
Critical issues in international management research: an agenda for future advancement

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Abstract: This paper examines the roles of theory, country context, and disciplinary knowledge in International Management (IM) studies. It is argued that the existing approach of conducting research that is theory-motivated and phenomenon-based, treats country-context as boundary conditions, and draws on knowledge from a single-discipline, has serious limitations and is impeding progress. It proposes a new path for IM research that is phenomenon-motivated and theory-based, treats country context as analytical variables, and draws on and integrates knowledge from multiple disciplines. This approach expands the domain of IM research and produces interdisciplinary theories that have greater explanatory and predictive power than the current practice.

Keywords: international management; international business; interdisciplinary approach; theory advancement.

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1 Introduction

It is a great honour to be invited to write for the inaugural issue of the EJIM. This is a happy and important occasion, as it marks a key milestone of progress in the field of IM. The significance of the launch of the EJIM is the recognition that a new journal, rooted in the rich tradition of European scholarship and aimed to promote a regional perspective

through interdisciplinary inquiry, will complement the existing leading journals in the field and, together, contribute to the further advancement of IM theory and practice. This is a worthwhile and achievable goal, and one that I very much endorse.

In this essay, I will examine the current state of IM research and discuss a number of critical issues for future advancement. Supported by the two sub-areas of multinational management and comparative management, IM has accomplished much in advancing its status as a respected academic field during the past 50 years. A review of the research done to date, however, indicates that there are some serious problems that need to be resolved in order for IM to reach its next level, including:

- the lack of cross-fertilisation between the multinational and comparative management sub-areas
- undue fragmentation in the field
- its recent focus on conducting theory-extension research
- the over-reliance on quantitative analysis methods
- the imbalance between global and local concerns.

It is argued that these problems are only symptoms of some more basic issues in the existing investigative approach, including the design of research to advance understanding of the theory rather than the phenomenon under study, the treatment of country context as ‘boundary conditions’ instead of ‘analytical variables’, and the use of single-discipline rather than cross-discipline knowledge as a basis for analysis. The paper proposes a new research approach to correct these problems, and concludes with an action agenda to help facilitate its implementation and adoption by colleagues in the field.

The ideas developed in this paper draw from my 30 years of research and publishing in both the fields of IM and organisation theory, as well as other observations and experiences learned during this period. The latter include my seven years of serving as the director of a federally-funded Center for International Business Education and Research (CIBER) at the University of Illinois (1999–2006), and five years (1999–2004) as an elected officer of the Academy of Management’s International Management Division, including the posts of pre-conference, program, and division chairs. Additionally, my work as Co-editor of the *Elsevier/JAI Research Series, Advances in International Management*, for the past ten years has broadened my perspective, enabling me to see the larger forest of IM research as well as having a close-up look at the individual trees. It is my view that IM currently stands at an important cross-roads, and has the opportunity to choose a path that will not only further advance IM, but also help re-shape the larger field of management research for the 21st century. This is an exciting time to be an IM scholar, and we should seize the moment to have an impact.

2 Current state of the IM field

As a scientific field of academic study, IM includes two main sub-areas, multinational management and comparative management. Started originally as an inquiry into the Multinational Corporation (MNC) as an alternative form to market transactions for cross-border economic exchange (Buckley and Casson, 1976; Hymer, 1960; Vernon, 1966), multinational management has evolved over the years into a more

comprehensive area of study about the what, why, where, and how of the MNC and its effective functioning (see review by Cheng (1991)). More recently, the study domain has expanded to include topics concerning the globalisation of the world economy and its effects on competition between firms and nations (Snowdon and Stonehouse, 2006; Werner, 2002). Most of the researchers active in this area have more of a macro strategy or organisation theory orientation, with primary disciplinary training in economics, sociology, or political science. In the academic literature, multinational management is often referred to as International Business (IB) studies, which has its origin in the field of international trade and commerce (Tallman, 2004).

Comparative management, which began in the late 1950s as an inquiry into the transfer of management practices across countries, has since developed into a larger area of study about the cross-national or cross-cultural similarities and differences of management phenomena (Farmer, 1984; Miller, 1984). Lately, this area has expanded to examine interpersonal interactions across country or cultural contexts and their implications for managing multinational teams (Shapiro et al., 2005). While most of the earlier comparative management studies were descriptive in nature, recent research has adopted a more analytical approach to investigate the effects of various country level factors, such as those pertaining to a nation's economic, legal, political, and cultural systems, on management and organisational practices (see review by Cheng (1989)). Unlike multinational management which has more of a macro orientation, comparative management encompasses studies conducted at multiple levels of analysis, including those at the individual or group level conducted by researchers trained in psychology and anthropology.

A number of observations can be made about the current state of the IM field. First, while multinational management and comparative management have progressed substantially over the years, each with its own advances in both theory and research methodology, there remains little cross-fertilisation between these two sub-areas. Miller (1984) first commented about this apparent separation more than 20 years ago, which has since been repeated by other senior scholars in the field, including Child (2000) and Shenkar (2000). This is unfortunate, as the two sub-areas are rooted in the social and behavioural sciences, and have great complementarity potential that can help further one another and contribute to the development of joint knowledge with synergistic impact. There are multiple ways through which this can be accomplished, as have been proposed by Child (2000) and Shenkar (2000), but their suggestions have largely been ignored by IM researchers to date. This artificial and unnecessary separation between multinational and comparative management has weakened the capability of the IM field to explain and predict the complex cross-border phenomena that constitute its study domain.

Second, the traditional eclectic orientation of the IM field (Buckley and Lessard, 2005; Dunning, 2004), both in theory and research methodology, has been an asset as well as a liability in its development into a more mature area of academic study. While the use of multiple conceptual lenses and theoretical perspectives, coupled with differing methodological approaches borrowed from various fields, help scholars frame and investigate their studies in novel and creative ways, this eclectic orientation has the negative effect of bringing undue fragmentation to the field and hinder the systematic accumulation of knowledge. Such effect is particularly damaging to the IM field, as most of the studies done to date have been single-discipline based, designed to apply existing knowledge from economics, sociology, psychology, or political science to investigate phenomena within a multinational or comparative management context

(more discussion on this later in the paper). In the absence of a concerted effort to combine cross-discipline knowledge into an integrative framework, the research findings remain separated and dispersed. This is an impediment to progress in scientific fields, including IM, as their advancement depends on continuous paradigm development based on shared, accumulated knowledge over time (Cheng, 1984; Kuhn, 1962; Lodahl and Gordon, 1973).

Third, in its quest for respect and legitimacy in the academic world, IM research has in recent years moved away from its tradition of being phenomenon-motivated to one that aims to test and extend existing theories from the more established social science disciplines (see recent review by Buckley and Lessard (2005)). The main reason for this change is to increase rigor in IM research and for it to further advance the 'parent' discipline from which it borrows theory and method. Instead of asking such research questions as "Why do multinational firms exist and what determines their effective functioning?" or "Why are there cross-national differences in management practice and what accounts for these differences?" The questions being asked now are more in the form of "What does transaction cost theory have to say about joint venture governance and how can research on this topic help further advance transaction theory?" or "Are theories of organisation and management developed in the west, e.g., institutional theory and resource-based view of the firm, valid in Asian societies?" While this theory-motivated approach makes IM research more similar to studies done in the other sub-areas of the larger management field, such as the economics-based strategy and the behavioural science-based organisation theory/behaviour, its main focus is on extending existing theory rather than developing new ones to more fully explain the phenomena under study. This is inconsistent with the goal of science which is explanation and prediction (Hempel, 1965), not the advancement of a particular theory or discipline for its own sake.

Fourth, the increased reliance on quantitative data analysis using large samples, as is characteristic of much of the management research published in the academic journals, is another recent development in the IM field. While this practice has improved the perceived sophistication of IM research, it has the negative effect of leading scholars to investigate phenomena that can readily be investigated with quantitative indicators (e.g., firm or industry characteristics), at the expense of those that are hard to measure such as societal culture and its influence on behaviour (e.g., managerial decision-making). This is particularly problematic for IM research that involves data collection from senior MNC executives, who often do not have the time and/or interest to fill out survey questionnaires; or research that investigates IM phenomena in emerging economies, which often do not have existing archival data pertaining to their firms or industries as those commonly available in the west. If IM research is to keep up with the rapid pace of globalisation, and to advance both theory and practice in the business world, we need studies that investigate phenomena that are easy as well as difficult to quantify. This would require the use of both quantitative and qualitative data analysis methods in IM research, particularly in a triangular way that combines the strengths of case research and large sample investigation (Morris et al., 1999).

Finally, there is a significant imbalance between global and local concerns in IM research that seems to be increasing as indicated by recent publications in the field's leading journals (Shenkar, 2004). More specifically, the view of convergence and standardisation across countries and cultures, such as those expressed by Levitt (1983), Yip (1989), and others, is gaining greater acceptance than the opposite view of

divergence (e.g., Berger and Dore, 1996; North, 1990; Whitley, 1999) or regionalisation (Rugman, 2005) as a working premise for IM inquiry. This is evidenced in the increased use of the word 'global' in IM research and publishing to convey the image of an integrated world with diminishing cross-national or cross-cultural differences. There is also increased tendency to view specific countries as members of a particular grouping such as the North America, European Union, Association of South East Asian Nations (ASEAN) and Brazil, Russia, India and China (BRIC), implying high homogeneity within the country grouping which may not be actually present (as noted by Oded Shenkar in the recently distributed 2007 Academy of IB Call for Papers). This bias toward global issues has limited the scope of IM research and its opportunity to incorporate local country knowledge into the development of more comprehensive theories about organisation and management.

In sum, the IM field has accomplished much in advancing its academic status as a respected area of study during the past five decades. While this is both good and desirable, there are obstacles ahead that need to be overcome in order for IM to reach its next level. As will be discussed in the next section, the problems identified above can be addressed by examining three critical issues in IM research. Unless and until these issues are resolved, IM will not be able to realise its full potential as a scientific field of academic study.

3 Critical issues in IM research

3.1 The role of theory

Among the social science trained management scholars, both in and outside of IM, there is general consensus that theory plays an important role in our research and teaching. We often hear reference to Kurt Lewin's famous quote that "there is nothing so practical as a theory" to justify why we focus our research on theory development and base our teaching on confirmed theories in the field. In the case of IM, there was a strong push during the 1970s to move away from its traditional use of descriptive, case study to adopt a more analytical, scientific approach to inquiry (Heydebrand, 1973; Miller and Cheng, 1978). This led to the borrowing of existing theory and methodology from the more established social science disciplines (e.g., economics, psychology, sociology, political science) to investigate topics of interest to IM scholars. While such effort had contributed significantly to enhancing the academic rigor of IM research, it also brought an un-anticipated consequence which has not been helpful to the field's advancement in either theory or practice.

This un-anticipated consequence is the tendency of IM scholars to conduct their research more as a tool to test and extend existing theory from the established disciplines rather than to deepen the understanding of the phenomena under study (see recent review by Roth and Kostova (2003)). A typical study would usually start with the selection of a theory of interest to the investigator from a single discipline, e.g., economics or sociology, followed by an application of the theory to frame the research question and formulate the study design to answer it, all done in ways that are consistent with and conform to the investigative norms and requirements of the 'home' discipline. Additionally, in the write-up of the research paper for journal submission, the emphasis is on highlighting the study's contribution to supporting (or refuting) and

extending the selected theory from the home discipline which provided the analytical framework for the study. Throughout the whole process, the dependent phenomenon that is the object of the investigation remains largely in the background, only to be used as a medium for hypothesis testing as relating to the selected theory. If the hypothesis is confirmed, the findings are treated as lending further support to the established theory. If not, there will be post-hoc interpretation as to what the findings might mean and also their implications for modification to the original theory. In either case, the main focus of this “theory-motivated, phenomenon-based” research approach is on advancing understanding of the established theory from the home discipline, not the phenomenon under study.

In their acceptance speech at the 2001 Academy of Management Meeting for their Distinguished IM Scholar Awards, which they later published in the *Advances in International Management*, Bartlett and Ghoshal (2002) reflected on their seminal work on the transnational firm and discussed the underlying dynamics that led to the breakthrough discovery. Specifically, in describing their research approach, Sumatra Ghoshal commented:

“We talk about our research as literature-induced because it gives legitimacy to it. I do not believe the transnational really came out of any of that literature. That does not mean reading doesn’t influence minds. But basically, the findings and conclusions came from a different place. You look at the phenomena with authenticity, respect, curiosity, speculation, the occasionally journalistic privileges, and you get something.” (Bartlett and Ghoshal, 2002, p.13)

What is interesting and significant about Bartlett and Ghoshal’s work is that it differs substantially from the predominant “theory-motivated, phenomenon-based” research described earlier. Specifically, they started their inquiry process by observing an interesting phenomenon, followed by identifying and describing the salient aspects of the phenomenon for investigation, and then “... trying through carefully structured samples to compare and create models and frameworks that take it to the next stage” (Bartlett and Ghoshal, 2002, p.34). In the process, they grounded their research in theory by drawing on and combining ideas from various areas within economics and management. The conceptual models and frameworks that they created along the way provided the foundation for further elaboration and refinement in subsequent investigations. This research approach can be described as “phenomenon-motivated, theory-based”, and it led to the end product of the revolutionary new corporate form for global competition – the transnational – which Bartlett and Ghoshal described in their 1989 book, *Managing Across Borders*. The book, named by *Financial Times* as one of the 50 most influential business books of the century, has since been translated into multiple languages and motivated a new stream of academic research on multinational strategy and organisation both in and outside of the IM field (Rugman, 2002).

It should be noted that Bartlett and Ghoshal (2002) describe their work as ‘hypothesis-creation’ rather than ‘hypothesis-testing’, and thus one should not treat their final product as a ‘formal theory’ (Blau, 1970). Rather, it is a work in progress to be further developed and refined by subsequent investigations. In this respect, the “phenomenon-motivated, theory-based” research approach as practiced by Bartlett and Ghoshal is not unlike that adopted by Nobel laureate Herbert Simon which led to his breakthrough knowledge about bounded rationality in decision making (Simon, 1945). As described in Tsang’s (2006) recent paper on behavioural assumptions and theory development, Simon (1979) noticed in a field study of administrative decision making

that the assumption of perfect rationality was not tenable. Subsequently he and his colleagues conducted several 'anthropological' studies that elicited descriptions of decision-making procedures and observed the course of specific decision-making episodes. They grounded their investigations in theory by drawing on existing ideas from both economics and psychology and, in the process, combined these ideas into new ones which later resulted in their bounded-rationality model of administrative decision-making. As the saying goes, the rest is history!

In sum, theory can play a facilitative or impeding role in the future advancement of IM research. If we are to continue the current practice of conducting theory-motivated, phenomenon-based research, the end results will be incremental theory extension with little *new* knowledge development. Also, the primary beneficiary of such work will continue to be the established social science disciplines which provide the original theory for the investigation. Its value-added contribution to advancing IM theory and practice will be limited. However, if we decide to take a different path, and start conducting research that is phenomenon-motivated and theory-based, such as that represented by Bartlett and Ghoshal (1989) and Simon (1945), it will lead to break-through knowledge and, subsequently, new theory, that will help re-shape future research in the IM field and beyond. Furthermore, given the dependent variable (rather than independent variable) centred orientation of this investigative approach, it will have the additional benefit of providing a focus for future research which will help reduce fragmentation and enhance knowledge accumulation in the field, something that Mohr (1982) recommended for the organisation theory field some 25 years ago.

3.2 The role of country context

In an earlier paper (Cheng, 1994), I examined the concept of universal knowledge in the field of organisational science, and argued that there are two kinds of generalised research findings that can be applied cross-nationally. The first refers to findings that are invariant across different national or societal settings, such as the often reported positive relationship between firm size and structural differentiation (Blau, 1970). This 'context-excluded' universal knowledge is most commonly recognised and sought after by organisational scholars and social scientists in all fields (see recent exchange between van de Ven and Johnson (2006) and McKelvey (2006)). The second refers to research findings that include characteristics of the societal context as explanatory variables of the dependent phenomenon under study. These societal characteristics can take on the role of an independent variable having a main effect on the dependent phenomenon (Cheng, 1994, p.163), e.g., a positive relationship between a country's cultural diversity and firms' structural differentiation. Or, they can be a moderator variable having a conditioning effect on the relationship between a third variable and the dependent phenomenon (Cheng, 1994, p.163), e.g., a country's population density weakens the positive relationship between firm size and structural differentiation. These 'context-embedded' relationships can be empirically tested using societal settings as quasi-experimental sites (Bhagat and McQuaid, 1982; Cheng and Miller, 1985; Przeworski and Teune, 1970). If confirmed, the research findings can be applied cross-nationally and complement the 'context-excluded' relationships in the development of a more comprehensive theory about the dependent phenomenon.

What is important about these two types of universal knowledge is their implied roles for country context in IM research. In the case of the first type, where universal

knowledge is viewed as research findings that are invariant across national settings, country context is not considered a source of variation in the dependent phenomenon under study. Universality of the postulated relationship is assumed, and country context is treated as 'boundary conditions' to be noted later if and when the empirical evidence shows otherwise. This 'context-excluded' approach has been the dominant perspective in social science inquiry (Hickson et al., 1974; Kohn, 1987), and is characteristic of much of the IM research done to date, in both the sub-areas of comparative management and multinational management.

By contrast, the second type of universal knowledge, which concerns relationships that include societal characteristics as explanatory variables, implies a more substantive role for country context in research and theorising. Instead of assuming the absence of societal influence on the dependent phenomenon under study, country context is recognised right at the outset as a potential source of variation, and is treated as 'analytical variables' whose effects are to be identified and incorporated into the analysis. This 'context-embedded' approach to research and theorising requires the scholar to conduct cross-level effect analysis (Cheng, 1983, 1989; Rousseau, 1978, 1985), which is different from the social science tradition of single-level effect analysis where both the explanatory and dependent variables are usually at the same level of observation (e.g., individual or group).

As applied to IM research, adoption of the 'context-embedded' approach will help address the significant imbalance between global and local concerns currently existing, as discussed earlier. In the case of comparative management research, instead of conducting cross-national replication studies to test for universality of established 'context-excluded' theories, as is commonly reported in the literature (see review by Cheng (1989)), the 'context-embedded' approach would require the researcher to incorporate local country knowledge into the analysis of the phenomenon under study. Specifically, the researcher would need to identify relevant characteristics of the societal context as explanatory variables of the dependent phenomenon, and test the postulated relationships using cross-national settings as quasi-experimental sites (see Cheng (1989, 1994) for a detailed discussion of the study design). In the case of multinational management research, the 'context-embedded' approach would help bring the country back as a critical factor in the analysis of strategic options for multinational firms (Prahalad and Doz, 1978), something that Rugman (1981) first talked about in his international business matrix linking country-specific to firm-specific advantages. Instead of assuming away or minimising cross-national differences, as is characteristic of much of the recent multinational management literature, researchers would first identify these differences and seek to incorporate them as obstacles to overcome or contingencies to be dealt with in the design of a firm's multinational strategy.

In sum, the role of country context in IM research is a critical issue in the future advancement of the field. The traditional approach of treating country context as 'boundary conditions' has limited the scope of IM research and its capability to incorporate local country knowledge into the development of more comprehensive theories about organisation and management. This is true for both of its sub-areas of comparative management and multinational management. To bring balance to the global and local concerns in future IM research, we need to pay greater attention to treating country context as 'analytical variables', and incorporate their effects into the analysis of the dependent phenomenon under study. In doing so, we expand the domain of our inquiry and further our ability to develop comprehensive theories that have both universal

applicability and global relevance. Additionally, the incorporation of country context into the conduct of comparative and multinational management research will help bring these two sub-areas of IM closer to each other and facilitate cross-fertilisation of knowledge with synergistic impact (Child, 2000; Shenkar, 2000).

One should note that much of the local country knowledge cannot be easily obtained without the use of qualitative research methods, as has been noted by Redding (2005), Morris et al. (1999), and others. This is particularly true for a foreign researcher trying to learn about and understand the cultural norms and values of a host country. While there are aspects of the society that one can investigate by way of quantitative indicators such as trade and employment statistics and other country characteristics, the *correct* interpretation of such data would require a deep understanding of the underlying societal dynamics, information which is best obtained through in-depth observational research like case studies. This will provide the needed background knowledge for subsequent large sample quantitative studies with a finer focus of investigation.

3.3 The role of disciplinary knowledge

As mentioned earlier, the IM field has a strong eclectic orientation which is both an asset and a liability in its development into a mature area of academic study. On the asset side, the benefit is the diversity in theoretical and methodological perspectives which makes IM research interesting and open to new ideas. On the liability side, the cost is undue fragmentation and difficulty in systematic knowledge accumulation. This problem is worsened by the field's 'discipline centric' bias, and its reliance on the theory-motivated, phenomenon-based research approach, as discussed earlier. In the absence of a concerted effort to combine cross-discipline knowledge into an integrative framework, the research findings remain separated and dispersed, with limited impact on advances in either IM theory or practice.

One learns from high school art or physics that there are three basic colours – red, blue, and yellow, each comes with its own set of physical properties. By mixing any two of the three basic colours, and by adding white in different intensities, an artist will be able to create a wider range of new and beautiful colours.

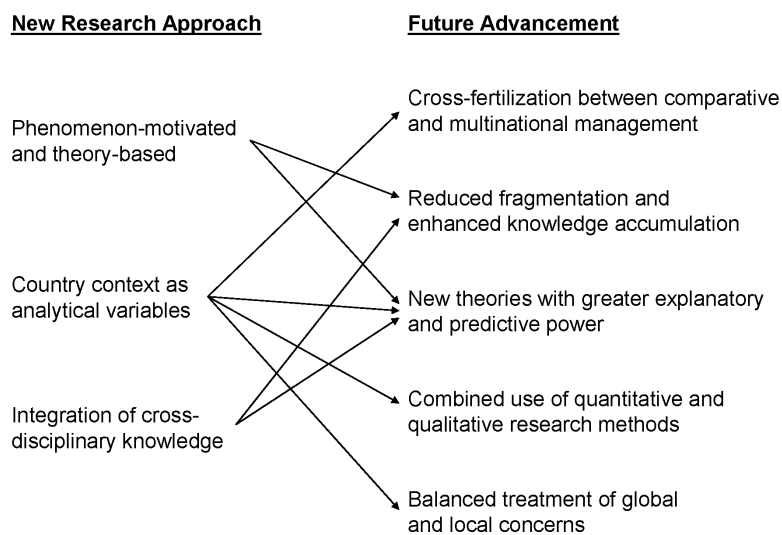
In applying the colour principles, one can see that conducting IM research based on knowledge from one single discipline (e.g., economics or sociology) is like an artist painting with one basic colour. The resulting product will only reveal the real phenomenon in one specific, uniform way, with no contrast or comparison to other perspectives. To improve on this, one can draw on ideas from two or more disciplines, but without combining them in the process. This represents a multi-disciplinary research approach, and is similar to an artist painting with all three basic colours, but without mixing them to create new colours. The end result is an improvement over the painting with only one colour, but it still does not capture the full colour complexity of the real phenomenon. To achieve the latter, a painter would need to mix the basic colours in different ways, and create a range of new colours to match the colour variety found in nature.

As applied to IM research, the above discussion suggests that we need to conduct inter-disciplinary inquiry by drawing on and integrating ideas from different areas (Cheng and Bolon, 1993). This will help create new concepts (e.g., transnational form and bounded rationality) to capture the complexities of the phenomena we study, thus leading to the development of more comprehensive theories for explanation and

prediction. Additionally, the conduct of inter-disciplinary inquiry will help support and further the phenomenon-motivated, theory-based research practice described earlier, as they both have the same goal of advancing science through new theory development (instead of extending existing theory) that adds to the existing knowledge about the phenomenon under study. Coupled with the treatment of country context as analytical variables in study design, IM research will be able to produce findings that are of value to both academics and practitioners by developing generalised, contextual theories (Cheng and McKinley, 1983; Cheng, 1989, 1994; Cheng and Cooper, 2003), something that the larger management field has long been struggling to deliver (see recent debate between van de Van and Johnson (2006) and McKelvey (2006)). In this respect, IM has the opportunity to take a leadership role in re-shaping management research as a whole toward greater integration between theory and practice, thus making our field more scientifically rigorous and practically relevant.

Figure 1 summarises the key points discussed above regarding the new research approach and its potential contributions to future advancement in the IM field.

Figure 1 New research approach and future advancement in IM



4 An agenda for future advancement

As a relatively new field of academic study, IM has progressed much during the past 50 years, and is currently at an important point in history where some critical decisions need to be made which will affect its future development. Specifically, we can continue the current practice of conducting research that is theory-motivated and phenomenon-based, treats country-context as boundary conditions, and draws on knowledge from one single-discipline. The end results will be incremental extension of existing theories from the more established disciplines which focus primarily on context-excluded relationships and offer a partial, discipline-based view of the phenomena under study. Alternatively, we can take a new path, and start conducting research that is phenomenon-motivated and theory-based, treats country-context as

analytical variables, and draws on and integrates knowledge from multiple disciplines. This path will lead to very different outcomes, which include new and interdisciplinary theories that are context-embedded and offer a more comprehensive view of the cross-border phenomena we study. These theories will have greater explanatory and predictive power, and have cross-national applicability and global relevance.

From both an academic and practice standpoint, IM research, and management research in general, will be valued and supported by the society only if it helps advance understanding and solve real-life problems, either in the short or long-term. To this end, we need to focus our research on generating new and comprehensive knowledge about important phenomena that surround us, particularly those that are emerging due to changing world events such as globalisation and new economic and technological developments. The current practice of conducting research will not produce the break-through knowledge that we need to keep pace with the fast changing world. To accomplish this, we need to try something different, and the new research approach described above may be one such alternative.

As with any behavioural change, particularly those that deviate from and challenge the status-quo, there will be great resistance from the establishment. To help facilitate implementation of the new approach in future IM research, and its adoption by colleagues in the field, I propose the following action agenda.

First, we need to increase the awareness of our colleagues, both in and outside of IM, of the serious limitations of the existing research approach and the costs of continuing such practice, including the risk of losing further support from the larger society. In parallel, we need to introduce the alternative research approach and explain the advantages it offers, particularly its contribution to developing new and more comprehensive knowledge to help explain and predict important emerging phenomena resulting from rapid changing world events. This awareness-enhancing activity is a most critical first step to any systemic change effort, and can be conducted in numerous forums, including presentations at academic conferences, journal papers, daily interactions with colleagues at school, and teaching of doctoral students. In particular, we need to invite senior colleagues to involve in this discussion, and encourage them to tell others about the need for a new approach to future IM research.

Second, we need to help colleagues who are interested in trying out the new research approach to acquire the needed knowledge and skills for its implementation. Given that the new approach differs in several important aspects from the traditional one, such as the theoretical framing of the phenomenon based on integrative frameworks that draw on existing knowledge from multiple disciplines, the conceptualisation of country context in terms of relative standings on a set of societal-level variables and the subsequent selection of comparison cross-national settings as quasi-experimental sites, and the final writing of the research report to achieve a balanced coverage of new theory development and systematic accumulation of knowledge from past investigations, it is important for the newcomers to learn first-hand from the experienced colleagues. This can be achieved by organising research workshops at annual academic conferences such as those at the Academy of Management (AOM), the Academy of International Business (AIB), and the European Group for Organisation Studies (EGOS). Alternatively, the newcomers can join a research team led by the experienced colleagues and learn it by doing. Either way, it is important that learning opportunities be provided so that the needed knowledge and skills will be acquired by those who wish to try the new research approach.

Third, to provide the needed support and incentives for trying out the new research approach, we need to invite editors of the more established journals, both in IM and the larger management field, to consider publishing special issues to highlight the new work and provide a forum for focused debate and evaluation. In parallel, we should also encourage editors of the newer journals, such as *EJIM*, to include in the editorial policy a desire to promote the new research approach as a way to carve out their own niche. Finally, to ensure that the papers submitted to the journals are of the highest quality, authors should be encouraged to seek feedback from colleagues through in-house seminar or open conference presentations, and to revise their drafts for improvement before sending them off. On the side of the journals, editors should understand that they are reviewing manuscripts that deviate from the norm but have potential for significant advances in both IM theory and practice, and find reviewers who are open to new ideas to evaluate the submissions. It is only through the combined effort of diligent researchers/authors and fair-minded journal editors/reviewers that the new approach will have the best chance to showcase itself and demonstrate its value-added contributions to the field.

Fourth, to the extent the new research effort will lead to more comprehensive theories with greater explanatory and predictive power, as discussed earlier, we should actively seek opportunities to apply the resulting knowledge to managerial practice and monitor its impact. This can be done through presentation of the research findings to a managerial audience either through executive training or consultancy. We should also invite senior executives who are in a position to initiate change in corporate strategy and organisation to offer their firms or business units as experimental sites for theory testing and refinement. If shown successful, this will be convincing evidence of the new research approach's effectiveness, thus providing strong support and incentive for its further implementation and adoption by colleagues in the field.

Fifth, to ensure that the new approach will have a long-term presence in IM research, we need to train a new generation of scholars by reforming our doctoral curriculum. Instead of producing graduates who are schooled in single-disciplines and socialised into being a loyal citizen of one particular discipline (e.g., economics, psychology, etc.), as has been the norm in most doctoral programs in IM as well as in the larger management field, we need to provide students with theoretical and methodological grounding in multiple disciplines, and socialise them into interdisciplinary thought leaders capable of doing integrative, boundary-spanning research that advances both theory and practice. In the process, we should encourage students to engage in field investigations that make use of both qualitative and quantitative methods, and conduct cross-level effect analysis which is critical to developing a deep understanding of complex cross-border phenomena, as discussed earlier. Finally, schools that are interested to adopt the above approach to their IM doctoral programs should consider forming a consortium to provide professional and social support for their students as well as a primary marketplace for recruiting graduates from their programs. This will ensure the incoming students that their efforts will be rewarded upon graduation.

It should be noted that the five actions recommended above need to be implemented as a system in order to have the maximum impact. Given the consensus nature of paradigm development within scientific fields (Kuhn, 1962), and the tendency of organisational incentive systems, including those at universities, to support the prevailing norms, we will need all the help we can get in order to change behaviour and bring about the advances in future IM research discussed earlier. This is a most

challenging task, but one that can be accomplished if we put our collective minds and efforts to it.

5 Conclusion

It has been 47 years since the publishing of the *Management International Review*, the first major journal devoted to advancing IM research, which also happens to be European-based. During this period, a number of other journals have also been launched to keep pace with the field's rapid growth, including the *Journal of International Business Studies*, the *Journal of World Business* (formerly the *Columbia Journal of World Business*), the *Journal of International Management*, and the Elsevier/JAI Research Series, *Advances in International Management* (formerly *Advances in International Comparative Management*). None of these publication outlets, however, aims to promote a regional perspective through interdisciplinary inquiry, as indicated in their editorial policy statements.

The EJIM, which seeks to advance European management knowledge with an interdisciplinary emphasis, fills an important niche in the IM field that will support the new research approach described earlier. Given the great diversity of societal characteristics across the member nations of the European Union (EU), and the many interesting and important emerging phenomena resulting from the recent changes taking place within the EU, there are exciting opportunities for conducting phenomenon-motivated, theory-based IM research that develops and tests new interdisciplinary theories using cross-national settings as quasi-experimental sites. Coupled this with the rich European tradition in employing qualitative research techniques, EJIM is well positioned to attract and publish the highest quality works that complement those appearing in the other journals, and together, propel IM into a leading academic field with potential to re-shape management research for the 21st century.

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