



The future of strategic management research: Assessing the quality of theory borrowing

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ABSTRACT

In many business schools, the field of strategic management has been elevated to the same status as more traditional subject areas such as finance, marketing and organizational behaviour. However, the field is rather unclearly delineated at present, as a result of the heavy usage of borrowed theories, a phenomenon we discuss in this article. For strategic management to become a legitimate subject area, truly at par with the more conventional fields taught in business schools, we recommend much stronger selectivity when borrowing theories from other areas of scholarly inquiry than management, as the foundation of empirical work. We propose a new model consisting of *seven quality tests* to assess whether proper selectivity is being applied when ‘importing’ concepts from other fields than management. Our perspective has major implications both for future, evidence-based strategic management research and for the field’s key stakeholders such as strategy teachers, practitioners and policy makers – who rely on research outputs from strategy scholars.

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Introduction

Modern management research is struggling to combine effectively academic rigour and practitioner relevance. Chia (2014, p. 684) has argued that a shift is necessary towards common sense scholarship so as to serve well the two above goals. Along the same lines, Hernes (2014, p. 852) has suggested that scholars must cut loose the shackles of misplaced scientific ideals and should instead focus on, “...the localized, embedded, fluid and contingent nature of managerial work.”

In the present article, we propose an actionable approach that should improve both the rigour and relevance of a particular organization and management subfield, namely that of strategic management (SM), whereby we focus on the development and usage of appropriate theory to guide research efforts. It has been suggested that: “There is nothing so practical as a good theory” (Lewin, 1951, p. 169). Van de Ven (1989, p. 486) extended this perspective as follows: “Good theory is practical precisely because it advances knowledge in a scientific discipline, guides research towards crucial questions, and enlightens the profession of management.”

However, developing theory that is helpful to management is not necessarily easy to achieve, and this is for at least two reasons. *First*,

what actually constitutes rigorous and relevant SM theory is debatable, but presumably it must have some type of predictive and explanatory capacity that would support a broad area of practice. A theory’s predictive and explanatory power should hold across a range of empirical phenomena in this area of practice, but such generalizability may be difficult to establish. Indeed, knowledge creation efforts in SM seldom lead to generally accepted, cumulative knowledge development, in contrast to what occurs in the hard sciences. Controlled experiments, accurately simulating/replicating situational contexts from practice are often difficult – if not impossible – to achieve, especially when addressing ‘wicked’ management issues in the SM sphere (see below). *Second*, irrespective of a high level of generalizability and cumulative knowledge development, SM theory can inform practice, but the transfer of concepts and predictive templates from an academic setting to a managerial environment is not necessarily easy to achieve. SM scholars may develop substantial insights on real world, managerial phenomena, but few are the equivalent of, e.g., medical experts, who combine academic research and clinical practice, whereby issues arising in the latter area of work translate directly into identifying critical questions and formulating testable hypotheses in the former. In addition, few SM research outputs are read and used systematically by strategy practitioners. Only a fraction of SM research thus combines a strong predictive and explanatory capacity, and usefulness as perceived by practitioners, because of the absence of systematized back-and-forth between research and practice.

The scientific discipline of SM addresses the establishment and pursuit of a firm’s long-term goals, as well as the ways in which

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business leaders or managers respond to – and shape – environmental forces, and orchestrate internal resources. Here, SM theorizing can be important, to the extent that it allows for better prediction and explanation of SM choices in clearly specified situational contexts. As a result, the SM practice becomes more ‘intellectualized’: it can be understood not only by being actually engaged in it (‘learning by doing’), but also through the mediating and framing force of conceptual ideas (Kiechel, 2010).

SM theorizing can be important not only for purposes of immediate applicability in management practice, but also for business school teaching because it transcends all functional areas of management, and addresses directly the firm’s long term survival, profitability and growth. However, as noted above, in addition to the need for some level of predictive and explanatory power, and for generalizability, effective interaction between scientific research and clinical practice in SM is a major challenge, especially because of the ‘wickedness’ of many SM problems, characterized by discontinuous change, high technical and social complexity, management biases and conflicts among multiple stakeholders that are difficult to resolve (McMillan & Overall, 2015).

Given the above context, we address one specific question in the present article that we consider critical to supporting the dual objectives of rigour and relevance: should SM scholars engaged in empirical research, restrict themselves to testing (and possibly extending) field-specific theories, or is there value in borrowing theories from other fields of academic inquiry, as the basis of hypothesis formulation and testing? Recently, Oswick, Fleming, and Hanlon (2011) assessed whether theory borrowing is commonplace in the broader organization and management theory (OMT) research. They defined theory borrowing as, “...the importation of coherent and fully formed ideas that explain a phenomenon (or phenomena) into OMT from outside the discipline” (p. 319). Oswick et al. (2011) found that approximately two-thirds of OMT research is driven by theories borrowed from other disciplines. The research revealed theory borrowing from relatively closely related fields such as sociology, psychology and economics, but also from less proximate fields such as biology, education, engineering, history, law, linguistics, mathematics, philosophy, and politics. Oswick et al. (2011, p. 328) voiced serious concerns about the observed imbalance between field-specific and borrowed theories. One key concern was that the ‘original nuance’ of borrowed theories, well understood when used in their base field, was in danger of being sacrificed when applied in an OMT context.

Other scholars have echoed such concerns. For example, Suddaby, Hardy, and Nguyen Huy (2011, p. 237) argue that OMT:

“...has somewhat awkwardly imported theories, research questions, and methods from foreign disciplines without fully adapting them to the new context. Rather than treating organizations as a subject of inquiry in their own right, management theorists appear to have treated organizations merely as new empirical sites to test, prove, and tweak old theories.”

In the OMT field, the heavy borrowing practice has come at the expense of indigenous theory development. Here, Markóczy and Deeds (2009, p. 1079) insist that OMT:

“...needs to build theories based on our unique position and understanding of management and organizational phenomena rather than co-opting the theories of economics, psychology or sociology or even of finance, accounting and marketing.”

However, the potential benefits of theory borrowing have also been acknowledged:

“...organization theory is always empowered primarily by methods and perspectives from the wider social sciences. Predominantly, it is informed by theories and methods from

anthropology, economics, psychology, and (especially) sociology. Thus, for many scholars, notions such as indigenous theorizing in organization theory are anathema to the everyday reality of research practice.” (Hassard, Wolfram Cox, & Rowlinson, 2013, p. 310).

What may therefore ultimately be more important than ‘purity’ in indigenous development is that SM scholars would follow an appropriate selection process when contemplating the borrowing of theories, and would refrain from borrowing those theories that do not meet a set of straightforward selection criteria (Ferraro, Pfeffer, & Sutton, 2005).

The early years of SM research¹, with SM being a scholarly sub-field of OMT, were characterized by a small number of research outlets publishing predominantly case-based analysis (Hoskisson, Hitt, Wan, & Yiu, 1999). Today, the SM discipline claims several dedicated journals (Azar & Brock, 2008), and a host of other outlets regularly publish SM research (e.g., *Academy of Management Journal* – AMJ, *Academy of Management Review* – AMR, *Administrative Science Quarterly* – ASQ, *Journal of Management* – JoM, and *Journal of Management Studies* – JMS). The cumulative output of these research efforts has elicited a variety of comments about the scientific status and progress of the discipline. Some researchers have reported significant progress (e.g. Hoskisson et al., 1999), whereas others have voiced substantive concerns (e.g., Ghoshal, 2005; Jarzabkowski & Whittington, 2008).

As noted above, our main focus in the present paper is the extent to which the most frequently tested theories in SM research are indigenous, as opposed to borrowed from other fields.² A theory can be defined as:

“...a collection of assertions, both verbal and symbolic, that identifies what variables are important for what reasons, specifically how they are interrelated and why, and identifies the conditions under which they should be related or not related.” (Campbell, 1990, p. 65)

As we will demonstrate below, our investigation revealed substantial theory borrowing in SM prompting the development of a screening model to permit proper selectivity in such borrowing. We hope that the model (and possible future extensions thereof) will serve the SM field in terms of improving both the rigour and relevance of its empirical work. *First*, the model allows for an ongoing ‘open door policy’ to good theory from other fields (Hassard et al., 2013). *Second*, it provides a reasoned defence to protect the indigenous body of rigorous and relevant SM theories against invasion by theories from other fields that bring no value added at best and negative value added at worst to the SM field (Markóczy & Deeds, 2009; Suddaby et al., 2011).

Explicit theory-driven research in strategic management

In order to examine the nature and extent of theory usage in SM, we chose to limit our scope with respect to time period and content. With respect to time period, we began our analysis in 1980 for two reasons. *First*, SM arguably became a legitimate academic discipline around 1980 (Azar & Brock, 2008). *Second*, the long, but manageable time frame allowed us to capture various trends and mitigate the influence of journal editorial policies on the findings. With regard to content, we limited our analysis to articles published in seven (out of a set of fifteen) dedicated SM journals (Azar

¹ Nag, Hambrick, and Chen (2007) provide insight into how the SM field may be defined.

² This question has also been raised in other fields, such as retailing (Brown & Dant, 2009) and purchasing and supply chain management (Chicksand, Watson, Walker, Radnor, & Johnston, 2012).

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