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EDITORIAL

Strategic management: Building depth as well as breadth

The field of strategic management has come a long way in a very short space of time. In the late 1970s, our best known theory was the ubiquitous SWOT analysis and while the work of authors such as Penrose (1959), Cyert and March (1963), Chandler (1962) and Selznick (1957) was recognized by some in the field, they did not feature heavily in what was largely a teaching inspired field that was designed to be integrative in nature and teach traditional capstone style material. In 1979, the field was rechristened as strategic management and Porter's five forces model was first published. A year later, the *Strategic Management Journal* was first published. Within 5 years, strategic management scholars were able to achieve tenure for the first time in North American universities beyond Harvard (Hambrick, 2004) and the top management journals quickly began to accept and publish papers that clearly fell within the domain of strategic management.

As the most immature of the major management disciplines, strategic management has both benefited and suffered from the exponential increase in research that has been occurring since the 1980s. On the positive side, the field has developed very quickly and has coalesced around a (wide-ranging) set of foundational theories. It has benefited from the collective learning that has been possible through observing the development of other related fields and, as very few of the senior scholars in the 1980s and 1990s were specifically trained in the discipline of strategic management, the field has been well positioned to adopt a variety of methodologies and theoretical underpinnings from which to further develop ideas.

However, many of these same drivers have created potentially negative outcomes. For example, the breadth of scholars in the field has led to an extremely fragmented research agenda that is seemingly moving ever-further away from an accepted central theoretical paradigm. The massive growth in research has limited the extent to which experienced business professionals can transition into the field with many suggesting we are moving further away from managerial relevance, and the drive for more (and faster) published research has decreased the extent to which we as scholars are willing to engage in longitudinal research or even collect primary data. This has occurred in the context where competition between journals has resulted in the need for each and every paper to make a theoretical contribution and this has limited the extent to which we are able to consolidate our gains via very detailed studies, including replication studies, of particular phenomena or theory. Overall, there has been something of a theoretical and methodological 'free for all.' Nevertheless, strategic management scholars find themselves in an enviable position. In the past three and a half decades, the field has grown to become one of the largest sub-disciplines of management, its future is assured via its central role in meeting AACSB, EQUIS and AMBA accreditation requirements for generalist undergraduate and graduate business degrees, and it can now look at a range of well-established field-specific journals through which to disseminate its research. As the field is in such a strong position, now may well be a time to take stock and build upon the gains of recent decades.

THE PRESENT STATE OF PLAY

A quick look at textbooks in the field of strategic management would suggest that there is indeed a degree of uniformity in respect of what constitutes the accepted theoretical base of the field. Before 1979, the focus was centered upon what firms did, how strategies were enacted and recognized the human element in respect of the behaviors managers exuded. The widespread success of Porter's industrial organization economics inspired five forces model that fundamentally changed the dominant focus within the field to one where rationality was assumed (rather than political or other forms of non-rational behavior) and the focus of competitive advantage shifted to factors outside of the firm. To use Hoskisson, Hitt, Wan, and Yiu (1999) terminology, the theoretical pendulum swung from an internal focus to a focus on factors that were outside individual firm boundaries. Few strategy researchers today would argue that industry structure, or any set of factors outside of the boundaries of the firm could consistently explain competitive advantage in the majority of cases. It is thus unsurprising that the pendulum swung back toward a more internal focus in the early 1990s. However, this internal focus has splintered somewhat into a variety of theoretical approaches including the resource-based view, the knowledge-based view and dynamic capabilities.

These various approaches to understanding competitive advantage may be well recognized (if not always entirely accepted) by researchers in the field. However, research that tests, extends or otherwise directly addresses these theories tend to form a distinct minority of the strategy research presently being published in journals around the world. Industrial organization economics inspired research is tending toward building a better understanding of competitive dynamics, and other theory bases here such as game theory are increasingly being drawn upon within this strand of research. Within the internally focussed perspective, there is a lack of clarity concerning the relationship between resource-based view and dynamic capabilities – as addressed by Galvin, Rice, and Liao (2014), and even the area of dynamic capabilities seems to be developing into alternate 'clubs' (Peteraf, Di Stefano, & Verona, 2013) while the resource-based view has been subjected to numerous criticisms (e.g., Priem & Butler, 2001). Other approaches also focus on this internal dimension including the knowledge-based view, learning models, the competence-based framework and possibly some of the work concerning collaborative advantage.

Understanding how strategy is developed and enacted has for too long been ignored as economic inspired frameworks and methodologies have dominated the field, but this area is now making considerable headway under the 'strategy as practice' banner. The role that individuals play is also recognized in much of the work concerning top management teams, leadership and aspects of decision making. Corporate strategy, especially the interest in the boundaries of the firm and the efficiencies that can derive from appropriate boundary choices tends to rely upon transaction cost economics or capability approaches. The growth of alliances has seen a significant body of work grow around this topic including collaborative strategies/advantage. This is by no means an exclusive list and we are seeing advances in the field emerging using institutional theory, agency theory, organization theory (particularly in respect of firm and industry architectures), learning, entrepreneurship, cognitive modelling and increasingly a range of ideas from organization behavior.

There is little doubt that each of these theoretical positions is capable of adding to our understanding of strategic management. Some theories are going to work best in understanding particular phenomena and these need to be developed further to better understand the context in which they work, the limits of the theory and how it may integrate with other theories to give managers a richer perspective of strategic management. Part of this may require some comparative studies that seem to have been almost systematically ignored since a series of papers compared the role of industrial organization-related principles and resource-based view in explaining firm performance (e.g., Rumelt, 1991; Powell, 1996; McGahan & Porter, 1997, etc.). This appreciation of context is something that the *Journal of Management & Organization* has chosen to focus upon as understanding where the boundaries lie for the applicability of a theory, those contexts for which we may need to develop new theory, or adapt what we have, or how different contexts may provide new moderating or mediating variables in accepted 'universal' relationships. For more details see the Editorial by Galvin (2014).

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It is in this spirit of consolidating some of the gains that the strategic management discipline has achieved in the last 3 decades, this paper highlights some potential avenues for research that may help the field move toward more depth rather than ever-expanding breadth. The field is unlikely to suddenly move toward replication studies or cases where there are very incremental developments that allow for sensitivity analysis concerning phenomena, but some incremental research that addresses important contextual questions concerning particular theoretical approaches as well as developing links between different theories may well prove to be a useful starting point for some of this consolidation. It is with this agenda in mind that we suggest a variety of areas that may provide opportunities for a significant contribution to be made to the field of strategic management as per this consolidation objective.

NOT EVERY ORGANIZATION IS PROFIT-SEEKING

A quick review of strategy articles in this Journal or any other generalist management journal or specialist strategy journal will reveal that an extremely small percentage of the research addresses strategy within a public sector or even not-for-profit context. One of the fundamental reasons for this is that many of our strategy concepts center on the notion of competitive strategy and a significant portion of the remaining research considers a range of corporate strategy concepts. But most public-sector organizations do not have competitors, and the boundaries of their operations at a macro level tend to be determined by government policy rather than giving senior management the opportunity to move into or out of certain activities.

However, the public sector is increasingly looking toward strategy research and our frameworks for insight concerning their own strategic direction. Certainly, the move toward 'New Public Management' with its focus upon users as clients (i.e., schools, hospitals and immigration departments now have clients) has ensured that many of the ideas concerning value creation are transitioning into the public sector. However, the majority of our central frameworks do not transition quite so neatly (e.g., Vining's, 2011 attempt to create an adapted five forces framework for the public sector). Thus, the public sector is implementing many of the ideas and practices that emerge from our research, but they have not been developed for, or necessarily even tested, in a public-sector context. There are obvious differences between public and private-sector firms, and yet even though public-sector activity constitutes a significant part of most economies' gross domestic product, the strategy research that pertains specifically to them is virtually non-existent in the mainstream (i.e., non-public sector focussed) journals.

But against this background, public-sector organizations do develop strategy. They need to allocate resources, create value for their 'clients' and in most cases 'do more with less' each and every year. They talk publically about building capabilities, engage constantly in activities that determine firm boundaries such as outsourcing, alliance contracting and public–private partnerships and they increasingly employ senior executives with private-sector backgrounds. Even if we do not develop public sector-specific frameworks, there would seem to be enormous opportunities in recognizing the private sector versus public sector operational environments as a major contextual variable in much of our research.

Extending this notion of the public sector as a potential contextual variable, there would seem to be significant value in appreciating that the institutional environment varies enormously between countries on the basis of government policy. Government interventions rarely create the free-market environments that are subject simply to the forces of competition, but which tend to be assumed to exist within many of our models. Rather, government policy (at all levels) creates significant distortions to the market that vary according to regions. Investments in some industries over others, restrictions on ownership according to 'the national interest,' other restrictive laws, government grants, minimum wage levels, differential tax treatments of different industries and the creation (or lack of support for) of certain infrastructure can both directly and indirectly create winners and losers before the firms themselves actively engage in the strategy process. For example, in Australia, the 'Four Pillars Policy' in the banking industry has made for one of the most concentrated banking industries in the world where there is very little competition at the retail level, but further consolidation is extremely unlikely owing to the restrictions that exist in respect of the four largest banks. Too often government policy is assumed to be a benign factor that sits in the background and is constant across an industry – even where the industry operates across national borders and is subject to significant government involvement such as the motor vehicle industry. Yet often government policy and interventions operate as a contextual variable that may significantly impact both the strategy development process (e.g., government bail-outs of large firms may create a 'too big to fail' mentality among senior managers that encourages risk-taking behavior) and the strategy itself (e.g., resources and capabilities that are enhanced or depleted significantly according to government policy or the institutional environment in which the competitive dynamics play out). Overall, strategic management as a field would do well to stop ignoring the public sector (and the not-for-profit sector) given the significant role it plays in the economy and the impact that it may have upon private-sector firms.

THE MICRO-FOUNDATIONS OF STRATEGY

In 2004, Hambrick encouraged strategy researchers to recognize that strategy is developed and implemented by individuals and therefore we must recognize the role of the human element. Given recent developments in this area, these micro-foundations of strategy are likely to become a cornerstone of the field of strategic management in the next years. Strategic management theory consists of macro theories such as the resource-based view, capability theory or institutional theory. Very little is known how the predictions of these theories emerge from the micro-level. In all, three components have been argued to be core to the debate how individual actions result into organizational outcomes (Felin, Foss, Heimeriks, & Madsen, 2012):

1. the individual,
2. (social) processes,
3. and routines.

Until now, the genes of organizations have been routines (Nelson & Winter, 2009) as an emergence of standard operating procedure that played an important role in the behavioral theory of the firm (Cyert & March, 1963; Arndt, Pierce, & Teece, 2014). Routines have been defined by Levitt & March (1988: 324) as 'forms, rules, procedures, conventions, strategies, and technologies around which organizations are constructed and around which they operate. It also includes the structure of beliefs, frameworks, codes, culture and knowledge buttress, elaborate and contradict the formal routine.' According to this definition, a routine is little more than the way an organization works. However, such a heuristic ignores heterogeneity within an organization, of individuals, processes among individuals and eventually the dynamics within routines (Feldman & Pentland, 2003). Human behavior is therefore experiencing something a resurrection in strategic management and has recently led to the establishment of a new interest group in the Strategic Management Society.

Two topics that increasingly grasp attention in the strategic management literature are closely related to the emerging stream around the micro-foundations of strategy. First, the insight that individuals are humans with cognitive limitations, emotions and social beliefs. Only recently have we start to understand the importance of cognition beyond bounded rationality (Helfat & Peteraf, 2014).

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Being organized and managed by humans is an inherent part of every organization and is inadequately addressed by coding everything that makes us humans a 'bias' (Teece, 2007; Foss & Lindenberg, 2013).

Second, the field of entrepreneurship has become more and more intertwined with strategic management with many leading scholars in each area drawing from (and publishing in) the each other's fields. This renewed focus upon entrepreneurship has led to the (re)emergence of two themes in strategy that would benefit from further impetus: diversification and incentives. Diversification principles have been established with constructs such as complementarity, synergy and strategic focus among others through the works of Teece (1980), Barney (1986) and Hamel and Prahalad (1990). Corporate venturing and entrepreneurial incentives revitalize this discussion by adding a new point of view. Mergers, acquisitions, divestments and spin-offs are not solely actions to reorient a firm's portfolio, but are indicators of how a firm aims to balance exploration and exploitation activities (March, 1991; Teece, Pisano, & Shuen, 1997). From this perspective, individuals with specific skills and the potential of high value creation are incentivized by becoming entrepreneurs (spin-off, consulting, start-ups) rather than employees that are bound to more traditional forms of organizing within a firm's hierarchy.

This growing focus upon human actions and the role of the individual as opposed to the economic rational models will have a number of implications upon our research. Three likely avenues for further consideration include more interdisciplinary research, the recognition of different cultures and the role of foresight.

As we look deeper into human behavior we will start to look more to psychology and potentially even further to neuroscience to help us make a closer connection to what happens in our brain when making different kinds of decisions (Laureiro-Martínez, Brusoni, Canessa, & Zollo, 2014). More micro-oriented business research such as that considering corporate psychopaths is already looking toward and utilizing the neuroscience that has been undertaken in the study of psychopathy. More work that bridges science, engineering and management is expected to emerge from newly founded schools such as Skolkovo and international think tanks that put a particular emphasis on interdisciplinary research.

Another area is concerned with the question how management in an increasingly globalized world can benefit from multiculturalism, different logics or different institutional settings (Fourne, Jansen, & Mom, 2014). While there is a recognition that there are different national cultures and this is often an explicit part of the research design in international business, such a recognition is not commonly incorporated in strategy research. This multiculturalism may in itself bring new strengths to organizations and this stream of work is currently led by Chen (2014) under the label of ambiculturalism. However, it goes much further than the duality between China and North America and may be more adequately captured by fusion theory (Ashkanasy, 2012; Arndt & Ashkanasy, 2014). The strategic importance of combining the strength of different cultures is currently a largely unexplored field.

The third aspect concerning the human dimension of strategy that we would like to highlight in this editorial is the notion of foresight. In his seminal paper, Fischhoff (1975) demonstrated how foresight can be conceptualized and suggest an experimental setting to measure it. In the strategy literature, Ahuja, Coff, and Lee (2005) uses asymmetric information as one mechanism of how early information leads to economic returns. Tetlock (2005) investigates the role of expertise for foreseeing future development. He demonstrates that expertise has little predictive power when looking at macro developments in political settings. Overall, we know very little about foresight, but think much of it when connecting it to inspiring visionaries such as Bill Gates, Jack Ma, Richard Brandson or Steve Jobs. Eisenhardt (1989) opened up new avenues of enquiry concerning decision making processes with her landmark publication but the field of foresight has not benefited from a similarly detailed and insightful study.

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METHODOLOGICAL ISSUES

Pragmatism may be an apt description for some of the research undertaken within the strategy field. The benefit of pragmatism is that it provides the scope to undertake research in such a way that it allows research questions to be addressed in the most appropriate fashion. Case studies, historical narratives, mixed method studies as well as traditional deductive studies utilizing quantitative data and inductive studies using qualitative data have all been used in strategic management research.

Quantitative strategy research has always been challenging as the unit of analysis tends to be the firm. Thus, if controlling for industry effects (i.e., considering just a single industry), it is probable that the sample size will be limited and potentially insufficient to researchers more used to the sample sizes found in fields where the individual tends to be the unit of analysis (e.g., human resource management or organizational psychology). With more complex multivariate statistical techniques becoming common place and the massive cost and/or time required to generate primary data with a significant number of firms sets (especially covering multiple years), it is becoming increasingly common to use secondary data sets and proxy measures for particular constructs. At one level, what we have gained from our ability to look more deeply at particular relationships we have lost via our inability to necessarily measure particular variables accurately. This may be compromising our ability to build and develop new and appropriate theory.

This challenge became obvious when undertaking a friendly review recently for a colleague (with numerous top-tier publications to their name) before they submitted a well-known top journal. To ensure their confidentiality, the variables shall remain nameless, but they used a proxy for one of their independent variables and another for a dependent variable. When looking at the actual measures, there was absolutely no logic as to why one should impact the other. When I questioned them about this fact, their response was that a very similar proxy had been used for the independent variable in one study and the dependent variable had also been measured in another study by a proxy similar to what they were using. When I asked that they take away the labels they were giving the proxies and explain to me the rationale for why these two variables (in their original form) may be in some way related, they could not. In fact they just kept repeating that these were proxies that had been established in the literature, even though in this case a relationship between the variables made no sense.

Some of the most insightful work in the field has come when researchers have been willing to 'get their hands dirty,' engage with industry and ask new questions. Our field is not sufficiently advanced to the point that we can refine our understanding of the relevant theory simply through more complex and more detailed models. In fact the opposite is true. We tend to treat an R^2 of anything >0.1 as an important result (and often publish material with far weaker relationships than this). Even with control variables, we end up with well over 80% of the variance in our dependent variable being unexplained. This is not a result that requires refinement, but rather an indication that we still have a lot more work to do to understand those factors that are instrumental in affecting firm performance or any other dependent variable we may be using. It is therefore the pragmatism of the earlier years of the field with its case studies, narratives, qualitative and quantitative studies along with theorizing, all remain legitimate options in the strategy field, as adopting the dominant research designs of alternative (more established fields) such as economics or organizational psychology is only likely to stunt the field in this period when so many of our fundamental questions remain at best, only partially answered.

STRATEGY AND ETHICS

The lead-author was, for a number of years, until late 2013 employed in the United Kingdom and since then has been working in Australia. In both countries, there have been recent government enquiries and investigative journalist pieces into the tax affairs of certain well known multinationals. It has become clear that many large firms today have devised their operations around tax minimization.

This reduced corporate tax income across numerous advanced economies has led these national governments to engage in significant cost-cutting across different areas including health, education and social services. Yet, these same firms that rely upon the national infrastructure (e.g., roads, ports, telecommunications, the legal system for IP protection) seek to employ skilled and semi-skilled staff that have been educated largely at a cost to the state and whose executives seek the lifestyle benefits of an advanced economy (such as healthcare, infrastructure for their local community, etc.) are often the ones that are paying virtually no tax to the government whatsoever.

While the area of corporate social responsibility and even ethics per se have become increasingly fashionable as research topics, they are invariably independent to our core strategic management theory. While most of our frameworks are silent on the role of ethics, extrapolating Porter's five forces framework the optimal situation for a firm to work toward is one where super-normal profits are able to be obtained through high levels of power of buyers and suppliers and limiting competition as part of achieving monopolistic rents. There is a call by some scholars to find models that go beyond the primary purpose of the profit-seeking firm and instead highlight sustainability or others factors as core to the philosophy of an organizations. As an example, how do we approach firms that pursue higher goals while simultaneously being faced with the challenge of having to make money to meet the needs of the owners? At this point, however, economists are still resistant to abandon the neoclassical model and the beauty that comes with it for explaining a firm's rationale.

It may be that the rational economist lens that has led us to ignore the role of the individual and the choices (ethical or otherwise) that they may make. However, in terms of the dominant theory of the time, to get an alternative perspective, we have to return to the work of Andrews and colleagues (Andrews, 1980; Learned *et al.*, 1965) where the importance of stakeholders was expressly recognized as opposed to a pure profit motive. Perhaps with the rise of the behavioral school and the recognition of the role of individual decision makers, we may once again have the opportunity to appreciate that there can be (and should be) an ethical dimension to our frameworks and that this is not something that can be conceptualized independently of strategic management and the strategy formulation process.

WHERE TO FROM HERE?

Journals like *Journal of Management & Organization* have a critical role to play in filling in some of these gaps in the theory as the field consolidates to a degree in the coming decade. Perhaps most importantly, the recognition of context as an important part of the puzzle will help the field build depth. There are opportunities to recognize that different cultures, different institutional environments, different firm objectives all fundamentally impact the strategy process. This, coupled with an increased focus on the human dimension potentially spells out part of an agenda that is likely to underpin further success for the field in the next decade.

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