate and MBA level curriculum.

    - Extend such real-world problem-formulating and problem-solving opportunities as a component of the Ph.D. curriculum in bridging the theory–practice gap.

- In relation to the suggestion above for the inclusion of real-world interaction opportunities as part of doctoral training, business schools and their faculty could pursue opportunities to make ties to viable consulting firms concentrating on strategy work. This pursuit may include outreach to a school's MBA alumni working with major firms, as well as building ties with the managing partners of the offices in close geographic proximity to the school. The simple act of inviting a partner from a firm's local office to lead a Ph.D. seminar session or present in a departmental workshop, would be a start towards bridging the theory–practice gap and building the skills of doctoral students to pursue research that is more impactful.

- Doctoral training needs to be more interdisciplinary in scope to address real-world problems. As such, business schools and their departments would take a balanced portfolio approach to faculty recruiting. This hiring approach could involve recruiting some entry-level assistant professors who are well rounded and show evidence of knowledge beyond their area of specialization, which enables connection with real-world managerial problems.

*Further Suggested Solutions for Faculty Promotion and Tenure Process Issues*

Beyond solutions to doctoral training and faculty recruitment-process issues, the incentive alignment and collective action problems in faculty promotion, and tenure processes are perhaps the most pressing issues in need of solutions. Current practices are reminiscent of the ideas presented by Steven Kerr some years ago, but still as relevant today: "On the folly of rewarding A, while hoping for B" (Kerr, 1995). Some further suggestions include:

- Weigh more heavily research *quality* and deemphasize singular focuses on research quantity in hiring, promotion, and tenure processes. Simply put, "paper counts" neither measure engaged scholarship nor reflect impactful research. Expert evaluation by both resident and outside experts is required to assess research quality.

- Increase faculty–practitioner interactions by seeking practice help through successful, proven alumni willing to engage with faculty (and students) to collaborate in research discussions and/or to evaluate faculty contributions to managerial practice.

- Establish faculty internships within a business (Oviatt and Miller, 1989) in which "scholars-in-industry" programs provide opportunities for faculty to use sabbaticals and summer breaks.
- Encourage and incentivize faculty to undertake executive education that advances professional managers as students. Doing so would not only improve executive education and executive MBA teaching capabilities of the faculty and the school, but these two-way interactions between faculty and practitioners would help promote more engaged scholarship and impactful research.

*Further Suggested Solutions for Academic Journals, Societies, and Accreditation Organizations*

We also offer a few further suggested solutions for the incentive alignment and collective action problems in our academic journals, societies, and accreditation organizations. Some further suggestions include:

- Institute in at least some of our premier management journals that one of its (three) reviewers are from the practice of the field. This change in the journal review process would better evaluate if problems posed in research papers were grounded in reality.
- Encourage efforts by our academic societies to re-engage the nascent and declining "B" and "C" practitioner membership by involving more executives and consulting companies in conference programming and participation in sessions focused on real-world practice issues.
- Expect faculty participation in the infrastructure of the strategic management field, such as refereeing, membership in professional society structures whose goals are advancing the field, as well as in the development of on-going criteria for improving the field, and the accreditation standards for the business school.

## Conclusions

We submit that the management field's drift from a real-world problem-focused, engaged and pragmatic scholarly discovery logic substantially challenges our collective capability to develop new and impactful theory, and has led to a loss of relevance and value that threatens our institutional legitimacy and our managerial impact. Moving forward both *theoretically* and *methodologically*, one promising path of engaged scholarship for bridging the theory-practice gap and developing new theory that is rigorous and relevant is the aforementioned problem-finding and problem-solving approach (Baer *et al.*, 2013; Nickerson and Zenger, 2004). This approach suggests that scholars should focus first on finding, framing, and formulating problems, and second, on organizing knowledge sets to determine valuable solutions to these problems, which can be efficiently implemented to create and capture value (Felin and Zenger, 2016; Nickerson *et al.*, 2012). Thus, pragmatically reconnecting to problems originating in practice by using the logic of discovery, the problem-formulating and problem-solving perspective in particular, and engaged scholarship more generally, is likely to be a more fruitful path for generating relevant questions and developing rigorous theory, which will help bridge the theory-practice gap in the strategic management field.

Our objective in this paper was to provide a call for a refocusing of strategic management research on the logic of discovery of real-world problems for refining existing and developing new theory that is both scientifically rigorous and practically relevant. We maintained that the field would more likely achieve this refocus on the logic of discovery by joining a problem-formulation and problem-solving approach, engaged scholarship, and a philosophy of pragmatism. We provided several exemplars of how such real world problem-focused, engaged and pragmatic scholarly discovery processes produced many of the seminal works and core theories in the field. Further, these processes continue to likely underpin some of the most impactful more recent strategy research. (Re-)adopting such processes offers perhaps the most effective path forward for addressing many of the core challenges to the management field. This path can result in more engaged and impactful research that identifies and addresses real-world problems and creates economic value for business and society. Such future research, if scientifically rigorous and practically relevant, can both bridge the theory-practice gap and lead to the development of new theoretical contributions that revitalize the management field and advance the evolving science of organizations. Finally, we outlined structural and incentive impediments that need to be overcome, and we offered some suggestions for management scholars to operate more for the collective good of their school, the field, and society. We can do much better as we move forward in the field of strategic management.

## References

AACSB. 2017. "Eligibility Procedures and Accreditation Standards for Business Accreditation". AACSB International — The Association to Advance Collegiate Schools of Business. Adapted: April 8, 2013, Updated September 22, 2017. Available at: https://www.aacsb.edu/-/media/aacsb/docs/accreditation/standards/business-accreditation-2017-update.ashx?la=en (accessed August 9, 2018).

Ackoff, R. I. 1970. *A Concept of Corporate Planning*. New York: John Wiley & Sons.

Adler, N. J. and A. W. Harzing. 2009. "When Knowledge Wins: Transcending the Sense and Nonsense of Academic Rankings". *Academy of Management Learning & Education*. 8: 72–95.

Allison, G. 1971. *Essence of Decision: Explaining the Cuban Missile Crisis*. Boston, MA: Little, Brown.

Alvesson, M. and J. Sandberg. 2013. "Has Management Studies Lost its Way? Ideas for More Imaginative and Innovative Research". *Journal of Management Studies*. 50: 128–152.

Andrews, K. 1971. *The Concept of Corporate Strategy*. Homewood, IL: Richard D. Irwin.

Ansoff, H. I. 1965. *Corporate Strategy: An Analytical Approach to Business Policy for Growth and Expansion*. New York: McGraw-Hill.

Aram, J. D. and P. F. Salipante. 2003. "Bridging Scholarship in Management: Epistemological Reflections". *British Journal of Management*. 14: 189–205.

Argyris, C. and D. A. Schön. 1978. *Organizational Learning: A Theory of Action Perspective*. Reading, MA: Addison-Wesley.

Augier, M. and J. G. March. 2011. *The Roots, Rituals, and Rhetorics of Change: North American Business Schools After the Second World War*. Stanford, CA: Stanford University Press.

Bacharach, S. 1989. "Organizational Theories: Some Criteria for Evaluation". *Academy of Management Review*. 14: 496–515.

Baer, M., K. T. Dirks, and J. A. Nickerson. 2013. "Microfoundations of Strategic Problem Formulation". *Strategic Management Journal*. 34: 197–214.

Balogun, J., A. S. Huff, and P. Johnson. 2003. "Three Responses to the Methodological Challenges of Studying Strategizing". *Journal of Management Studies*. 40: 197–224.

Bansal, P., S. Bertels, T. Ewart, P. MacConnachie, and J. O'Brien. 2012. "Bridging the Research-Practice Gap". *Academy of Management Perspectives*. 26: 73–92.

Barnard, C. I. 1938. *The Functions of the Executive*. Cambridge, MA: Harvard University Press.

Barr, P. S., J. L. Stimpert, and A. S. Huff. 1992. "Cognitive Change, Strategic Action, and Organizational Renewal". *Strategic Management Journal*. 13(Summer): 15–36.

Bartunek, J. M. 2003. "A Dream for the Academy". *Academy of Management Review*. 28: 198–203.

Bartunek, J. M. and M. R. Louis. 1996. *Insider/Outsider Team Research*. Thousand Oaks, CA: Sage.

Bartunek, J. M. and S. L. Rynes. 2014. "Academics and Practitioners are Alike and Unalike: The Paradoxes of Academic-Practitioner Relationships". *Journal of Management*. 40: 1181–1201.

Bartunek, J. M., S. Rynes, and D. I. Ireland. 2006. "What Makes Management Research Interesting, and Why Does it Matter?" *Academy of Management Journal*. 49: 9–15.

Bennis, W. and J. O'Toole. 2005. "How Business Schools Lost Their Way". *Harvard Business Review*. 83(5): 96–106.

Bettis, R. A. 1991. "Strategic Management and the Straightjacket". *Organization Science*. 2: 315–319.

Bidwell, M., G. Di Stefano, and A. Gambardella. 2016. "Five-year (2011–2015) Division Review Report: Business Policy and Strategy Division". Internal report, *Academy of Management*.

Bingham, C. B., K. M. Eisenhardt, and N. R. Furr. 2007. "What Makes a Process a Capability? Heuristics, Strategy, and Effective Capture of Opportunities". *Strategic Entrepreneurship Journal*. 1: 27–47.

Bourgeois, L. J. 1984. "Strategic Management and Determinism". *Academy of Management Review*. 9: 586–596.

Bowman, E. H. 1990. "Strategy Changes: Possible Worlds and Actual Minds". In: *Perspectives on Strategic Management*. Ed. by J. Fredrickson. Cambridge, MA: Ballinger. 9–37.

Burgelman, R. A. and A. S. Groves. 2007. "Let Chaos Reign, Then Rein in Chaos — Repeatedly: Managing Strategic Dynamics for Corporate Longevity". *Strategic Management Journal*. 28: 965–979.

Byington, E. K. and W. Felps. 2017. "Solutions to the Credibility Crisis in Management Science". *Academy of Management Learning & Education*. 16: 142–162.

Carlile, P. R. 2004. "Transferring, Translating, and Transforming: An Integrative Framework for Managing Knowledge Across Boundaries". *Organization Science*. 15: 555–568.

Certo, S. T., D. G. Sirmon, and R. A. Brymer. 2010. "Competition and Scholarly Productivity in Management: Investigating Changes in Scholarship from 1988 to 2008". *Academy of Management Learning & Education*. 9: 591–606.

Chandler, A. D. 1962. *Strategy and Structure: Chapters in the History of the Industrial Enterprise*. Cambridge, MA: The MIT Press.

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

Chandler, A. D. 1990. *Scale and Scope: The Dynamics of Capitalism*. Cambridge, MA: Harvard University Press.

Cooper, A. C. and D. Schendel. 1976. "Strategic Responses to Technological Threats". *Business Horizons*. 19(1): 61–69.

Cyert, R. M. and J. G. March. 1963. *A Behavioral Theory of the Firm*. Englewood Cliffs, NJ: Prentice Hall.

Delbecq, A. L. and A. H. Van de Ven. 1971. "A Group Process Model for Problem Identification and Program Planning". *Journal of Applied Behavioral Science*. 7: 466–492.

Denzin, N. K. 1978. *The Research Act: A Theoretical Introduction to Sociological Methods*. New York: McGraw-Hill.

Dewey, J. 1938. *Logic: The Theory of Inquiry*. New York: Henry Holt and Company.

Diesing, P. 1971. *Patterns of Discovery in the Social Sciences*. Chicago: Aldine-Atherton.

Eisenhardt, K. M. and M. E. Graebner. 2007. "Theory Building from Cases: Opportunities and Challenges". *Academy of Management Journal*. 50: 25–32.

Evered, R. and M. R. Louis. 1981. "Alternative Perspectives in the Organizational Sciences: "Inquiry from the Inside" and "Inquiry from the Outside"". *Academy of Management Review*. 6: 385–395.

Felin, T. and T. R. Zenger. 2016. "Strategy, Problems, and a Theory for the Firm". *Organization Science*. 27: 222–231.

Gellman, L. 2016. "A New Push for Real-World Lessons at Business Schools". *The Wall Street Journal*. April 5: B5.

George, G. 2014. "Rethinking Management Scholarship". *Academy of Management Journal*. 57: 1–6.

Ghemawat, P. 2007. *Redefining Global Strategy*. Boston, MA: Harvard Business School Publishing.

Godfrey, P. C. and C. W. L. Hill. 1995. "The Problem of Unobservables in Strategic Management Research". *Strategic Management Journal*. 16: 519–533.

Gulati, R. 2007. "Tent Poles, Tribalism, and Boundary Spanning: The Rigor-Relevance Debate in Management Research". *Academy of Management Journal*. 50: 775–782.

Habermas, J. 1973. *Legitimation Crisis*. Tr. T. McCarthy. Boston, MA: Beacon Press.

Hambrick, D. C. 1994. "What if the Academy Actually Mattered?" *Academy of Management Review*. 19: 11–16.

Hambrick, D. C. 2007. "The Field of Management's Devotion to Theory: Too Much of a Good Thing". *Academy of Management Journal*. 50: 1346–1352.

Hannan, M. T. and J. Freeman. 1977. "The Population Ecology of Organizations". *American Journal of Sociology*. 82: 929–964.

Hanson, N. R. 1958. *Patterns of Discovery*. Cambridge, UK: Cambridge University Press.

Hax, A. C. and N. S. Majluf. 1984. *Strategic Management: An Integrative Perspective*. Englewood Cliffs, NJ: Prentice-Hall.

Henderson, B. D. 1984. *The Logic of Business Strategy*. Cambridge, MA: Ballinger Publishing Company.

Henderson, R. and I. Cockburn. 1994. "Exploring Firm Effects in Pharmaceutical Industry". *Strategic Management Journal*. 15(Winter): 63–84.

Hicks, J. R. 1976. "'Revolutions' in Economics". In: *Method and Appraisal in Economics*. Ed. by S. Latsis. Cambridge, UK: Cambridge University Press. 207–218.

Hitt, M. A. and C. R. Greer. 2012. "The Value of Research and Its Evaluation in Business Schools: Killing the Goose that Laid the Golden Egg?" *Journal of Management Inquiry*. 21: 236–240.

Hodgkinson, G. P. and D. M. Rousseau. 2009. "Bridging the Rigour–relevance Gap in Management Research: It's Already Happening!" *Journal of Management Studies*. 46: 534–546.

Hofer, C. W. and D. Schendel. 1978. *Strategy Formulation: Analytical Concepts*. St. Paul, MN: West Publishing Company.

Holt, C. C., F. Modigliani, J. F. Muth, and H. A. Simon. 1960. *Planning Production Inventories and Work Force*. Englewood Cliffs, NJ: Prentice-Hall.

Honig, B., J. Lampel, D. Siegel, and P. Drnevich. 2014. "Ethics in Management Research: Institutional Failure or Individual Fallibility?" *Journal of Management Studies*. 51: 118–142.

Huff, A. S. 2000. "Changes in Organizational Knowledge Production". *Academy of Management Review*. 25: 288–293.

Jick, T. D. 1979. "Mixing Qualitative and Quantitative Methods: Triangulation in Action". *Administrative Science Quarterly*. 24: 602–611.

Kaplan, A. 1961. *The New World of Philosophy*. New York: Vintage Books.

Kaplan, A. 1964. *The Conduct of Inquiry: Methodology for Behavioral Science*. San Francisco, CA: Chandler Publishing Company.

Kerlinger, F. N. C. 1964. *Foundations of Behavioral Research*. New York: Holt, Reinhart and Winston.

Kerr, S. 1995. "On the Folly of Rewarding A, While Hoping for B". *Academy of Management Executive*. 9: 7–14.

Ketokivi, M. and J. T. Mahoney. 2016. "Transaction Cost Economics as a Constructive Stakeholder Theory". *Academy of Management Learning & Education*. 15: 123–138.

Ketokivi, M. and S. Mantere. 2010. "Two Strategies for Inductive Reasoning in Organization Research". *Academy of Management Review*. 35: 315–333.

Khurana, R. 2007. *From Higher Aims to Hired Hands: The Social Transformation of American Business Schools and the Unfulfilled Promise of Management as a Profession*. Princeton, NJ: Princeton University Press.

Khurana, R. and J. C. Spender. 2012. "Herbert A. Simon on What Ails Business Schools: More Than 'A Problem in Organizational Design". *Journal of Management Studies*. 49: 619–639.

Kiechel, W. 2010. *The Lords of Strategy: The Secret History of the New Corporate World*. Boston, MA: Harvard Business School Publishing.

Kieser, A., A. Nicolai, and D. Seidl. 2015. "The Practical Relevance of Management Research: Turning the Debate on Relevance into a Rigorous Scientific Research Program". *Academy of Management Annals*. 9: 143–233.

Koontz, H. and C. O'Donnell. 1964. *Principles of Management*. New York: McGraw-Hill.

Kor, Y. Y. and J. T. Mahoney. 2000. "Penrose's Resource-based Approach: The Process and Product of Research Creativity". *Journal of Management Studies*. 37: 109–139.

Kor, Y. Y., J. T. Mahoney, E. Siemsen, and D. Tan. 2016. "Penrose's *The Theory of the Growth of the Firm*: An Exemplar of Engaged Scholarship". *Production and Operations Management*. 25: 1727–1744.

Ladd, G. W. 1987. *Imagination in Research*. Ames, IA: Iowa State University Press.

Lam, S. L. 2010. "What Kind of Assumptions Need to be Realistic and How to Test Them". *Strategic Management Journal*. 31: 679–687.

Lawrence, P. R. 1992. "The Challenge of Problem-Oriented Research". *Journal of Management Inquiry*. 1: 139–142.

Learned, E. P., C. R. Christensen, K. R. Andrews, and W. D. Guth. 1965. *Business Policy: Text and Cases*. Homewood, IL: Richard D. Irwin.

Leonard-Barton, D. 1992. "Core Capabilities and Core Rigidities: A Paradox in Managing New Product Development". *Strategic Management Journal*. 13(S1): 111–125.

Lewin, K. 1951. *Field Theory in Social Science: Selected Theoretical Papers*. New York: Harper.

Louis, M. R. and J. M. Bartunek. 1992. "Insider/outsider Research Teams: Collaboration Across Diverse Perspectives". *Journal of Management Inquiry*. 1: 101–110.

Lyles, M. A. 1981. "Formulating Strategic Problems: Empirical Analysis and Model Development". *Strategic Management Journal*. 2: 61–75.

Lyles, M. and I. Mitroff. 1980. "Organizational Problem Formulation: An Empirical Study". *Administrative Science Quarterly*. 25: 102–119.

Mahoney, J. T. 1993. "Strategic Management and Determinism: Sustaining the Conversation". *Journal of Management Studies*. 30: 173–191.

Mahoney, J. T. 2005. *Economic Foundations of Strategy*. Thousand Oaks, CA: Sage Publications.

Mahoney, J. T. and A. M. McGahan. 2007. "The Field of Strategic Management within the Evolving Science of Strategic Organization". *Strategic Organization*. 5: 79–99.

Mahoney, J. T. and R. Sanchez. 2004. "Building New Management Theory by Integrating Processes and Products of Thought". *Journal of Management Inquiry*. 13: 34–47.

Maritan, C. A. 2001. "Capital Investment as Investing in Organizational Capabilities: An Empirically Grounded Process Model". *Academy of Management Journal*. 44: 513–531.

Markides, C. C. 2007. "In Search of Ambidextrous Professors". *Academy of Management Journal*. 50: 762–768.

Martin, B. R. 2016. "Editors' JIF-boosting Stratagems — Which are Appropriate and Which Not?" *Research Policy*. 45: 1–7.

McCloskey, D. 1985. *The Rhetoric of Economics*. Madison, WI: Wisconsin University Press.

McGahan, A. M. 2007. "Academic Research that Matters to Managers: On Zebras, Dogs, Lemmings, Hammers, and Turnips". *Academy of Management Journal*. 50: 748–753.

McGrath, R. G. 2007. "No Longer a Stepchild: How the Management Field can Come into its Own". *Academy of Management Journal*. 50: 1365–1378.

Miller, K. D. and E. W. K. Tsang. 2011. "Testing Management Theories: Critical Realist Philosophy and Research Methods". *Strategic Management Journal*. 32: 139–158.

Mintzberg, H. 1994. *The Rise and Fall of Strategic Planning*. New York: The Free Press.

Mintzberg, H., D. Raisinghani, and A. Theoret. 1976. "The Structure of 'Unstructured' Decision Processes". *Administrative Science Quarterly*. 21: 246–275.

Mitroff, I. I. and T. R. Featheringham. 1974. "On Systematic Problem Solving and the Error of the Third Kind". *Behavioral Science*. 19: 383–393.

Nelson, R. R. and S. G. Winter. 1982. *An Evolutionary Theory of Economic Change*. Cambridge, MA: Cambridge University Press.

Newman, W. H. and J. P. Logan. 1971. *Strategy, Policy, and Central Management*. Cincinnati, OH: South-Western Publishing.

Nickerson, J. A., C. J. Yen, and J. T. Mahoney. 2012. "Exploring the Problem-finding and Problem-solving Approach for Designing Organizations". *Academy of Management Perspectives*. 26: 52–72.

Nickerson, J. and T. Zenger. 2004. "A Knowledge-based Theory of Governance Choice: The Problem Solving Approach". *Organization Science*. 15: 617–632.

Nonaka, I. and H. Takeuchi. 1995. *The Knowledge-creating Company*. New York: Oxford University Press.

Olson, M. 1965. *The Logic of Collective Action*. Cambridge, MA: Harvard University Press.

Oviatt, B. M. and W. D. Miller. 1989. "Irrelevance, Intransigence, and Business Professors". *Academy of Management Executive*. 3: 304–312.

Peirce, C. A. 1940. *The Philosophy of Pierce: Selected Writings*. New York: Harcourt Brace.

Penrose, E. T. 1959. *The Theory of the Growth of the Firm*. New York: John Wiley & Sons.

Penrose, E. T. 1960. "The Theory of the Growth of the Firm. A Case Study: The Hercules Powder Company". *Business History Review*. 34: 1–23.

Pettigrew, A. M. 1973. *The Politics of Organizational Decision-Making*. London, UK: Tavistock.

Pettigrew, A. M. 2001. "Management Research after Modernism". *British Journal of Management*. 12(Special Issue): S61–S70.

Pfeffer, J. and C. Fong. 2002. "The End of Business Schools? Less Success than Meets the Eye". *Academy of Management Learning & Education*. 1: 78–95.

Popper, K. 1963. *Conjectures and Refutations: The Growth of Scientific Knowledge*. London: Routledge & Kegan Paul.

Porter, M. 1996. "What is Strategy?" *Harvard Business Review*. 74(6): 61–80.

Quinn, J. B. 1980. *Strategies for Change: Logical Incrementalism*. Homewood, IL: Irwin.

Reichenbach, H. 1938. *Experience and Prediction*. Chicago, IL: University of Chicago Press.

Reuer, J. J. and M. J. Leiblein. 2000. "Downside Risk Implications of Multinationality and International Joint Ventures". *Academy of Management Journal*. 43: 203–214.

Richardson, G. B. 1964. "The Limits to a Firm's Rate of Growth". *Oxford Economic Papers*. 16: 9–23.

Romme, A. G. L. 2003. "Making a Difference: Organization as Design". *Organization Science*. 14: 558–573.

Rorty, R. 1979. *Philosophy and the Mirror of Nature*. Princeton, NJ: Princeton University Press.

Rousseau, D. M. 2006. "Is There Such a Thing as "Evidence-based Management"?" *Academy of Management Review*. 31: 256–269.

Rousseau, D. M. 2012. "Designing a Better Business School: Channeling Herbert Simon, Addressing the Critics, and Developing Actionable Knowledge for Professionalizing Managers". *Journal of Management Studies*. 49: 600–618.

Rumelt, R. P., D. E. Schendel, and D. J. Teece. 1994. *Fundamental Issues in Strategy: A Research Agenda*. Boston, MA: Harvard Business School Press.

Rynes, S. L., J. M. Bartunek, and R. L. Daft. 2001. "Across the Great Divide: Knowledge Creation and Transfer Between Practitioners and Academics". *Academy of Management Journal*. 44: 340–355.

Schelling, T. C. 1960. *The Strategy of Conflict*. Cambridge, MA: Harvard University Press.

Schendel, D. and C. W. Hofer, eds. 1979. *Strategic Management: A New View of Business Policy and Planning*. Boston, MA: Little, Brown, and Company.

Schön, D. A. 1983. *The Reflective Practitioner: How Professionals Think in Action*. New York: Basic Books.

Schwab, A. and W. H. Starbuck. 2016. “Collegial “Nests” can Foster Critical Thinking, Innovative Ideas, and Scientific Progressive”. *Strategic Organization*. 14: 167–177.

Selznick, P. 1957. *Leadership in Administration*. Berkeley, CA: University of California Press.

Semadeni, M. 2006. “Minding Your Distance: How Management-Consulting Firms Use Service Marks to Position Competitively”. *Strategic Management Journal*. 27: 169–187.

Shah, S. K. 2006. “Motivation, Governance, and the Viability of Hybrid Forms in Open Source Software Development”. *Management Science*. 52: 1000–1014.

Shapiro, D., B. Kirkman, and H. Courtney. 2007. “Perceived Causes and Solutions of the Translation Problem in Management Research”. *Academy of Management Journal*. 50: 249–266.

Shrivastava, P. and J. H. Grant. 1985. “Empirically Derived Models of Strategic Decision-making Processes”. *Strategic Management Journal*. 6: 97–113.

Simon, H. A. 1947. *Administrative Behavior*. New York: Macmillan.

Simon, H. A. 1957. *Models of Man: Social and Rational*. New York: John Wiley & Sons.

Simon, H. A. 1967. “The Business School: A Problem in Organizational Design”. *Journal of Management Studies*. 4: 1–16.

Simon, H. A. 1973. “The Structure of Ill-structured Problems”. *Artificial Intelligence*. 4: 181–201.

Simon, H. A. 1982. *Models of Bounded Rationality: Behavioral Economics, and Business Organization*. Cambridge, MA: MIT Press.

Simon, H. A. 1996. *Sciences of the Artificial*. Cambridge, MA: MIT Press.

Smith, G. 1951. *Policy Formulation and Administration: A Casebook of Top Management Problems in Business*. Chicago, IL: Richard D. Irwin.

Stokes, D. E. 1997. *Pasteur’s Quadrant: Basic Science and Technological Innovation*. Washington, DC: Brookings Institution Press.

Summer, C. E., R. A. Bettis, I. H. Duhaime, J. H. Grant, D. C. Hambrick, C. C. Snow, and C. P. Zeithaml. 1990. “Doctoral Education in the Field of Business Policy and Strategy”. *Journal of Management*. 16: 361–398.

Szulanski, G. 1996. “Exploring Internal Stickiness: Impediments to the Transfer of Best Practice within the Firm”. *Strategic Management Journal*. 17(Winter): 27–43.

Tranfield, D., D. Denyer, and P. Smart. 2003. “Towards a Methodology for Developing Evidence-informed Management Knowledge by Means of Systematic Review”. *British Journal of Management*. 14: 207–222.

Tripsas, M. and G. Gavetti. 2000. “Capabilities, Cognition, and Inertia: Evidence from Digital Imaging”. *Strategic Management Journal*. 21: 1147–1161.

Tukey, J. W. 1962. “The Future of Data Analysis”. *Annals of Mathematical Statistics*. 33: 1–67.

Tushman, M. and C. O’Reilly. 2007. “Research and Relevance: The Implications of Pasteur’s Quadrant for Doctoral Programs and Faculty Development”. *Academy of Management Journal*. 50: 769–774.

Van Aken, J. E. 2005. “Management Research as a Design Science: Articulating the Research Products of Mode 2 Knowledge Production in Management”. *British Journal of Management*. 16: 19–36.

Van de Ven, A. H. 2007. *Engaged Scholarship: A Guide for Organizational and Social Research*. Oxford, UK: Oxford University Press.

Van de Ven, A. and P. Johnson. 2006. “Knowledge for Theory and Practice”. *Academy of Management Review*. 31: 802–821.

Vermeulen, F. 2005. “On Rigor and Relevance: Fostering Dialectic Progress in Management Research”. *Academy of Management Journal*. 48: 978–982.

Vermeulen, F. 2007. ““I Shall Not Remain Insignificant:” Adding a Second Loop to Matter More”. *Academy of Management Journal*. 50: 754–761.

Volkema, R. J. 1983. “Problem Formulation in Planning and Design”. *Management Science*. 29: 639–652.

Volkema, R. J. 1986. “Problem Formulation as a Purposive Activity”. *Strategic Management Journal*. 7: 267–279.

Weick, K. D. 1989. “Theory Construction as Disciplined Imagination”. *Academy of Management Review*. 14: 516–531.

Whetten, D. A. 1989. “What Constitutes a Theoretical Contribution”. *Academy of Management Review*. 14: 490–495.

Whitaker, J., R. New, and D. Ireland. 2015. “MOOCs and the Online Delivery of Business Education: What’s New? What’s Not? What Now?” *Academy of Management Learning & Education*. 15: 345–365.

Williamson, O. E. 1975. *Markets and Hierarchies: Analysis and Antitrust Implications*. New York: The Free Press.

Williamson, O. E. 1986. *Economic Organization: Firms, Markets and Policy Control*. New York: New York University Press.

Wilson, C. H. 1954. *History of Unilever*. London: Cassell.