Asian Contexts and the Search for General Theory in Management Research: A Rejoinder

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April 7, 2007

Forthcoming in: *Asia Pacific Journal of Management*

Acknowledgements: I thank all colleagues who communicated their views on my earlier paper to me, especially those who prepared formal comments for *APJM*, and Editor-in-Chief Mike Peng for encouraging the debate.

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Abstract

"Asian Management Research Needs More Self-confidence" (Meyer, 2006) generated a surprisingly extended and diverse set of responses from Asia and beyond. In this rejoinder, I draw together a few lines of arguments that have emerged in that debate with the aim of moving the debate—and thus Asian management research agendas—forward. In particular, I argue that context is a crucial variable to explain management behavior, yet for practical reasons, it has been neglected in research published in top journals. Thus, I challenge management scholars in Asia and beyond to devise new research strategies to enhance our understanding of the contextual boundaries of our knowledge.

The positive and constructive reaction that I received on my recent paper (Meyer, 2006) has somewhat surprised me. I received supportive e-mails from various parts of Asia, and I was approached at conferences by people wishing to share their views. None of my (presumably) more high profile conventional research had elicited such strong responses from the scholarly community. As the paper treads on the toes of some people in the academic establishment, I did not expect that the responses would be so overwhelmingly supportive (Au, 2007; Puffer & McCarthy, 2007; Yang & Terjesen, 2007).

This not withstanding, some of my arguments need clarification, and certainly the data that I presented have limitations. I am grateful for the opportunity to provide a rejoinder to the debate. Given time constraints, I have not been able to present new data, or a more sophisticated index[1] in this rejoinder; suffice it to say that my arguments are based on conversations with a large number of scholars over many years, not only the quantitative data that I presented. I am confident that in our dynamic community someone will soon present and analyze more sophisticated data—and time trends.

My rejoinder thus focuses on the conceptual issues arising with respect to context in management research. In particular, I argue that context is a crucial variable to explain management behavior, and thus challenge management scholars to devise new research strategies to enhance our understanding of the contextual boundaries of our knowledge. Moreover, to make theory relevant for practice, it is important to identify the boundary conditions of theories, and the moderating contextual variables.

[1] The main methodological concern relates to the way that I have chosen to illustrate US influence on top management journals. Journals do not publish much information on their editors. However, it would be desirable to use multi-items indices including notably the place of graduate education—as these tend to be the formative years of a scholar. I do not expect substantively different results. If other indicators such as passport or place of birth are taken into account a more diverse picture is likely to emerge. For example, the measured influence of Hong Kong and France may decline as many scholars at for instance HKUST or INSEAD have moved to the territory only late in their scholarly life.

The quest for general theory

Geert Hofstede (2007) outlines the implications of cultural differences, and makes the case that management practices are highly culturally embedded. Hence, practices in one culture may not have the same impact in another culture, and insights gained in one context are rather unlikely to be transferable to another culture. Hofstede (2007) thus reemphasizes his well known though oft-ignored “Cultural constraints in management theories” (Hofstede, 1993). If the effects of management practices are in fact so diverse, this raises the question of whether management researchers’ quest for general theories (Huff, 1999) is indeed justified!

I would agree with critics of my paper, such as Ramaswamy (2007), that a general theory would be more desirable than a context-specific one. Yet, do we actually have theories in management that can justly claim to present ‘general theory’ in the sense that their insights would be valid anywhere in the world? Moreover, how can truly general theories be created – and how useful would they be for management practice?

To start with, we have to accept two realities. First, general theories tend to be of a very abstract nature, and they require operationalization to be applied in empirical work or to provide managerial advice. A good example is transaction costs economics (TCE), probably the most generally used theoretical framework within business research. The basic causal relationship stipulated by TCE holds in any context: agents prefer organizational forms that minimize transaction costs. However, transaction costs are caused by a variety of different influences that greatly vary across contexts. Thus, empirical tests and practical advice based on the same theory, TCE, may look different across contexts: In a low trust context with solid legal contract enforcement mechanisms we may advise businesses to write complex contracts, whereas in a high trust context we may advise businesses to leave their lawyers at home. Thus, TCE, properly applied, suggests where in the context to look for relevant variables, but it does not provide simple universally valid policy advice (Casson, 1993). Politicians in Eastern Europe following their Western advisors and rushing into mass privatization found this out the hard way (Meyer & Peng, 2005; Puffer & McCarthy, 2007).[2]

Second, most contributions in management journals—including top journals, such as the *Academy of Management Journal*—are in fact context-specific theories that have never been tested in a context other than the authors' original field. This is a fairly wide-spread problem. Most scholars acknowledge that their empirical findings need replication before generalizing to other contexts (except research targeted at domestic audiences only, or in outlets with an ethnocentric perspective). For example, Lyles and Salk (2007), upon receiving the *JIBS* decade award for Lyles and Salk (1996), acknowledge that their paper on knowledge transfer in Hungarian JV has been widely cited, yet their framework has not been tested elsewhere to verify that their findings actually hold true elsewhere. In fact, I believe that the very specific context of their data explain some of their more surprising results (Meyer, 2007).

Tsang and Kwan (1999) outline the methodologies for replication studies, yet their advice has failed to generate substantial flows of research. I am aware of only two replication studies in international business, respectively by Makino and Neupert (2000)

[2] Another good example is the OLI framework. The concepts underlying the O, L, and I factors are very broad and – suitably adapted - can be applied to a wide variety of contexts and processes. Thus, OLI can perfectly well explain the emergence of Asian multinationals (Dunning, 2006; Narula 2006), though some scholars prefer to develop new or modified theoretical frameworks that allow them to obtain a sharper focus on the specific issues that they identify as relevant in a particular context (Mathews, 2006).

and by Peng, Zhou and York (2006). Why are replication studies so rare? The answer may in part be to adverse incentives by journals eager to publish original theoretical contributions (Au, 2007; Yang & Terjesen, 2007). But, equally important, good replication studies are hard to do.

The lack of construct equivalence inhibits exact replication as concepts do not travel. For example, ownership categories used by local statistics do not correspond to those used in other countries, such that it is essential to identify the ultimate owners (Estrin and Wright, 1999; Delios, Wu and Zhou, 2006). European research has long established that ownership patterns vary widely across countries, and owners vary in their motivations and thus the strategies that they would advocate (Pedersen & Thomsen, 1997; Mayer & Whittington, 2004), and the same applies in Asia (Peng & Delios, 2006). The Anglo-American pattern of disconnected owners focused solely on financial performance may be more of an outlier than the pattern in, for example, India. Tests of propositions derived from Anglo-American thought are bound to confirm these differences, but this result is obvious for those familiar with the local context. Replication is important, but it is crucial to investigate the equivalence of constructs and moderating contextual factors to establish why results vary, and what additional variables are important. This is why we need indigenous research to advance our knowledge (Tsui, 2004; Farh, Cannella & Lee, 2006; Tsui, Nifadkar & Ou, 2007).

However, we cannot blame authors alone for inappropriate generalizations; readers and users are also at fault. Scholars usually acknowledge the limitations of their studies, yet they may find themselves cited *as if* they had found some general truth. The generalizing style of writing literature reviews or theoretical section in mainstream management journals (and editors' pressure to be short and 'to the point') leads us to jump over the need to reflect whether insights from earlier work can reasonably be assumed to be valid in whatever context we are analyzing.

What context?

In Meyer (2006), I focused on the Asian context and country-specific issues because my main motivation has been to challenge scholars working in and on Asia. Some commentators replied that similar problems emerge elsewhere—for example in Australia (Yang & Terjesen, 2007) and in Russia (Puffer & McCarthy, 2007). Naturally, I agree. I have worked on Central and Eastern European (CEE) topics for many years, where I perceive this issue as less pronounced as a number of highly regarded European research outlets welcome research on CEE business. Such research has made substantive contributions in management and international business, for instance, by developing new explanations for the interaction of institutional frameworks and different aspects of business strategies and operations (Meyer & Peng, 2005).

A different line of critique emphasizes that contexts also vary along a number of other dimensions, including industries, time, and regions or provinces within a country. Again, I am happy to agree, and there is short remark on this in Meyer (2006). Indeed all these aspects of context deserve more attention though it would be unfair to say that no one has noticed it because some studies indeed look within-country variation or inter-temporal variation, not to forget a vast literature in economics on industry characteristics and firm performance. With respect to the unit of analysis, I thus disagree with Hofstede (1993, 2007) who focuses on nation states as unit of analysis with little attention to the implications of intra-country variations, and the evolution of culture over time. My views are closer to Taras and Steel (2006) who trace changes of culture over time, and to Fang (2006) who analyses culture as a dynamic construct. In fact, I believe that management research can be pushed forward by taking ideas about contextual variation developed across one dimension and investigating these in other dimensions.

Why local research?

Another line of critique of Meyer (2006) questions why it should be local researchers doing Asian research, while numerous Europe- and North America-based scholars conduct research on China. Moreover, academics are proud of their freedom to choose their research topics according to what they personally consider most interesting, which may or may not be local. From an individual perspective, this is a very sensible avenue to design research projects, and it is indeed important to sustain the persistence to implement a project over often many years.

However, as a collective phenomenon, it is problematic. First, universities have a role in society that goes beyond the sum of their faculty members' citation indices. They are the prime source of knowledge of a nation, and thus inform decision makers in business and politics. Au (2007) points out that the Hong Kong authorities drew lessons from the consequences of a lack of intimate knowledge on important local issues in the local scholarly community, and thus modified their incentive scheme. In principle, I believe this is a sensible policy.

Second, I am concerned that scholars selecting research fields by the availability of secondary quantitative data may lack the understanding of the context in which these data were collected, which poses a major barrier to interpreting the results. This affects Asian scholars crunching US numbers just as well as European or American scholars analyzing an Asian context based on numbers only. Taking the US context as an example, data are readily available as base populations tend to be large and statistical data are *comparatively* reliable. Yet the US context is highly idiosyncratic, not only because of the size of the domestic market. Thus, it allows little generalization. Moreover, official data have their flaws too, which is an often under-rated problem of business research (Ramaswamy, 2007).[3] Understanding the nature of the data, say local accounting practices, is an important aspect of contextual knowledge that researchers need to make sense of results obtained using quantitative methods of analysis.

Local knowledge is potentially an asset of a social science researcher, as is the broader perspective that sets a particular country in context, and the knowledge of the literature in the field. A single scholar is unlikely to understand all aspects of a complex research issue. Thus, we should work with suitable collaborators and co-authors. Moreover, researchers should use their local knowledge to ensure their methods are valid (Farh et al., 2006), and to provide meaningful interpretations of their results.

On publishing indigenous research

In Meyer (2006), I discuss constraints perceived by scholars pursuing indigenous research agendas with respect to the opportunities to publish such research. These arguments have been made by many people I have talked to during my years in academic life, and they are expressed also by Au (2007) and by Yang and Terjesen (2007) from,

[3] Fortunately, management scholars, including reviewers and editors, take a more balanced view of the relative merits of survey and ‘official’ data than for instance (mainstream) economics journals.

respectively, a Hong Kong and Australia perspective. Ramaswamy (2007) disagrees with these aspects of my arguments in this second part of the paper. Space limitations prevent me from taking up all his arguments, though many relevant explanations are in fact in the first part of this paper where I discuss the complementary contributions of different types of research. Let me just pick up two points.

First, Ramaswamy suggests a manuscript flow analysis to assert whether or not a given journal has a bias against Asian research. Similar lines of argument are frequently adopted by journal editors. However, this suggested research methodology would only identify whether editors comply with their own stated objectives. It would not address the more fundamental issues.

- This methodology assumes that editors receive a random selection of good research—and that authors do *not* adapt their research design or their writing style to the *perceived* expectation of what the journal wants. Anyone who has ever listened to the questions in a meet-the-editor session or a doctoral consortium knows that researchers often adapt study designs and writing styles to what they think the journals want. In fairness to editors, I should add that some editors—even those in North America—aim to broaden their scope, but are constrained by reviewers who take a narrower view of what research deserves to be published.
- The proposed methodology assumes that objective criteria exist by which the quality of research can be assessed. Anyone who has tried submitting a piece of work in a different discipline will have encountered this problem, as I did with economics journals. Many techniques common in management journals are not

> accepted in economics journals, while many economics papers even from the top journals would fail to meet what management scholars consider basic construct validity tests. More specifically, evaluating an empirical paper always involves a weighting between different methodological weaknesses—no paper has a perfect methodology. For example, how important is it if authors fail to contextualize their methodology appropriately?

Second, Ramaswamy's (2007) concluding remark dismissing the entire discourse on appropriate methods with the quote "a bad carpenter always complains about his tools" is offending, to put it mildly, to a lot of good researchers. Readers may form their own opinion.

From researcher to scholar in the age of globalization

Globalization is a matter of life for businesspersons and for researchers, yet we only gradually come to understand its consequences for how we do our research. I have come to believe that central themes in the global management research agenda in the near future will be variations across contexts, and interactions between contexts. Culture and institutions have emerged as central themes in this debate, but they are not the only ones.

To push these research agendas, however, requires not just skills in empirical techniques and abilities to write for reviewers. It requires courageous scholars to grasp the broader issues, conceptualize key variables, and take risks in pursuing their agenda, undisturbed by certain reviewers with narrower intellectual horizons.

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