Advancing New Theory Development in the Field of International Management

Contribution Factors, Investigative Approach, and Proposed Topics

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Abstract:

- This paper seeks to re-engage international management (IM) scholars in conducting research that aims to develop breakthrough knowledge for major advancement.
- A “phenomenon-motivated, existing-theory informed, and interdisciplinary-based” investigative approach is proposed based on an analysis of the new theory development process with illustrations from the influential works of a select group of pioneering researchers in IM and related fields.
- Five emerging IM phenomena resulting from recent changes in the global business environment are recommended for study using the proposed investigative approach to create new theories that have both scholarly and practical significance.

Keywords: Theory creation · Breakthrough knowledge · Interdisciplinary research · International business · Globalization

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Introduction

Central to the mission of an academic field of study is the generation of new knowledge to help enhance understanding and inform practice. Depending on the particular field concerned, the phenomena that constitute its study domain may remain relatively stable, change, or be replaced by new phenomena over time. If the study domain does not remain stable, the field would need to continuously update its existing theories and/or develop new ones in order to keep pace with the evolving world and remain relevant.

Compared to the physical and biological sciences, the phenomena investigated by most of the social science-based professional fields (e.g., business, education, law, etc.) do not stay unchanged over time. This is particularly true for the field of International Management (IM), which studies organized human behavior within and across different national and societal contexts (Cheng 2007; Shenkar 2000). Because of globalization and other major world events, many of these contexts have undergone dramatic changes during the last 20–30 years (Buckley and Ghauri 2004; Stanbury and Vertinsky 2004). These changes are often accompanied by fundamental alterations to the political, economic, legal, and/or social systems which, directly or indirectly, affect human behavior (Parson 1951; North 1990).

A number of IM scholars have recently raised concern about the field’s declining output in new knowledge creation. Buckley (2002), for example, observes that the IM research agenda is running out of steam and challenges colleagues to discover a new “big question” for future inquiry. Adding to Buckley’s observation, Shenkar (2004) comments that the field is losing its distinctiveness in knowledge creation as evidenced by a continuous increase in IM research that investigates topics and uses concepts, data sets, and methodologies borrowed from the Strategy field. More recently, Cheng (2007) points out that many of the published IM studies are designed to test and extend an existing theory from a more established discipline, rather than to create new theories with greater explanatory and predictive power. Similar observations about stagnation in new knowledge production have been made by other scholars in the related fields of Organization Studies (e.g., Davis 2010; Schoonhoven et al. 2005) and Strategy (e.g., Barney 2005; Hambrick 2004). If left uncorrected, research in these three fields (IM, Organization Studies, and Strategy) will become increasingly less influential and relevant by producing incremental knowledge gains that have little impact on either science or practice.

This paper seeks to re-engage IM scholars in new knowledge creation activities and thereby help preserve and advance IM as a vibrant and relevant field of academic study. Inspired by Kurt Lewin’s vision that “there is nothing so practical as a theory,” it calls on IM scholars to pay greater attention to new theory development in their research and conduct studies that aim to discover breakthrough knowledge for major advancement. Toward this end, the paper first maps out the critical stages and the associated activities that constitute the new theory development process. It then proposes three contributing factors and an investigative approach that will help make the process more fruitful in producing ground-breaking new theories. These ideas are then applied to the selection and theoretical framing of five research topics for illustration. The paper closes by discussing a few challenges that need to be overcome in order for the field to move forward in the recommended direction.
Definition of Terms

Although the term “new theory development” is frequently mentioned in academic discourse, it is not a commonly examined topic in the research literature. To help facilitate the discussion that follows, we first provide a definition for each of the three component words followed by a complete definition of the term.

For the present purposes, we use the word “new” to mean something that is different from what is currently known. The greater the dissimilarity between what is known and what is being introduced, the newer the introduced idea or knowledge. As for “theory,” we define it as a set of statements devised to explain an observable phenomenon (Bacharach 1989; Mohr 1982). These statements consist of abstract constructs and their definitions as well as claims about the relationships among the constructs (Campbell 1990; Palmer 2006). Finally, “development” refers to a process or sequence of activities that starts with the initial conception of a tentative idea and ends with something more concrete and substantial.

Taken together, “new theory development” can be characterized as a creative process that seeks to provide either a first-ever explanation for a freshly identified or emerging phenomenon, or a different explanation for an existing phenomenon. The explanation is expected to differ from the existing theories in some significant and noticeable way (McKinley et al. 1999; Whetten 1989), for example, different constructs and/or postulated relationships among the explanatory variables. The greater the difference, the newer the proposed theory is considered to be.

It should also be noted that in this paper, we use the term “International Management” to refer to the broad academic field that concerns the cross-border study of business and management practices at various levels of analysis, ranging from the micro levels of individual and group to the macro levels of firm, industry, and country. As such, this definition covers the full spectrum of IM and IB (international business) research conducted by colleagues in the field.

New Theory Development

A review of the existing literature reveals that little research has been done that investigates the phenomenon of new theory development and identifies its salient dimensions or antecedents. Although rich in delineating the scientific method (e.g., Cook and Campbell 1979; Hempel 1965; Popper 1965) and also in explaining what theory or theorizing is and is not (e.g., Colquitt and Zapata-Phelan 2007; Dubin 1978; Sutton and Staw 1995; Weick 1995), the literature lacks a descriptive framework that explicitly outlines the stages or procedural steps that constitute new theory development, particularly one that builds on and supports the scientific method. To help fill this gap, we present below a model of the new theory development process by piecing together relevant ideas from previous writings about the scientific method and theory building (or theorizing). This model will then be used as an analytical framework for the discussion that follows.

The new theory development process can take one of two forms depending on an academic field’s norm of investigation. For fields that adopt the traditional scientific method
of inquiry (Hempel 1965; Kuhn 1962), such as the physical, biological, and social science disciplines as well as the professional fields on which they are based (e.g., business, education, engineering, and medicine), the process consists of three main sequential stages (and the steps or activities within each) as shown in Tab. 1: (1) Theory Conception and Articulation, (2) Empirical Testing and Refinement, and (3) Theory Affirmation and Extension. By contrast, for academic fields that are in the arts and humanities, which usually do not adopt the traditional scientific method of inquiry, their new theory development process mostly covers only the first stage of Theory Conception and Articulation. This is followed by a less formal and deliberative procedure of evaluation before the proposed theory becomes widely adopted by colleagues in the field. Of the three stages that constitute the new theory development process (see Tab. 1), the first stage, Theory Conception and Articulation, is perhaps the most complex and difficult to describe. It also involves the most number of steps, including: (1) conceptual identification and exploration of a research topic, (2) sharing of initial ideas with colleagues and soliciting feedback, (3) refinement and elaboration of initial ideas, (4) formulation of revised ideas into a sketchy proposition, (5) informal assessment and verification of the sketchy proposition, and (6) formal articulation of the proposed theory in written form. It should be noted that although these six steps are presented as sequential activities to be performed, some may need repeating before proceeding to the next one, such as step (2) idea sharing and feedback, and (5) informal assessment and verification. This reciprocal characteristic of the process is consistent with the “non-routine” (Perrow 1967; Cheng and McKinley 1983) nature of scientific research, particularly in the conceptual stage.

<table>
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<th>Table 1: Stages of new theory development</th>
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A. Theory conception and articulation
1. Conceptual exploration
2. Idea sharing and feedback
3. Refinement and elaboration
4. Sketchy proposition
5. Informal assessment and verification
6. Formal articulation of proposed theory

B. Empirical testing and refinement
7. Hypothesis formulation
8. Initial empirical testing
9. Consolidation and revision
10. Further testing across different settings

C. Theory affirmation and extension
11. Acceptance, diffusion, and application to study other phenomena
12. Consolidation, revision, and testing of updated theory
The second stage, *Empirical Testing and Refinement*, consists of four steps involving activities that are more structured in nature than those identified earlier. Specifically, this stage begins with (7) formulation of a hypothesis logically derived from the proposed theory, followed by (8) initial empirical testing of the hypothesis, and (9) consolidation of testing results and revision of the proposed theory, if needed. The second stage concludes with (10) further testing of the revised theory using multiple methods and across different settings.

The third and final stage, *Theory Affirmation and Extension*, concerns the long-term standing and impact of the new theory. It starts with (11) acceptance and diffusion of the proposed theory within the field and also adoption by colleagues in other fields to study phenomena not originally intended for its application. To the extent that these applications prove successful and help contribute to furthering understanding about these other un-intended phenomena, the scientific status of the theory will grow and so will its diffusion. This is then followed by another round of (12) consolidation of application results, revision, and testing of the updated theory. By now, the proposed theory will have become firmly established and be considered as received knowledge unless the phenomenon that it explains starts to change.

It should be noted that a new theory developed does not necessarily mean that it provides breakthrough knowledge for major advancement. Also, even if it is a ground-breaking theory, the new knowledge may not be well received by colleagues in the field and thus has little impact on either science or practice. If our aim is to develop ground-breaking theories that are impactful, we need to take a closer look at the new theory development process and identify the contributing factors that would help achieve this goal.

**Contributing Factors and Investigative Approach**

As mentioned earlier, the first stage of the new theory development process, *Theory Conception and Articulation*, involves activities that are most complex, non-routine in nature (Perrow 1967), and less structured than those of the other two stages. It is also the most consequential, as what is done in this stage will shape and determine the content and significance of the proposed theory. To help with the effective conduct of this important stage, we propose below three contributing factors and an investigative approach that will help increase the probability of success for future efforts. These factors are identified based on an examination of the influential works of a select group of pioneering researchers in IM and related fields, including: Bartlett and Ghoshal (1989), Birkinshaw (1997), Hymer (1976), Kogut and Zander (1993), North (1990), Shenkar (2001), Simon (1945), and Williamson (1981).

**Topic Identification**

The first step in the *Theory Conception and Articulation* stage is conceptual exploration. It assumes that the researcher has already developed an interest in and acquired some basic knowledge about a general phenomenon. The aim of the conceptual exploration is to identify a specific aspect or dimension of the phenomenon as the object of investiga-
tion. This is usually done through a series of iterative inductive and deductive reasoning involving knowledge gained from existing theories and/or personal observation.

It should be noted that personal observation can be through direct or indirect contact with the phenomenon of interest. In the case of learning about organizational phenomena, for example, direct contact could be personal experiences working as an administrator in a government agency or in serving as a management consultant for a client firm. Indirect contact could include such activities as interactions with practitioners in the field, reading descriptive accounts from newspapers or other media outlets, and arms-length viewing of the phenomenon in action through recurring or unfolding events, among others.

Contrary to what is commonly taught in graduate schools, existing theories may not provide the best knowledge base from which to conduct the conceptual exploration. In recounting the breakthrough research that led to the development of their transnational model of the multinational firm, Bartlett and Ghoshal (2002) report that they got most of their initial ideas not from the existing literature, but from observing the phenomenon “with authenticity, respect, curiosity, speculation, (and) the occasionally journalistic privileges (p. 13).” This observational learning enabled them to identify and describe the salient aspects of the phenomenon for further analysis and also for discussion with colleagues for feedback. It is this iterative cycle of observation, idea sharing, reflective thinking, and re-observation that constitutes the initial three steps (conceptual exploration, idea sharing and feedback, and refinement and elaboration) of the new theory development process.

Although many generations apart, Bartlett and Ghoshal’s discovery journey is not unlike that of Isaac Newton that led to the law of gravity as told by the legendary apple tree story. They both started the inquiry with an inquisitive observation of the phenomenon rather than a critical analysis of the existing theories as the primary exploration activity, and ended with the discovery of breakthrough knowledge about an important phenomenon. A similar account can also be made for Hymer’s (1976) theory of foreign direct investment (FDI), who observed at the time that firms maintained control over productive activities outside of their home countries but there was no theory to explain it. Also, in launching his influential research program on subsidiary initiative, Birkinshaw (1997) based his initial investigation on the observation that subsidiary initiative is a “relatively rare and underresearched phenomenon but its potential value to the MNC is high (p. 208).” In all these cases, the original research that led to the major discovery or theoretical invention was motivated by a desire to learn more about an observed phenomenon rather than to modify or advance an existing theory. These examples suggest that an important contributor to the success of identifying a high-potential topic for new theory development is close familiarity with the phenomenon through direct and/or indirect means of observation (Eveder and Louis 1981).

The notion that IM scholars should have their research motivated by a phenomenon of interest instead of an existing theory may come across as a risky proposition, given that most of the top journals in management require that all manuscripts contribute to theory (Colquitt and Zapata-Phelan 2007). In addressing this issue, Hambrick (2007) points out that the current practice of fitting an inquiry into an existing theoretical framework is harmful to the scientific advancement of the field of management because it has “the unintended perverse effect of stymieing the discovery of important theories (p. 1351).”
As illustrations, he cited the influential “phenomenon-motivated” research published by Dr. Franz Muller on smoking (see Brandt 2007) in the field of epidemiology and by Schmalensee (1985) on business-unit profitability in the field of economics to show that such work is important and can lead to major theoretical breakthrough.

Existing Theories

It would be a mistake, however, to think that knowledge about existing theories does not play an important role in the new theory development process. To avoid the “re-inventing the wheel” problem, one needs to be familiar with the existing theories in order to determine if the proposed research has the potential to generate value-added contribution. More importantly, existing theories can help new knowledge development in two major ways. First, existing theories can help identify which aspects of a proposed research question can be answered based on current knowledge and which aspects require new concepts and/or theoretical reasoning. Second, existing theories can help with the proper framing of the new theoretical contribution (step 6 in the Theory Conception and Articulation stage) for easier acceptance by the field. Given the norm of theoretical path dependence (Astley 1985; Palmer 2006), a new theory will be more positively received if it represents an orderly progression of knowledge that builds on established works. This is consistent with McKinley et al.’s (1999) finding that a balance between novelty and continuity is needed for a published paper to receive attention and be read widely. Otherwise, the new theory will most likely be met with suspicion and skepticism, making it difficult for acceptance by colleagues in the field.

The above exposition can be illustrated with the research on the transnational model, FDI theory, and subsidiary initiative mentioned earlier. Specifically, Bartlett and Ghoshal presented their discovery as a new organizational form that evolves from the previous “international,” “multi-domestic,” and “global” models. Similarly, Hymer articulated the FDI process as an international extension of the existing industrial organization theory resulting from market failures (Dunning and Rugman 1985). Finally, Birkinshaw defined subsidiary initiative as a manifestation of corporate entrepreneurship and thus linked his research to an established area of study which was growing in popularity at the time. By framing the object of inquiry and its theoretical invention as building on and adding substantially to the existing theories, all three research programs (Bartlett and Ghoshal 1989; Birkinshaw 1997; Hymer 1976) were successful in introducing their theoretical inventions to the field. These experiences suggest that using the existing theories to properly frame the object of inquiry and its theoretical invention, even though they did not actually motivate or guide the initial study that eventually led to the discovery, is a critical step in advancing the new theory development process.

Mode of Inquiry

The two contributing factors identified above focus, respectively, on the input (identification of a high-potential research topic) and output (proper framing of the new theoretical contribution) sides of the new theory development process. A close look at the award-winning works of Simon (1945), North (1990), and Williamson (1981) (all Nobel Lau-
reates) as well as those of Kogut and Zander (1993) and Shenkar (2001) (all recipients of the JIBS Decade Award presented by the Academy of International Business) reveals that what happens on the production side of the process may well be the deciding factor in shaping the quality of the final outcome.

More specifically, in all five cases, the researcher drew on and combined knowledge from multiple social science disciplines and, in the process, developed an integrative theory to explain the phenomenon under study. In his work on bounded rationality, for example, Simon (1945) integrated insights from micro economics and cognitive psychology in explaining why administrators make satisfying decisions. Similarly, North (1990) combined ideas from sociology, political science, and macro economics in his analysis of the co-evolution between institutions and national development. Finally, Williamson (1981) blended concepts from micro economics, contract law, and organization studies in formulating his transaction cost theory. In the case of Kogut and Zander (1993) and Shenkar (2001), both of their research challenged an established paradigm in the field by using ideas from other disciplines to build a convincing argument that exposed the paradigm’s weaknesses. Specifically, Kogut and Zander (1993) challenged the prevailing market failure explanation of the MNC by drawing from the organization studies and sociology literatures to show that as a social community, a firm is more efficient than the market in transferring organization-based knowledge. Similarly, drawing from knowledge in cross-cultural and area studies, Shenkar (2001) identified serious conceptual and methodological problems with the widely-adopted cultural distance index and proposed a more valid and comprehensive framework to replace it. The common theme among these five award-winning cases is that adopting an interdisciplinary mode of inquiry makes it more likely that the research will lead to breakthrough knowledge.

Taken together, the above discussion provides the building blocks for designing an investigative approach that aims at new theory development for major advancement. Specifically, the approach would start by relying more on observational knowledge about a phenomenon than existing theories as the primary basis for selecting a research topic. This topic (or object of investigation) would then be carefully evaluated for new knowledge generation potential through discussion with colleagues for feedback and various methods of informal assessment, including additional observation through direct and/or indirect methods. After that, attention would shift to the existing theories, both as a basis for identifying areas where new concepts or theories are needed and also for framing the new theoretical contribution as an orderly progression of knowledge from the past. Finally, the investigation would adopt an interdisciplinary mode of inquiry that seeks to create an integrative theory by combining ideas from two or more disciplines (Cheng et al. 2009; Greenwood and Miller 2010; Okhuysen and Bonardi 2011).

This investigative approach, which can be characterized as “phenomenon-motivated, existing theory-informed, and interdisciplinary-based,” is quite different from the current dominant practice. As reported by Cheng (2007), most of the recently published IM research has the characteristics of being “existing theory-motivated, phenomenon-informed, and single discipline-based.” Many of these studies are designed to affirm and extend an established theory by applying it to study an IM phenomenon. Their main goal is to advance understanding about the theory and its long-term standing, not to develop a new integrative theory to more fully explain the phenomenon under study. In terms of
the new theory development process shown in Tab. 1, the current research practice only concerns the last stage of *Theory Affirmation and Extension*. Consequently, most of the reported research output tends to be incremental knowledge gain with little value-added contribution.

It should be noted that while both the proposed investigative approach and the current research practice can be described as “theory-driven,” their motivations are very different and so are their end points. Specifically, the proposed investigative approach is motivated by a desire to learn and understand an observed phenomenon by adopting a method of inquiry which includes at least the first 10 steps of the new theory development process presented in Tab. 1. Its end point is the creation of a *new* theory that combines concepts and theoretical reasoning from two or more disciplines into an integrative framework that has greater explanatory and predictive power than the existing models. By contrast, the current research practice is motivated by a desire to affirm and extend an existing theory by applying it to the study of a phenomenon and then using the findings to further enhance the validity and/or applicability of the theory. This method of inquiry consists primarily of the last two steps (11 and 12) of the new theory development process (see Tab. 1), with the end point of either modifying or expanding the domain of the original theory. Thus, both research approaches are driven to advance theory, but one is about creating a new theory and the other is on enhancing an existing theory.

**Proposed Research Topics**

To help illustrate the utility of the proposed investigative approach and facilitate its adoption, we apply it to the mapping of five emerging world-wide phenomena as possible topics for future IM research that aims at new theory development. These global trends started about ten years ago but have become more prominent in recent years as affected by the 2008 global financial crisis. As will be discussed below, they all have the potential of changing in fundamental ways how international management will be conducted in the future, with significant implications for both IM theory and managerial practice.

First, there is wide consensus among experts that government regulation and business oversight will increase substantially in the coming decade and beyond (Ernst and Young 2009). While this change first took place in the financial sector, it has now spread to many other industries such as health care, energy, insurance, and telecom (Hasbani et al. 2009). Noteworthy to IM research is that many of the new government regulations are not just local responses to domestic issues, but are globally-coordinated policies aimed at transparency and consistency seeking to affect behavior worldwide (Ernst and Young 2009). These regulatory changes constitute a fundamental alteration to the global business environment and present exciting opportunities for new theory development. Globally-consistent regulations, for example, would eliminate many of the existing cross-national arbitrage opportunities (World Bank 2010) that international firms commonly leverage on to create competitive advantage. If implemented worldwide, these regulations would also create a global business climate that discourages irresponsible opportunistic behavior. Because cross-national arbitrage opportunities and opportunistic behaviors with guile are among the key assumptions upon which many of the existing IM theories are built, such
as those concerning global competitive strategy (e.g., Bartlett and Ghoshal 1989; Yip 1989) and foreign market entry (e.g., Anderson and Gatignon 1986; Hill and Kim 1988), new theories with contrasting assumptions will be needed to explain firm behavior under the new regulatory global environment.

Second, the changing relationship between developed and developing economies is another emerging phenomenon that can provide valuable opportunities for new theory development. Until recently, this relationship has been primarily economic in nature, with firms from developed economies investing in developing economies for market and resource access. In return, the latter would receive benefits in the form of imported technology, money, and managerial know-how. This economic exchange has been the corner-stone for many of the existing IM theories in explaining firms’ out-sourcing decisions (e.g., Witt and Lewin 2007), internationalization strategy (e.g., Vernon 1966), and patterns of foreign direct investment (e.g., Dunning 1980). The 2008 global financial crisis, however, has fundamentally altered the nature of the developed-developing world relationship. Specifically, the vast amounts of foreign reserves that the developing countries possess allow them to do things that increase their political power, including: massive domestic spending to dampen world recession, lending to the developed countries through treasury bond purchase, acquisition of de-valued assets overseas, and increased foreign aid to poor nations. This new-found political power has given the developing countries better access to and more influential participation in important international institutions such as the IMF, The World Bank, WTO, etc. It also helps boost the confidence of developing country’s firms in competing against foreign multinationals, as evidenced by the Brazilian aircraft-maker Embraer, Chinese computer firm Lenovo, and others (The economist 2008). The latter development is particularly important as many of these indigenous firms will soon become multinationals and challenge established firms for global dominance. This combination of increased political power of the developing countries and enhanced competitive capability of their indigenous firms represent an important emerging phenomenon. IM scholars should investigate this development and develop new theories to describe its salient dimensions and identify their antecedents and consequences.

Third, the growing influence of non-economic actors on world economic affairs at both the firm and country levels is another emerging phenomenon that IM scholars should pay attention to and explore for new theory development opportunities. These actors include non-government organizations (NGOs), sovereign wealth funds (SWFs) owned by nation-states, and grass-root activist organizations such as the Green Movement Group, among others. Because these actors represent the social, political, and cultural sectors of the society, their primary concerns are not economic in nature. Instead, they seek to influence firm or country decisions on economic affairs to protect or further their social, political, or cultural interests. This kind of economic decision-making is very different from the traditional free-market, for-profit model upon which many of the existing business management theories are based, including those in the IM field. To fully understand how firms and countries make major economic decisions in the future, particularly those that have global impact, scholars need to adopt an interdisciplinary approach and bring in ideas from the non-economic disciplines to help capture the complexity involved. Additionally, given the increased use of strategic alliances as a platform for conducting global
affairs (Doz and Hamel 1998; Dyer et al. 2001), it would be important to investigate if firms form partnerships with non-economic actors to gain a competitive advantage, such as seeking a national fund to ease regulatory approval and mitigate political risk. Such inquiry would help expand the domain of IM research on strategic alliances, which has so far focused on inter-firm collaboration, and contribute to new theory development in the field.

Fourth, partly as a result of the three emerging phenomena described above, the global competitive landscape of the next decade will likely be very different from the existing one. Not only will there be increased globally-coordinated government regulation and intervention both within and across countries, there will also be more non-economic actors involved in making major world economic decisions representing various social, political, and cultural concerns. All these changes are taking place within the context of shifting world powers from the West to East and North to South, with the indigenous firms from developing countries gaining increased confidence and capability in competing against the established firms from developed countries. How firms and nations respond to this new global competitive landscape would be an interesting and exciting phenomenon to watch and study. To be informative and useful, this research would need to span multiple levels of analysis (individual, group, firm, country, region, etc.), and be conducted across diverse country and industry settings. It would also need to go beyond the current dominant economic approach of studying competitiveness as productivity in resource utilization (Porter 1990, 2003). Specifically, we need to develop a more comprehensive conceptualization of competitiveness as a performance outcome and differentiate it from competitive capability and other antecedent variables. Our literature search reveals that no such conceptual work has been done in IM or the related fields of Organization Studies and Strategic Management. Given the importance of the international competitiveness topic, the research proposed above, particularly as it relates to how firms and nations respond to the newly emerging global competitive landscape, will help develop new IM theories that have both scholarly and practical significance.

Fifth, during the past fifteen years, particularly since the 9/11 attack in the U.S., terrorism has been a major concern among international business executives and public policymakers. According to Czinkota et al. (2010), annual reports of Fortune 500 firms from 2003 to 2006 included 1,141 mentions of “terrorism,” averaging more than one mention in every other annual report. Also, in a recent study of trends in the international business environment, Czinkota and Ronkainen (2009) found that terrorism was rated the highest both in terms of the intensity of the issue and its potential impact on international business by a panel of experts including corporate officers, policymakers, and academics. Despite this high-level concern, little research has been done by IM scholars to investigate the phenomenon of terrorism and identify its salient dimensions. In their literature review for the period 1986–2008, Czinkota et al. (2010) found only two published articles in IM journals (Harvey 1993; Kotabe 2005) that dealt with terrorism. To help advance future research, Czinkota et al. (2010) call for the development of new IM theory and conceptual frameworks to study terrorism. Because terrorism is a multifaceted phenomenon, an interdisciplinary mode of inquiry would be needed to uncover the multiple layers of casual dynamics and their interrelationships. This can only be done by drawing on and combining knowledge from multiple disciplines, including religion, history, sociology,
political science, and economics, among others. As most of the recent attacks were carried out by religiously inspired terrorists (Cronin 2003; Hoffman 2006), special effort is needed to investigate the role of religion in the “new terrorism” (Enders and Sandler 2006; Wernick and Kundu 2008) and explore its effects on terrorist acts. This knowledge is potentially important because it may tell something about the behavioral dynamics of terrorism and its possible resolution by combining the logic of religion and insights from other perspectives. This inquiry complements the research agenda proposed by Czinkota et al. (2010) which focuses on the direct and indirect effects of terrorism on international business and how these effects can be managed or responded to.

Challenges and Moving Forward

This paper has sought to re-engage IM scholars in new knowledge creation activities and thereby help advance IM as a vibrant and relevant field of academic study. Using illustrations from the influential works of a select group of pioneering researchers in IM and related fields (Bartlett and Ghoshal 1989; Birkinshaw 1997; Hymer 1976; Kogut and Zander 1993; North 1990; Shenkar 2001; Simon 1945; Williamson 1981), an investigative approach was proposed to help guide IM scholars in developing new ground-breaking theories. This investigative approach has the characteristics of being “phenomenon-motivated, existing theory-informed, and interdisciplinary-based.” It differs from the current “existing theory-motivated, phenomenon-informed, and single discipline-based” research practice whose main goal is to advance understanding about an established theory and its long-term standing, not to create a new theory to explain the phenomenon under study.

As discussed earlier, the success of the proposed investigative approach requires the researcher to learn about the phenomenon of investigation through intense personal observation based on direct and/or indirect contact. This is in addition to reviewing the research literature and learning about the existing theories. Both kinds of knowledge are necessary in order for the researcher to identify a high-potential topic for investigation and to conduct successfully the other steps outlined in the Theory Conception and Articulation stage (see Tab. 1). The amount of work required to complete this stage alone is quite substantial, and may be more time consuming than designing a theory-application study based on the current dominant research practice. Given the norm of efficiency and the existence of an academic culture that favors the status-quo, it would take extra motivation for university scientists to engage in research that aims at new theory development.

In his recent presidential address at the Academy of Management Meeting, Walsh (2011) discussed the “audit culture” surrounding many of the U.S. business schools and the problematic effects that it has on scholarship, including: faculty teaching to course evaluation ratings, “team production” to generate volume in research output, and a “win at all costs” mentality in pursuit of A-level journal publications including committing research misconduct, among others. This audit culture is pushing faculty to conduct “easy research” for short-term gain, which is also a major reason why there has been a decline in new theory development because such research is much harder and takes longer to complete.
Advancing New Theory Development in the Field …

It is beyond the scope of the current paper to identify solutions for the challenges mentioned above. While these are difficult challenges requiring substantial effort to overcome, the way forward is to address them directly and start the process of debating the pros and cons of the different approaches to conducting research. In fact, this dialogue has already begun among many of our senior colleagues, as reflected in the contents of the review and commentary articles cited earlier in this paper (e.g., Barney 2005; Buckley 2002; Cheng 2007; Hambrick 2007; Shenkar 2004; Walsh 2011). Many of these articles also provide suggestions to help address the challenges described above, including: changing evaluation criteria at leading management journals to accept manuscripts that report “interesting facts” with theory development potential (Hambrick 2007); calling upon tenured faculty to “step up and reveal some courage” in leading reform in promotion and tenure practices (Walsh 2011); developing new doctoral programs to train a new generation of management scholars as interdisciplinary thought leaders aspired to pursue breakthrough knowledge (Cheng 2007); and joining forces with faculty in the area studies and social science departments to conduct research that addresses important global issues (Shenkar 2004).

In sum, the “phenomenon-motivated, existing theory-informed, and interdisciplinary-based” investigative approach proposed in this paper has the potential to propel IM into a leading academic field where new theory and breakthrough knowledge are generated. However, the challenges discussed above are real and need to be resolved before progress can be made. We invite interested colleagues to join us in this important effort and debate the best way forward. It is through open discussion among concerned scientists that effective solutions are found and collectively contribute to the advancement of scholarship that benefits society.

References


