



Rigor and Relevance in Asian Management Research: Where Are We and Where Can We Go?

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Abstract. Has our collective research effort focused on management in Asian contexts addressed salient questions and produced useful results? Where are we in terms of deepening and broadening our understanding of the antecedents, manifestations and implications of phenomena that are relevant in this region? What contributions have we been able to make to general theory and practice? Where should we be moving in terms of research focus, methodologies and contributions? This paper draws on 840 articles from 30 journals to assess the state of management research in Asian contexts. The basic conclusions are that too much of the research effort has been limited to simplistic comparisons, correlational analyses providing no insight into underlying processes, and skewed, idiosyncratic sampling. The result has been a lack of theory development and contribution to conceptual discourse beyond an audience specifically interested in Asia, with little relevance for management practice. This analysis points to clear recommendations for increasing both the rigor and relevance of this collective research effort, while at the same time acknowledging the considerable institutional and cognitive barriers to moving forward.

Keywords: review, research methods, comparative research, Asia

Introduction

Before proceeding with the massive task of reviewing twenty years of research on Asian management, it is necessary to acknowledge the inherent limitations of such an undertaking. Fundamentally, the task may be questioned because the term “management in Asia” implies such an incredibly broad, multidimensional space that it is debatable whether it is coherent enough to qualify as a meaningful domain, in spite of its salience to practitioners. As a set of issues, “management” is itself broad and with diffuse and overlapping sub-boundaries. As a region, it comprises vast and diverse contexts defined in terms of geographical and political units, cultural traditions, economic systems, and related social institutions.

On the other hand, it is useful for anyone to have some idea where their own research sits relative that of the large and growing community that shares a fundamental interest, and often personal relationship beyond research, in this region. With this noble aim, and in spite of all of the dangers of simplification, generalization, subjectivism and pretension of which I will be to varying degrees guilty, I present my own interpretation of the state of our collective effort to understand management phenomena in Asia.

To do this, several research assistants and I have gathered material selectively in terms of both source and content, resulting in a final selection of 840 articles found in 30 journals over the period 1980–2000 that directly address managerial issues in one or more Asian contexts (see Table 1 for a list of journals and number of articles in each).

Table 1. Journal and article counts.

Journal	Social sciences citation index impact factor 2000	Articles 1980–1990	Articles 1991–2000	Total articles 1980–2000
<i>Academy of Management Journal</i>	2.375	14	28	42
<i>Academy of Management Review</i>	3.912	12	4	16
<i>Administrative Science Quarterly</i>	3.333	8	9	17
<i>American Journal of Sociology</i>	2.829	0	3	3
<i>American Sociological Review</i>	3.255	0	10	10
<i>Asia Pacific Journal of Management</i>	n.a.	68	77	145
<i>Group and Organization Management</i>	0.590	3	1	4
<i>Human Relations</i>	0.832	7	29	36
<i>Human Resource Management Journal</i>	1.268	0	1	1
<i>International Journal of Conflict Management</i>	0.633	0	3	3
<i>International Journal of Human Resource Management</i>	n.a.	0	128	128
<i>Journal of Applied Psychology</i>	1.730	3	13	16
<i>Journal of Applied Social Psychology</i>	0.722	1	12	13
<i>Journal of Cross Cultural Psychology</i>	1.218	2	7	9
<i>Journal of Economic Behavior & Organization</i>	0.567	3	8	11
<i>Journal of Human Resources</i>	1.235	0	1	1
<i>Journal of International Business Studies</i>	1.012	10	61	71
<i>Journal of Management</i>	1.235	4	5	9
<i>Journal of Management Studies</i>	0.646	3	22	25
<i>Journal of Occupational and Organizational Psychology</i>	0.466	7	7	14
<i>Journal of Organizational Behavior</i>	0.808	2	15	17
<i>Leadership Quarterly</i>	0.830	1	8	9
<i>Management Decision</i>	n.a.	3	2	5
<i>Management International Review</i>	n.a.	51	66	117
<i>Management Science</i>	1.011	5	10	15
<i>Organization Science</i>	1.052	0	7	7
<i>Organization Studies</i>	0.818	9	11	20
<i>Organizational Behavior and Human Decision Processes</i>	1.200	0	8	8
<i>Personnel Psychology</i>	1.733	1	2	3
<i>Strategic Management Journal</i>	2.531	10	52	62
Total		227	613	840

In spite of the danger of overlooking important work appearing in books and edited volumes, only journal articles were included simply to reduce the absolute number of works to be reviewed to a more practical level. Furthermore, practitioner-oriented journals such as *Harvard Business Review*, *Sloan Management Review* and *Long Range Planning*, were excluded. We have assumed that much of the work appearing in edited volumes and practitioner-oriented journals is based on academic research by the authors and has originally appeared in at least some of the academic journals included in this review. Next, six management scholars were asked to identify which peer-reviewed academic journals, out of an original list of 57, would be relevant in terms of content and quality, given the purpose of this review. Their ratings were quite similar, and resulted in a list of journals and number of articles considerably greater than other reviews with a regional focus (e.g., Peng et al., 2001; Li and Tsui, 2002).

As for content, we have included only those articles that clearly address managerial issues falling under the broad categories of organizational behavior or strategy. This focus on managerial issues first led us to exclude a number of discipline-based journals that have included articles reporting empirical results relevant to the region, but without a clear linkage to managerial issues. Based on this same criterion, we excluded some papers in some of the source journals that we judged to be directed primarily towards a discipline audience in, for example, psychology, sociology and economics, and not explicitly linked to managerial issues. A small number of journals did not have any relevant articles, so these journals were excluded from the final list in Table 1.

Almost all of the articles report findings from empirical research on phenomena in the region or involving individual or organizational actors from the region. However, a few are conceptual, drawing on Asian contexts to discuss issues that are not limited to the region (e.g., Peng and Heath, 1996; Sinha and Cusumano, 1991; Sullivan, 1983; Easterby-Smith and Malina, 1999).

This process has left us with a large but admittedly incomplete sample of articles. Still, it does provide a representative sample from which we can identify meta-trends in the research questions, methods and contributions of research on management in Asia. Given the breadth of topics falling within this area, I do not attempt to consolidate findings in particular research areas. Furthermore, this review does not attempt to identify the “top” individual or institutional contributors to research in this area, or judge the “impact” of particular articles.

Because this review includes a wide range of studies in terms of levels of analysis, phenomena and basic questions, I use a generic framework for discussing research questions and study design. In the following comments, I use the terms “antecedents”, “manifestations”, “performance implications”, and “process” to compare the types of explanation that particular studies or groups or studies are attempting. Antecedents refer to the relatively stable characteristics—such as personality traits at the individual level of analysis, or resource positions for organizations—that are often used to distinguish among actors and, more importantly, are assumed to precede particular manifestations. They are often used in regression analyses to “predict” manifestations; specifically, individual, group or organizational decisions, actions or practices. These manifestations may, in turn, be linked to outcomes—satisfaction, effort, profit, etc.—that serve as indicators of individual, group or organizational performance. Of course, performance implications in one study may be antecedents in other

studies, depending on the research question. Furthermore, antecedents at one level—for example, organizational climate or the national culture—may be used to explain manifestations at another unit of analysis—such as individual reward-sharing behavior—that should, of course, be aggregated to the same level of analysis (organizational or national population, respectively). Finally, processes are the cognitive or causal mechanisms that link antecedents and manifestations or manifestations and performance implications.

This nomenclature allows a comparison of research questions setting aside differences in level of analysis. It also allows for the range of research designs, whether process or variance studies, and comparisons across groups of particular variables or relationships among variables. Importantly, this differs from the traditional 2-way distinctions between cause and effect, or predictor-outcome, that imply a particular relational order, even in (strictly speaking) correlational analyses, such as multivariate regression using cross-sectional data. These categories are more useful in describing the range of research questions and designs found in the articles included in this review.

This generic framework also helps describe two fundamental weaknesses in the research on management in this region that will be discussed in more depth in later sections of this review. First, little of the research in either OB or strategy provides insights into the processes that link antecedents, manifestations and performance. We are generally left with cross-sectional correlations and differences within and among samples, but no grounded insight into the processes that give rise to these differences. Second, this framework helps describe a symmetrical weakness of OB and strategy research. As discussed in more detail below, OB researchers have generally limited their studies to measures of (usually cognitive) antecedents and (cognitive or behavioral) manifestations and not addressed performance implications. Strategy research, in contrast, has focused on (nearly exclusively behavioral) manifestations and linkages with performance, but is largely silent on the cognitive antecedents that give rise to the observed manifestations and implications. Although this critique may be relevant beyond research focused on Asia, it is particularly salient in this context because so much of the research is comparative and, unfortunately, generally does not tell us whether the differences have performance implications (the case of OB) or from what cognitive sources the differences in manifest behavior arise in the first place (the weakness of strategy research).

Looking back: 20 years of running in place

Despite the breadth in terms of phenomena, theoretical foci and approaches, and objectives of research and researchers in Asian management, it is still possible to discern general trends and tendencies in this domain. I have organized these findings under three, obviously linked, categories roughly describing the stages in research: (1) the questions being asked and the researchers involved, (2) methodologies and related epistemological issues, and (3) the contribution of research results.

Questions and contexts

Questions of similarities and differences—comparative research—are clearly the dominant concern of researchers focused on Asia. Of the 840 articles included in this review, 534 (64%)

included samples from 2 or more national or regional contexts. In Tsang and Kwan's (1999) categorization of replication studies, the majority of these are "empirical generalizations" or "generalization and extensions." In a large subset of these, the researchers are asking whether the constructs and relationships they find among variables in a particular Asian context are similar or different to those reported (in separate studies) from other, usually U.S., contexts. In another subset, the comparison is made between two or more samples within the same study.

While identifying differences among populations of individuals, groups and organizations is certainly important, most of the studies are less than satisfying in terms of insights into the sources of those differences or their performance implications. Redding's (1994) fundamental critique of comparative research is still painfully accurate of such research in the Asian context up to now:

The standard report says in effect, 'Managers in country A believe this; managers in country B believe that. They are different. Isn't that interesting?' A variant is 'We think this is why there are differences but we didn't look at that'. Another is 'We think these are the implications for practice, but more research is needed on that'. (p. 348)

Nor is such reporting limited to comparative research at the individual level or to organizational behavior phenomena. Strategy and firm-level comparative researchers typically relate differences in behavior (associated with organizations from different cultural or national contexts) to differences in performance. However, while discussions of the sources of those behavioral differences are often explicitly or implicitly linked to cultural or cognitive differences among the managers of those organizations, they are seldom based on systematic empirical research incorporated into study designs.

The bulk of comparative research in Asian contexts—at all levels of analysis—also reveals a fundamental weakness related to explanation. Comparisons have been largely limited to what I have termed antecedents (for example, cognitive orientations, values, organizational characteristics), manifestations (individual behaviors, choices, organizational actions) and performance. Nearly all of the research questions are subsumed under "What are the differences in antecedents, manifestations and/or performance among these actors?" Few studies have compared the processes giving rise to or linking antecedents, manifestations or performance; i.e., "How did these differences arise" or "How do antecedents give rise to manifestations, or manifestations to performance?"

Some notable exceptions to this trend identified in the course of this review include Wiersema and Bird (1993); Xie, Song and Stringfellow (1998); and Amba-Rao (1994). These and a few other studies in this review show how different processes can lead to the same antecedents having different manifestations, or different manifestations having different performance implications, in different contexts.

Another clear trend in comparative research is the de facto investigation of similarities and differences between particular Asian contexts and actors and those of the U.S. As Table 2 shows, of the 341 2-country comparative studies, 50% were between an Asian context and the U.S. Furthermore, although not obvious from this table, many of the studies included as single-country studies are replications citing results from U.S.-based studies. Even among studies involving 3 or more national contexts (Table 3), the U.S. is the primary reference.

Table 2. Distribution of one- and two-country or region studies.^a

	China	Hong Kong	India	Indonesia	Japan	S. Korea	Malaysia	Philippines	Singapore	Taiwan	Thailand
China	76	3			2			1	2	2	
Hong Kong	3	34			2				2		
India			29								1
Indonesia				3							
Japan	2	2			100	6	1	2	5		
Korea (South)					6	28			1		
Malaysia					1		8		3		
Philippines	1				2			1			
Singapore	2	2			5	1	3		48	1	
Taiwan	2								1	10	
Thailand			1								7
Australia			1		4				1		
Canada	3	1	2			1		1			
USA	19	13	9	2	109	7			4	1	
Europe	3				4					1	
France											
Germany	1		2		3						
Italy					1						
United Kingdom	3	1	2		10					1	
Other	7		1		4				3	1	
Subtotals											
1-country studies	76	34	29	3	100	28	8	1	48	10	7
2-country studies	46	18	18	2	153	15	4	4	22	7	1
Country total	122	56	47	5	253	43	12	5	70	17	8

^a604 unique studies (280 single-country studies, 324 two-country studies).

Table 3. Bilateral comparisons within studies including samples from 3 or more countries or regions.^a

	Asia, ASEAN	China	Hong Kong	India	Indonesia	Japan	S. Korea	Malaysia	Philippines	Singapore	Taiwan	Thailand
Asia, ASEAN												
China	4		2	4	1	3	2	2	1	1	1	1
Hong Kong	2	23		2	2	12	10	4	2	4	10	1
India		4	2			1	3	1	6	2	3	1
Indonesia	1		2			1	2	5	4	2	1	5
Japan	3	9	12	1	1		19	1	1	6	15	2
S. Korea	2	4	10	3	2	19			1	5	12	1
Malaysia	2		4	1	5	1		4	4	3		3
Philippines	1	3	2	6	4	1	1	4		2	1	5
Singapore	1	1	4	2	2	6	5	3	2		2	2
Taiwan	1	10	10	3	1	15	12		1	2		1
Thailand	1	1	1	1	5	2	1	3	5	2	1	
Vietnam		1										
Australia		2	4		1	2	1	1		2	1	1
Canada		2	3			4	1			3	1	
USA	10	32	16	8		85	16	1	3	8	16	4
Europe	8	4				26		1			1	
France		2	1	3		2					1	1
Germany		5	3	5		14	4			1	4	
Italy												1
UK		3	4	2		18	4			2	2	
Other	9	8	7	6		29	11				7	2
Subtotal	45	118	110	47	24	250	96	26	33	46	89	32

^a458 bilateral comparisons (not unique studies).

The justification for comparisons with individuals and organizations in the U.S. usually reduces to no more than “We want to know how results from (*the Asian context*) differ from results from the U.S.” When conceptual rationales are used, they are usually superficial and equate national boundaries with cultural boundaries, a weakness identified by Child (1981) and, twenty years later, still ubiquitous. This is particularly common in studies in which individualism and collectivism are central concerns; the attitudes, values and behaviors of the U.S. sample serve as the individualist standard, and those of any Asian sample as the collectivist standard.

While comparisons with U.S. findings and contexts may reflect relevant and interesting questions for a particular study and from an individual researcher’s point of view, at the aggregate level we are then left with sparse insights into differences between Asian and non-U.S. contexts. Ironically, we also have few insights into similarities and differences among Asian contexts. Even the multi-country studies (involving three or more countries; 23% of the 840 articles included in this review) are usually quite limited in scope; 52% of these (or, 12% of all articles) included only 3 countries. Researchers who do have the resources and organization to undertake large-scale multinational comparative research, therefore, can make major contributions to our understanding of national variation and provide finer-grained analyses of contextual contingencies. Examples include articles generated from studies by Hofstede (1980) on national culture (e.g., Hofstede, 1984) and House and associates (2001) on leadership (e.g., Den-Hartog et al., 1999).

Besides comparative questions

In contrast to the preponderance of comparative research questions investigated in Asian contexts, two types of questions have received little empirical research attention. The first are what could be categorized as “interface” questions involving phenomena in which two or more individuals, groups or organizations differing on some dimension interact. Relatively few studies included in this review address the major problems at that interface where actors with different values, attitudes, perceptions and behaviors must interact. It is both theoretically and practically important to understand what happens to the cognitive antecedents, behavioral manifestations, performance outcomes, and processes linking these when different actors meet or when actors encounter new contexts. What drives the outcome to asymmetrical adaptation, combination or creation of alternative structures and processes?

A number of studies identified in this review have addressed such questions, such as Brannen and Salk’s (2000) work on negotiated culture in joint ventures, Johnson et al.’s (1996) work on trust building between U.S. and Japanese firms, and Leung et al.’s (1996) analysis of interpersonal relationship quality in Sino-foreign joint ventures. Still, there is too little research addressing issues of major concern to the increasing number of firms and managers who must work at the “cultural interface.” The relative lack of attention to such managerially vital issues suggests that Boyacigiller and Adler’s (1991) general criticism that organization science has largely ignored the needs of managers is accurate for management research in Asian contexts.

The second type of question—important both theoretically and practically, and also largely unaddressed—is the degree of longitudinal stability in characteristics, relationships

among variables, and processes. The dominant assumption-in-use is that the “cultural” traits, attitudes, values, causes and effects are stable over time. If longitudinal change is addressed at all, it is usually no more than speculation to give the impression that the author is contributing to the “larger debates” of, for example, convergence vs. divergence. We find few researchers who have specifically asked and gathered the data necessary to address the question of which of these are more or less stable, and to what forces for convergence or divergence they are subject. Notable exceptions include Selmer and de Leon’s (1996) study of local employees’ work value acculturation within foreign multinationals, and Heuer, Cummings and Hutabarat’s (1999) study of cultural values in Indonesia.

National trends and “unique-ism”

The collection of articles also reveals extreme variation by national context in terms of level of analysis and empirical phenomena that have received researchers’ attention. This is particularly evident when one compares studies involving Chinese and Japanese contexts, which are the most common included in both single and multi-country studies. As Table 2 shows, of the 280 single country studies, 36% included Japanese samples and 28% Chinese. (Singapore and Hong Kong, the two smallest regions in terms of population, were the next most common contexts or sample sources, accounting for 17% and 12% of the remaining single-country studies, respectively.) Of the 324 two-country comparisons, Japan and China accounted for 47% and 14%, respectively.

Overall, the research in China has been dominated by 3 themes: (1) psychological characteristics and, in particular, *interpersonal* relationships, (2) enterprise reform and institutional change, and (3) multinationals in China. Nearly all of the comparative work has been psychologically-based and dominated by a concern with Hofstede’s individualism-collectivism dimension. Furthermore, there is a negative bias in the choice of topics and discussions; for example, *guanxi* make it difficult to do business in China, and Chinese organizations are managerial and economic disasters.

Research on Japan, in contrast, has focused on (1) *interorganizational* relationships, (2) “successful” practices (“Japanese management”, HRM, innovation and production) and their transferability, (3) Japanese multinationals, and (4) managerial attitudes and values. The dominant focus has been on organization-level phenomena and strongly influenced by sociology and economics. Although also a collectivist society, the individualism-collectivism distinction has not been a major focus of research. Furthermore, there has been a positive bias in research on Japan, with a focus on practices and values that are worthy of emulation. These are presented as sources of higher performance, even if not necessarily easy to replicate outside Japan. Ironically, even as the Japanese economy and Japanese firms have largely stagnated during the entire 1990s, there has been no upsurge in studies addressing sources of these weaknesses.

Studies in the two contexts do share a common tendency that could be termed “uniquism”, a desire to present the phenomenon being studied as unique to that context. In some cases, uniqueness has been reinforced by the use of either Chinese or Japanese terms to describe the phenomena (e.g., *guanxi* and *renqing* in China, *keiretsu*, *nemawashi* and *ringi* in Japan), or the association of particular English terms to these national contexts (e.g., “face” in China,

“lifetime employment” in Japan) with the implicit message that these are rare or even not found in other contexts. While such terms may serve as convenient shorthand to refer to complex phenomena particularly relevant in these contexts, the danger is that they may foster conceptual parochialism and create cognitive barriers to seeing relationships with similar phenomena in other contexts. Seldom do researchers who invest in such terms as theoretical constructs critically test whether they belong to “universal” or context-specific theories, as suggested by Cheng (1994). This tendency is exacerbated by dominant epistemological concern in comparative research for finding statistically significant differences among groups. The result is a flood of studies reporting differences, but few identifying conceptual and functional equivalence across diverse contexts. The balanced query, “how are we different, how are we the same”, remains only half-investigated.

Researchers

Who is contributing to research on management in Asian contexts? There are, of course, two caveats that must precede my comments. First, they are based on the English-language articles gathered for the purpose of this review. As a result, the extensive work reported in other languages is not included. Furthermore, I do not have the necessary data on researchers (background, citizenship, etc.), and it is beyond the scope of this review, to fully discuss the issue of who is an “Asian” or “local” researcher. The complex combinations lead to variations that make simple categorizations impossible. For example, how do we categorize an Indian-born, U.S.-educated and Singapore-based researcher, or a Chinese-born researcher who has worked for 20 years in the USA and now resides in Hong Kong, or a Japanese-born researcher who lived abroad most of his adolescence and completed a PhD in Canada but is now based in London?

In spite of the limitations and gross generalizations that are inevitable, several trends concerning who is doing what kind of research in Asia do seem clear. Overall, among “local” researchers (based simply on surnames and affiliations), and reflecting the contexts dominating studies in this region, Indians, Japanese, Korean, ethnic Chinese and Singaporeans are making substantive contributions to the research included in this review. (Research in other Asian contexts, as discussed above, is too limited to make statements about “trends”.) Among these groups, however, there are striking differences. Research involving Indian contexts—both single-country and comparative studies—is unique in that it is completely dominated by Indian researchers, although they are not necessarily based in India institutions. Research in Singapore is also dominated by Singaporean and Singapore-based researchers, although not as completely as that of India.

While we find a more equal representation of local and non-local authors in Japanese, Korean and Greater China (PRC, Hong Kong, Taiwan) studies, there are very different trends among the researchers and types of studies involving each context. Of the studies involving Korea, both local and non-local researchers are involved in both single (i.e., Korea-only) and comparative studies. Almost all Japanese researchers, however, have limited their studies to domestic (i.e., Japanese) contexts, and as a group have contributed little to comparative work at any level or to the large body of work (reported in English, at least) on Japanese foreign direct investment; for an exception, see work by Makino (e.g., Makino and Neupert, 2000;

Makino and Delios, 1996). Ethnic Chinese researchers, in contrast, are heavily represented in both single- and multiple context research at all levels of analysis involving Chinese samples; i.e., the PRC, Hong Kong, Taiwan and Singapore. On the other hand, there are disproportionately fewer Chinese researchers involved in comparative work involving Asian samples outside of these “Greater China” contexts and the U.S. or Canada; for example, non-Asians have undertaken nearly all of the comparing phenomena across Asian contexts (i.e., beyond intra-Greater China comparisons) or between Chinese and European samples. (Although, can be seen from Tables 2 and 3, there are few of such comparisons taken up by anyone.)

Finally, scanning the references of the articles included in this review also suggests a clear trend in who is contributing—or, more accurately, who is not contributing—indirectly to research in Asian contexts. Specifically, there is a dramatic knowledge trade flow imbalance between English and non-English domains. A casual review of local (i.e., non-English) language journals and articles clearly shows that these researchers and studies are drawing on English-language sources. In contrast, few English-language studies include references to non-English language sources except as sources of data. This suggests that researchers and their analyses are proceeding with little input from local-language studies or, for that matter, any non-English studies.

This tendency should be cause for concern because it likely results from a number of intellectually dysfunctional phenomena. For example, language may be acting as a high barrier to knowledge flows in management academia; there may be an actual scarcity of relevant or adequately rigorous work reported in other languages; or work in other languages lacks legitimacy in the eyes of English-language journal reviewers. The latter possibility, if true, is particularly troubling, since it reflects a faulty assumption that any results of “value” would be reported in an English-language source (and, conversely, assuming that if it does not appear in English is it not worth noting). Such a view is a dysfunctional combination of arrogance and ignorance.

Methodologies

The dominant methodologies of the studies included in this review reflect the unadventurous approaches and “comfortable conformity” that Redding (1994) and other reviewers of comparative management researchers have attacked, with little impact, over the last two decades (e.g., Adler, 1983; Boyacigiller and Adler, 1991; Whitley, 1984). Specifically, the studies are primarily variance studies, using quantitative data to test hypotheses relating variables within a sample or to find statistically significant differences across samples. This basic approach is not necessarily inappropriate, but is rather a reflection of the limited types of questions that have been investigated. Fundamentally, the dominance of such approaches underscores the dominance of logical positivist approaches to research in this area, with ethnographic, interpretive and process-oriented research relegated to the methodological fringe, along with associated research questions.

In terms of basic design, the majority of studies fall into four basic categories reflecting the basic questions that have been asked: local studies, replications, in-study comparisons, and interactions. Local studies borrow methodological and conceptual tools from

“mainstream” research to study phenomena in a particular Asian context. Their main interest is in understanding the phenomenon without particular emphasis on similarities or differences with findings in other contexts.

The majority of studies, however, are comparative and some form of replication. The researchers borrow methods and constructs from “mainstream” studies, but their main objective is to uncover similarities and differences between the sample investigated in the study and findings from samples from other, primarily U.S., contexts. Value-added replications include additional constructs that the researcher believes are salient in the focal Asian context and either overlooked or irrelevant in other, again primarily U.S., contexts. Many of these studies compare results from an Asian context with prior results from other contexts, while others analyze samples from two or more contexts within the same study.

Certainly, given the U.S. dominance as the training ground for many Asian-based or Asia-focused researchers, perhaps it is reasonable that the U.S. serves as a common reference point. This does not, however, justify the absence of or only superficial justifications given for including U.S. samples for comparison. Certainly, given the differences along many cognitive, social and other dimensions, comparisons with U.S.-based samples are quite likely to result in statistically significant differences between groups and relationships among variables. Should we be surprised that there are differences? To the promise of publishable results—we would be impressed by either similarities or differences across such “different” samples—are added the private whispers of researchers interested in this region suggesting that U.S.-based reviewers and journals will only be (or are more likely to be) interested in a comparative article if the U.S. is included as a sample.

The unhappy result of these factors is that we know little of the variations in the same dimensions and relationships among variables across Asia or between Asian and non-U.S. “western” contexts. Latin America, Central and Eastern Europe, Africa and the Middle East essentially do not exist in the world of Asian comparative management research. A few researchers have undertaken comparative work including these overlooked regions such as Hungary (Child and Markoczy, 1993), Russia (Ralston et al., 1995), Brazil (Fleury, 1996) or Sweden (Cole, 1985). Other researchers, with access to considerably greater financial and collaborative resources, have undertaken large-scale studies including dozens of national contexts (e.g., Den-Hartog, 1999). However, these are exceptions and, without a collective effort to fill comparative holes, our understanding of both intra- and inter-regional similarities and differences will become deeper and deeper for only a small subset of contexts.

Another weakness in comparative study design emerging from this review is the use of single samples within the national contexts being compared. By design, the researchers are not addressing the important question of whether variation within a national context is more or less than between national contexts (Au, 1997). This would not be an issue if the researchers were limiting their discussions and conclusions to the particular type of actors (for example, CEOs of local firms, or domestic banks). However, often these specific sub-groups are used to make country-level statements, a weakness identified by Child (1981) who pointed out that national borders are too often assumed to represent cultural boundaries. Thus we find, for example, that a sample of students in Hong Kong are used to make statements about “the Chinese”, or the small number of listed companies in Japan represent “Japanese firms”.

The research on Asian contexts reported in the journals included in this review has not escaped the restrictive and dysfunctional trend in management research in general towards singular reliance on quantitative and correlational analytic methods. Of course, these methods are appropriate for answering the simple question of whether two or more variables are associated within a particular sample or, in comparative research, whether that association is consistent across contexts. The major weakness, however, is that such methods cannot provide insight into processes linking these variables, or provide simple statistical measures of complex and often endogenous interactions among variables. As a result, the discussion sections of many comparative studies that rely solely on such analyses are no more than speculation on the reasons for different results across contexts. These studies entice us by documenting differences between groups, such as the same antecedents having different relationships with behavior, or the same behaviors having different performance implications. In the end, however, we are left unsatisfied and with no conclusions as to the processes leading to different outcomes.

Related to the lack of process studies is the paucity of interpretive approaches to relevant phenomena in Asian management contexts. Typically, studies will document differences in behavior or other manifestation, perhaps correlated with particular differences in antecedents. However, it is not clear whether the differences in behavior among these actors is an outcome of different decision processes or differences in perceptions of the “same” choices. Ironically, many researchers argue that replications or comparative studies involving Asian contexts are important because actors’ perceptions and cognitive or social processes could be fundamentally different “western” or “mainstream” actors. However, few of these researchers actually employ methods that go beyond testing correlations between variables. Those who have actually investigated (and reported) differences in processes linking antecedents, manifestations or performance outcomes are exceptional, and include Child and Lu’s (1996) study of investment decision-making in Chinese organizations, Varman and Bhatnagar’s (1999) study of grievance resolution, and Chikudate’s (1999) work on collective myopia.

Compounding the dominance of correlational analyses is the cross-sectional nature of most data gathered. Again, the lack of longitudinal designs is ironic, given that a common justification for undertaking a study in an Asian context is that the organizational environments have undergone dramatic changes in recent decades. Many researchers profess interest in the questions of convergence-divergence, emergence and adaptation, and the process and impact of institutional change. Indeed, these questions have major theoretical and practical implications. Too few, however, seem willing to undertake the longitudinal studies necessary to address them.

Another trend in the research on Asia is the obvious interest in multidimensional constructs, but the disinclination to clarify the dimensions or employ methods that allow the researcher to study them. Common examples include “Chinese family businesses”, “state-owned enterprise managers”, “networks” or “network relationships” and, perhaps the most pervasive and ill-defined, “culture”. Each of these terms comprises multiple constructs and, when associated with a particular group, implies specific states of particular variables. Therefore, we should expect a configurational approach (Meyer and Tsui, 1993) to the analyses of related phenomena.

For example, many researchers have investigated the conceptually interesting and practically important issue of the transferability of “Japanese management practices” or more specific practices such as quality control circles. These constructs, however, encompass a wide range of specific “components”, including both behavioral and cognitive processes and specific values and attitudes. To investigate related issues, a researcher must carefully describe these components as well as key interdependencies and contingencies, the essence of a configurational approach. By carefully describing components, interactions and contingencies, the researcher is able to do more than simply observe whether the practices exist or work successfully in other contexts. Rather, the researcher can assess the “fit” of this configuration and how it is (or must be) transformed when transferred. The researcher can go beyond the superficial question of whether or not the practice is transferred successfully, to a more sophisticated analysis of the process and nature of the transformation that takes place as a “bundle” of components, interactions and contingencies are adapted to a new environment.

Contributions

We can evaluate the contribution of management research in Asian context in terms of the intended or implicit objectives of the researchers, as well as impact of the results of those studies. For the first, the types of questions and methods dominating research in these contexts—correlational, quantitative and comparative studies—suggest that a fundamental objective is to uncover invariant laws relating specific variables. Whitley (1984) has ascribed this general trend to “physics-envy” that is all too common among social scientists, and an inappropriate, “pseudo-scientific” core paradigm (Redding, 1994:326). Numagami (1998) has similarly critiqued such objectives and the methods they dictate, saying that the effort to uncover such “laws” is a dysfunctional paradigm for conducting research in social science in general and management in particular. He argues that it establishes inappropriate criteria for case studies and qualitative research whose primary contribution is not to test hypotheses but to uncover processes, explore meanings and interpretations, and develop and refine constructs. Nooderhaven (2000) echoes this critique of the dominance of the quantitative variance study paradigm, arguing that it has inappropriately relegated qualitative methods to second-class status and legitimate only for “exploratory” work and early stages of a research project.

This objective—to uncover invariate relationships among variables—may be appropriate if all of the relevant factors that may affect the relationship are fixed or conditions under which the relationship holds can be completely specified, the conditions of a “closed system”. Both conditions are quite rare in empirical work based in real world settings, although perhaps more achievable in laboratory experimental settings. More commonly, the relationships are highly contingent on factors outside the scope of the study and control of the researcher. This is particularly the case in Asian contexts in which change is a major feature.

Ideally, a researcher will control for as many potentially important factors as possible. Given the open systems nature of the contexts in which most empirical studies take place, however, this is often infeasible. As a result, researchers must specify the boundary

conditions of a study that directly affect the generalizability of their conclusions. Regrettably, this is not the general trend of studies included in this review. In too many cases, there is an unjustified gulf between the scope of the construct that a researcher claims to be addressing (very general, such as “collectivist”) and the particular operationalization (undergraduate students at a Hong Kong university). The resulting overgeneralization and overstatement of conclusions is not only intellectually sloppy or even deceptive, it hinders our attempts to assess what we know and where we have gaps.

The unfortunate outcome of the focus on correlations and simply establishing differences among two or more groups on particular dimensions has been a lack of studies that use results to probe further, to ask why those differences exist or how they emerged. In addition to the sheer number of influences, answering such questions usually involves analyzing complex and often endogenous interactions among variables, mutual causation and feedback loops. Such conditions make analyses intractable given the limitations of existing statistical techniques. Nor have most researchers gathered the qualitative data necessary to describe processes that might lead to the results they found. As a result, most comparative studies are simply reports of correlations between variables and similarities and differences across groups, without any grounded insight into why those differences exist. This is particularly common in strategy research, in which linkages between behaviors and performance are meticulously documented but the question of why those behavioral differences arise in the first place (usually requiring insights into cognition and organizational processes) is left unaddressed.

What, then, has been the collective (or individual) impact of research in Asian contexts? One way to assess this is to gauge its influence on “mainstream” management research. At a superficial level, the sheer increase in the number of articles related to the region in non-regional focused journals (including most of the journals in this review) suggests that Asian-focused research is becoming an accepted part of journal research.

The conceptual impact, however, is not so clear. Have the constructs, relationships and findings from research in Asian contexts delivered on the promise to “inform” research and researchers who are not associated with the region? Indeed, this is a dominant conceptual justification that has been repeated by numerous scholars addressing the issue of Asian-based research and “western” parochialism (Shenkar and von Glinow, 1994; Biggart and Hamilton, 1992; Boyacigiller and Adler, 1991), as well as by scholars in their justifications for analyzing an Asian sample.

Although a thorough treatment is beyond the scope of this review, one could argue that the research has had a fundamental impact or, at the other extreme, little to none. Perhaps the strongest impact has been the steady stream of management research in Asia in which the nature of relationships among individual or organizational actors plays a central role. This focus on the nature and implications of relationships is a common focus in much of the regionally-focused research and across levels of analysis, reflected in the dominant themes and phenomena associated with networks, in- and out-groups, trust, cooperation and collectivism. Concern with one’s interconnections and dependence on others, often associated with collectivism or allocentricism, is considered a key cultural and psychological dimension. Relationships are also central features of organizational phenomena associated with the region, such as business groups, family businesses and collaborative action.

It is probably not coincidence that interest in relational and network phenomena among “mainstream” researchers and managers gained momentum during the 1980s as Japanese firms seemed able to gain competitive advantage through relationships among employees as well as between organizations. This presented a stark contrast to both conceptual and managerial assumptions that individuals and organizations should be viewed as atomistic economic actors. At a more general level, it suggested that more research and managerial attention should be directed towards issues of relationships among actors.

On the other hand, it is less clear whether research from Asian contexts has actually made an impact on non-Asian research in specific topical areas. In other words, are research and findings from Asia-based research cited by anyone besides those doing work in Asia? Indeed, have the researchers focused on Asia set for themselves the goal of theory-building rather than simply theory-testing, or to develop constructs that have general (and not just Asian) relevance? For example, anyone conducting research on leadership in China will cite other leadership studies and findings in Chinese or perhaps other Asian contexts, as well as “general” or benchmark findings primarily from U.S.-based research. It is not clear whether research on leadership in an American, European or other context would cite prior research from China or Asia. Although comparative research questions and designs would seem to make such references useful and even required, little comparative work is being done with non-U.S. “western” samples, as already discussed. Basically, Asia-focused researchers seem indifferent to pursuing comparative studies in contexts outside the U.S. (or even across Asian contexts, for that matter).

My analysis of the “impact” of research in Asian contexts is limited by the scope defined for this review. The articles included in this review were limited to those focused specifically on managerial phenomena, so they do not include works appearing in discipline-based journals or other outlets that may have impact outside Asian contexts. Aoki (1990), for example, has become a core reading in economics as an alternative theory of the firm. Similarly, this review has not included a number of influential books based on research in the region or including Asian context as part of larger studies, including work by Itami (1991), Nonaka and Takeuchi (1995) and Hofstede (1980).

Looking forward: Recommendations for change

What can we do to push research in Asia out of its current rut? The body of this review has identified significant aggregate shortcomings, together with a few examples of positive developments, of management research focused on Asia. Table 4 presents these as recommendations to move our collective effort forward.

Are these recommendations justified, useful or practical? Perhaps not surprisingly, I think the answer to the first two questions is definitely yes. Both individually and collectively, it is useful to occasionally turn our microscopes upon ourselves and critically assess both the nature and quality of our research focused on this region. Although pushing the limits of appropriate and practical breadth, this review has revealed clear “meta-trends” in research on management in Asia that are not limited to particular topics. Accordingly, while the recommendations I have made do not apply to all researchers, they do address the major dysfunctional trends and weaknesses identified in the course of this review. Incorporating

Table 4. Recommendations to increase rigor and relevance of Asian management research.*Questions*

Look beyond the currently limited ethnic, cultural and geographic boundaries. Expand comparisons beyond the U.S. and try to uncover similarities and differences with unexplored contexts. Discover Europe, Latin America, Africa . . .

Conversely, look more carefully within ethnic, cultural and geographic contexts to uncover heterogeneity where most researchers have implied homogeneity.

Don't be satisfied with simply statistically documenting differences or correlations among variables. Pursue causal relationships and processes in order to answer "how" and "why" questions: How do these outcomes arise? Why do these differences exist? How do these characteristics change?

Use the questions that perplex practitioners in this region to generate conceptually interesting and managerially relevant research questions. Asia represents a rich supply of phenomena at the interface between actors from different contexts, and questions of change and adaptation. Let these interesting questions define the research, not simply precedent or methodological biases.

Methodology

Match research questions and designs, and purposefully incorporate multiple methods (interpretive, correlational, process, quantitative, qualitative) to address dynamic and endogenous processes, not just static variables.

Avoid conceptually superficial and overgeneralized operationalizations of constructs (e.g., Hong Kong = collectivist, USA = individualist) to justify choice of samples.

Clearly define constructs and, in the case of multidimensional constructs, define key interactions, dependencies and contingencies. Adopt configurational and holistic approaches for studying multidimensional constructs.

Establish the "interpretive validity" of a research question, instrument or construct by exploring their meanings from the actor's point of view.

Contributions

Clearly state boundary conditions and limits to the generalizability of findings.

Contribute to "mainstream" research based on findings from Asian contexts by developing and defining constructs conceptually and without geographic or ethnic descriptors.

Contribute to management practice by generating potential solutions to problems motivating the research in the first place.

them, as appropriate, into each stage of the research process—from problem generation, to study design, to analysis and discussion and subsequent investigation—would greatly improve both the rigor and relevance of research focused on this region. Thus, they are both justified and useful.

Are these recommendations practical? Put another way, do individual researchers have the competence and motivation to undertake research that is more rigorous and relevant? It was beyond the scope of this review to address the characteristics of the researchers, their institutional environments, and their incentive structures. Indeed, such an analysis would entail dealing with even more variety and complexity than the literature review presented here. Furthermore, there is a question as to what elements of both the researchers and their environments, including their incentive structure, can be managed. Without such understandings, it is impossible (or foolish) to make specific recommendations about what should be changed.

Rather than completely avoid the question of whether these recommendations are practical or not, and since my own experience is admittedly idiosyncratic and limited, I would

prefer to suggest what difficulties can be expected in realizing some of the recommended changes. Of these, many are simply a matter of doing more thoughtful and careful research within a current paradigm. This includes expanding the diversity of contexts being studied and more completely describing them conceptually; better linking constructs with operationalizations; clearly stating the boundary conditions of a study's results; and providing a useful interpretation and "bottom line" message for mixed results. Therefore, the following discussion addresses the recommendations that will likely present more stubborn difficulties and overt contention; namely, the choice of research questions, choice of methods, and the short- and long-term objectives for results, as well as a comment on institutional constraints within Asia.

Questions

The recommendations regarding choice of phenomena, research questions and contexts seems straightforward enough; essentially, it is a plea for researchers to be more creative and curious in what they investigate (and not at the expense of rigor). Acknowledging my own biases, both my critique and recommendations are based on my own assumptions about what questions and contexts are worth pursuing. Still, to make collective progress in understanding (1) phenomena that are particularly relevant to management in Asian contexts, (2) similarities and differences within Asia and between Asian and non-Asian contexts, and (3) to contribute to research outside of Asia, we must be willing to pursue new questions in new contexts.

This pursuit, however, will require many researchers to go beyond their current intellectual comfort zone. It may simply entail asking similar questions in social, national, ethnic or industrial contexts with which they are not familiar. More fundamentally, it may lead researchers to study phenomena quite different from their current focus, or to confront the question of whether the constructs, research questions or methodological tools that are legitimate in "mainstream" research are appropriate or relevant for studies in the Asian context. Some researchers may be forced to critically review their own assumptions and past work. For example, they may be forced to consider variance within populations (nationally or ethnically defined) that they had assumed or implied in prior work to be insignificant. Others may have to confront issues of personal identity (e.g., Tsang, 1998) and their own need for uniqueness in order to more objectively tests hypotheses of similarity and difference with other groups, both within Asia and outside of Asia.

Methodologies

Many researchers focused on this region have developed skills in comparative study design, in addition to the human networks and logistic capabilities to carry out multi-sample studies. These are strengths that can be built on to investigate important questions. However, too many have used only correlational techniques, which they have also allowed to dictate the types of research questions they investigate. Accompanying the call for a critical reevaluation and diversification in the phenomena and research questions being studied, I am suggesting that many researchers should incorporate additional methodologies and data to at least

substantiate their context-dependent discussions of results and, even more, to answer more important questions of process and causality. Reporting factor loadings or correlations from cross-sectional data is inadequate for these purposes. Developing longitudinal panel data sets is a step in the right direction and relatively easy for researchers trained in quantitative methods. However, the related quantitative methods still only lead to conclusions about cross-temporal correlation or, depending on the design, causation. Such studies and analyses do not provide insight into the processes linking antecedents and outcomes.

It may be relatively more difficult for these researchers to develop skills in qualitative research—anthropologically-based observational and interpretive methods, or “ethnoscience” (Redding, 1994)—that require them to identify subjective meanings and processes. In addition to the differences in approaches to subjects, data and analysis, these methods are based on different assumptions about what is “rigorous”, and many researchers may not have the ability or personality to undertake both. Ironically, this may also be true of the (relatively few) researchers who use only interpretive methodologies; they may not have the skills or interest in complementing their qualitative work with relevant quantitative research.

For many researchers, the choice of methodology is not only a matter of training, but is intricately related to the perceived bias in leading U.S.-based management journals against qualitative work (and indeed, this perception is not limited to researchers in Asian contexts). The received wisdom is that qualitative studies are less likely to be published in “top-tier” journals, and conversations with individual researchers suggest that this is a key reason they are using quantitative methods. Whether or not this is accurate, in order to provide the insights into the origins and processes that lead to differences in behavior and outcomes—the types of questions that comparative researchers often and should be addressing—researchers must supplement quantitative approaches with qualitative, interpretive and process-based analyses and include these insights explicitly in their reportage. Such methodologies, combined with the plethora of phenomena in Asia that represent alternative trajectories to those in other contexts, promise interesting and important research opportunities that remain to be exploited.

Contributions

My recommendations for making better use of Asia-related research results seem simple enough: (1) broaden the horizon for comparative work beyond the U.S. and explore constructs identified through research in Asian contexts in non-Asian contexts, and (2) use the results to propose solutions to managerially relevant problems. The main challenges to implementing these recommendations will include not only institutional constraints, but also the cognitive constraints of the researchers themselves.

The first recommendation requires many researchers focused on this region to address their own parochialism, the mirror critique, ironically and overall justifiably, that these same researchers have of U.S.-based research. I am asking, optimistically, that researchers become more intellectually curious and adventurous, to see where the constructs and contexts with which they are personally familiar and originally concerned stand in relation to perhaps unfamiliar social, economic and institutional settings in other regions and countries. For example, the nature and implications of particularistic ties in Asia’s largely “collectivist”

societies has been a dominant focus of comparative research that has been largely limited to contrasts with relationships among U.S.-based individuals and organizations. As this review has suggested, and a more in-depth review of related topics would verify, researchers have collectively not explored similarities and differences on related constructs in other “western” settings. Anyone familiar with Italy or France, for example, will describe a social system in which “guanxi” is as fundamental an element as it is in China.

This Asian-focused parochialism (i.e., researchers not going beyond a particular Asian context or paradigmatic Asian vs. U.S. comparisons) creates a waste of potential and, more seriously, may impede research and understanding. First, it may result in researchers not taking up opportunities to involve a larger and more diverse audience in their work and investigate whether constructs and relationships have relevance beyond the original (Asian) context in which they were identified.

Such parochialism may also have a directly detrimental effect on the collective knowledge enterprise to which researchers should see themselves as contributing. The danger arises when a researcher perpetuates (unintentionally, we should hope) an unsubstantiated impression that the constructs and phenomena they are studying are limited to the particular Asian context that they have studied. This is the trap into which too many researchers studying, for example, “face” in Asia or “Confucian values” and “guanxi” in China and, before them, the scholars who have studied “lifetime employment” or “keiretsu” in Japan. Indeed, use of local terms such as guanxi can be dysfunctional when it blinds researchers to conceptually equivalent phenomena in other contexts, and also perpetuates stereotypes of individuals and organizations in the region with which such terms are linked. Focused on what they perceive as “unique” phenomena in the region, too many researchers have limited their often superficial comparisons to U.S. contexts and, after “failing” to find equivalent constructs or implications, present these constructs as uniquely Asian (or Chinese or Japanese or . . .).

In contrast, the few studies that have specifically, almost tongue-in-cheek, investigated such “Asian” constructs in non-Asian settings, such as “Confucian values” in the USA (Robertson and Hoffman, 2000) have found that the constructs and relationships are relevant beyond Asian settings. These studies allow for the possibility of finding that non-Asian actors exhibit the same cognitive or behavioral characteristics as those of a group in an Asian context, or that relationships among variables are similar across social or cultural contexts. Such studies are the first step in what may be called a type of “reverse-exporting” of concepts from Asia to “western” contexts and, thereby, contributing to a broader discourse on management. To facilitate this, however, researchers should be willing to de-link concepts from geographic, ethnic or other idiosyncratic linguistic descriptors. More generic terms increase the likelihood that researchers outside Asia will be able to identify or investigate related phenomena in new and perhaps unexpected contexts.

My second recommendation regarding the use of research results is consistent and intricately linked to my earlier recommendation regarding choice of research topics and questions; namely, not only should the research be grounded in managerially salient issues, but the results should suggest better management practice. (“Better”, in this case, may be defined in many different ways and I do not suggest a particular dimension, such as economic efficiency, nor limit the perspective to a particular stakeholder.) While simply establishing

differences between groups has become the de facto objective and contribution of much of the comparative research focused on the region (Redding, 1994), this alone does not inform practice. Similarly, uncovering “sources of performance difference” may lead to useless or dysfunctional advice if the results are spurious correlations and not corroborated with insights into the process that links the “sources” to “performance”.

It is clearly contentious (but not hopeless, I hope) to propose that researchers concern themselves with “real world” issues and implications. Although I feel strongly that management research should be normative and contribute to both theory and practice, I also recognize that this is a matter of personal conviction. Furthermore, this issue is obviously not limited to research in Asian contexts. However, many researchers justify their studies based on “the increasing importance of” or “managerial interest in” (Asia, NICs, China, Japan, etc.). Granted that this is true (and even Japan remains a major economic power, in spite of its current morass), these same researchers should attempt to provide more than simplistic conclusions that actors or processes in this region are different from those in other contexts, or that two variables are correlated. In other words, given those differences or correlations, what could a manager or other actor do to achieve desired, or avoid undesired, outcomes?

Incentive structures in regional institutions

We should expect researchers based in Asia to make major contributions to research on the region. To do this, however, Asian-based researchers and local academic institutions must deal directly with their collective ambivalence towards “U.S. academic hegemony.” If an institution’s reward structure simply imitates that found in the U.S., then the leaders of that institution, and not a faceless group of “U.S.-based researchers and journals”, are responsible for perpetuating that hegemony. For example, management scholars in Asian-based institutions are increasingly rewarded only for publishing in “mainstream” academic journals that are all English-language and mostly U.S.-based with a primarily U.S.-based audience. Publications in regionally-focused journals, non-English language journals, or books and chapters in edited volumes are deeply discounted in institutionalized evaluations of individual research productivity. This institutional structure reinforces an individual academic’s incentive to publish in those same “recognized” journals to increase his or her own marketability and, to do so, place priority on undertaking research and writing articles that appeal to those journals’ reviewers and audience, rather than choose to study perhaps regionally-important phenomena in “unorthodox” ways.

As a result, individuals face conflicting pressures. On one hand, researchers at local institutions are expected—by both the local community and often university leaders—to undertake locally relevant research and search for solutions to locally relevant problems. On the other hand, a review of the recognized “journal lists” and research evaluation criteria that have been implemented in recent years in some Asian institutions (e.g., Hong Kong and Singapore) clearly show that researchers are disproportionately rewarded for “hits” in outlets that usually do not have local (Asian) relevance as a high priority, and too often not rewarded for other contributions. Until Asian institutions have the confidence to say and act as if locally-focused publications and other intellectual contributions are equally legitimate as “western” (primarily U.S.-based) journals, individual researchers are quite

rational in orienting their research questions, methods and discourse towards the reviewers and audience of those “western” journals. And until there is a clear signal to individual researchers that local relevance is both important and rewarded, we should not expect to see an appreciable increase in output that has local impact as its primary objective.

On the other hand, greater local relevance cannot become a rationale for justifying lower standards of research quality than found in those same “western” journals. This is the weakness of, for example, university-supported journals that are particularly common in many Asian countries. Too often these outlets are simply refuges for low-quality research and create a façade of research productivity. Those who choose to promote locally-focused research must, at the same time, ensure that rigor does not suffer.

Conclusions

This review has drawn on an extensive sample of journal articles to identify trends and weaknesses within the extremely broad range of management studies focused on Asian individuals, organizations and contexts. It has argued that current questions, methodologies and objectives are too limited and in many cases not appropriate for understanding the phenomena that otherwise justify research (and practitioner) interest in this region. The proposed recommendations could help move our collective research effort towards greater rigor and relevance, although there are clearly institutional and cognitive barriers to implementing them. I can at least hope, however, that this article will contribute to a growing discourse among researchers regarding the fundamental issues of what, how and why we are undertaking research in this region, as well as how to improve our efforts. Doing so is an essential step for us to see an increase in the output of research focused on the region that (1) appears in and also has an impact on “mainstream” outlets and research, (2) broadens and deepens our understanding of phenomena in the region, and (3) improves practice.

Appendix

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