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The Future of Chinese Management Research: Rigour and Relevance Redux

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ABSTRACT We use the parable of the blind men and the elephant to suggest that Barney and Zhang (2009) and Whetten (2009) analogously touch on only a part of the Chinese management research puzzle. Their analyses remind us of many attempts at anchoring the research purpose – etic versus emic approaches, exploration versus exploitation approaches, rigor versus relevance scenarios – touched on by the many commentators in this issue. We suggest researchers first answer the 'purpose' questions before embarking on the research design. The research design should fit the purpose of the knowledge, which is either to improve the performance of Chinese organizations (meeting the relevance criterion) or to replicate, extend or refine a theory developed in the US (meeting the rigour criterion). We believe the strength of applied management research allows us to create knowledge that can meet the criteria of both rigour and relevance. We support the use of academic international research teams and dialectic debate as tools to move the field of Chinese management research forward.

KEYWORDS context, polycontextuality, relevance, research team, rigour

It was six men of Indostan
To learning much inclined,
Who went to see the Elephant
(Though all of them were blind),
That each by observation
Might satisfy his mind.

The *First* approached the Elephant,
And happening to fall
Against his broad and sturdy side,
At once began to bawl:
"God bless me! but the Elephant
Is very like a wall!"

The *Second*, feeling of the tusk,
Cried, "Ho! what have we here
So very round and smooth and sharp?
To me tis mighty clear
This wonder of an Elephant
Is very like a spear!"

The *Third* approached the animal,
And happening to take
The squirming trunk within his
hands,
Thus boldly up and spake:
"I see," quoth he, "the Elephant
Is very like a snake!"

The *Fourth* reached out an eager hand,
And felt about the knee.
"What most this wondrous beast is like
Is mighty plain," quoth he;
'Tis clear enough the Elephant
Is very like a tree!"

The *Fifth*, who chanced to touch the ear,
Said: "Even the blindest man
Can tell what this resembles most;
Deny the fact who can
This marvel of an Elephant
Is very like a fan!"

The *Sixth* no sooner had begun
About the beast to grope,
Than, seizing on the swinging tail
That fell within his scope,
"I see," quoth he, "the Elephant
Is very like a rope!"

And so these men of Indostan
Disputed loud and long,
Each in his own opinion
Exceeding stiff and strong,
Though each was partly in the right,
And all were in the wrong!

Moral:

So oft in theologic wars,
The disputants, I ween,
Rail on in utter ignorance
Of what each other mean,
And prate about an Elephant
Not one of them has seen!

John Godfrey Saxe, 'The Blind Men and the Elephant: A Hindoo Fable' (1873)

INTRODUCTION

A poet's observation of knowledge creation and science can be informative: John Godfrey Saxe (1873) could well have been commenting on the current debate surrounding the future of Chinese management research rather than the dogmatism against which he was railing. Philosophers' perspectives are equally compelling: this parable has its tradition in Buddhist, Hindu, Jain, and Islamic centuries old traditions. Its timelessness is noteworthy, and its lesson relevant to Chinese management research, indeed to all management research. The poet raises the following questions: How do we know what we know? How do we know what we *do not* know? How do we know that what we know reflects reality and is sufficiently inclusive? When do we know that we know enough? And finally, why does this matter? These are philosophical questions that must be answered in considering the positions taken by Barney and Zhang (2009) and Whetten (2009) on the future of Chinese management research. At this point, we assert that Barney and Zhang's (2009) and Whetten's (2009) positions reflect the poet's conclusion '. . . each was partly in the right, / And all were in the wrong!' (Saxe, 1873). Each argues for a future reflecting good science as they know it. However, we believe each inadequately incorporates a sufficiently broad definition of the purpose of Chinese management research. In this essay, we revisit the question of rigour versus relevance as two purposes of research (Daft & Lewin, 1990, 2008). The purpose of Chinese management research, according to Barney and Zhang and Whetten, is primarily to extend, enhance, and improve Western or non-Chinese theory – or 'rigour' from a natural experiment perspective. This goal is especially important if the Chinese researchers aim to join the conversation among Western scholars. If the purpose is to improve the practice of management in China by Chinese managers in Chinese organizations – the exploration of the unknown but 'relevant' perspective – their position is less clear. At least according to Barney and Zhang, such research is not likely to be publishable in Western journals. In joining this debate, neither Barney and Zhang (2009) nor Whetten (2009) have shown enough appreciation for the importance of the profound contextual differences and dynamism that surround *management change* in China. Below, we reflect on each of the two leading essays and, along the way, offer some suggestions to meet both rigour and relevance in advancing Chinese management research.

ANALYZING CURRENT PERSPECTIVES ON THE FUTURE OF CHINESE MANAGEMENT RESEARCH

Reflections on the Barney and Zhang Perspective

Barney and Zhang (2009) steer us toward an either/or proposition as they contrast the 'Theory of Chinese Management' road with the 'Chinese Theory of Management' road on the way to the future of Chinese management research. They suggest that 'given that each research road may be valuable . . . what is important for the field of Chinese management is that there are multiple scholars each pursuing one of these roads' (2009: 26). While we agree that each of these research roads has the potential to make a valuable contribution to understanding Chinese management, we believe that Barney and Zhang (2009) unnecessarily (and perhaps falsely) dichotomize the directions of future research on Chinese management. We suggest rephrasing their perspective as follows: *When would a theory of Chinese management make the greater contribution to Chinese management? When would a Chinese theory of management make the greater contribution to Chinese management?* And, *When would dialogue or dialectic debate between the two perspectives make a greater contribution to Chinese management?*

To answer these questions, one would need a deep understanding of the influence of context and/or boundary conditions on Chinese management to assess when a theory of Chinese management or a Chinese theory of management approach is best used. Barney and Zhang (2009) begin their arguments through a specific lens, one that sees the purpose of Chinese management research in the light of US-based management research. For example, they tie the study of *guanxi* to social capital theory without dealing with the profound contextualization conventions associated with *guanxi*. However, we cannot agree with this asserted tie or Barney and Zhang's lens without knowing first the purpose of the research.

Following Luft and Ingham (1955), we identify four types of knowledge germane to the Chinese management research debate (see Figure 1). They are (i) knowledge held by everyone, both US and Chinese scholars – or integrative knowledge; (ii) knowledge held by a particular foreign group, which does not extend beyond the boundaries of the group, in this case US scholars – or isolated US-based knowledge; (iii) knowledge held by local scholars, in this case Chinese – or indigenous Chinese knowledge; and (iv) knowledge that is yet to be discovered – or undiscovered knowledge. Luft (1969) and Luft and Ingham (1955) present their framework as the Johari window, a personal development tool which we have adapted to capture our thinking on knowledge. Given this classification scheme, the purpose of knowing makes any one of these cells equally important to the others and would be the primary determinant of the research design approach. If the purpose of the

Figure 1. Types of knowledge

Knowledge Held by US Scholars	**1** **Integrative Knowledge** ***Examples:*** • ***Accounting principles*** • ***Operations management tools and techniques*** • ***Statistical techniques***	**2** **Isolated US Knowledge** ***Examples:*** • ***Stories rooted in US history*** • ***Nonverbal behaviors*** • ***Jokes and riddles*** • ***References to television programs from childhood***
Knowledge Unknown to US Scholars	**3** **Indigenous Chinese Knowledge** ***Examples:*** • ***Face giving and face taking behavior*** • ***Historical memories*** • ***Precedence matching as a decision style*** • ***Paternalistic leadership***	**4** **Undiscovered Knowledge**
	Knowledge Held by Chinese Scholars	**Knowledge Unknown to Chinese Scholars**

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knowledge is to improve relevance or the performance of organizations, all four types of knowledge hold promise for the future of Chinese management research. If, however, the purpose of the knowledge is to promote research rigour *per se*, then one could argue that uniquely indigenous research that focuses on relevance to Chinese management is relatively unimportant to Western scholars.

Barney and Zhang (2009: 16) ask 'Will Chinese scholarship focus on knowledge for knowledge's sake [done by scholars seeking to develop a rigorous theory of Chinese management], or will it focus on application and improving firm performance [done by scholars seeking to develop a relevant Chinese theory of management]?' Are these options mutually exclusive? We believe not. Decoupling knowledge creation from application misses the point of applied management research – to create applied knowledge that will improve individual, group, intergroup, firm, or industry performance. Applied management research, we assert, is both rigorous and relevant. It is both exploitative and exploratory. It is both emic and etic and does not benefit from these dichotomies. Those scholars, including Barney and Zhang, who are more deeply involved in Chinese management research than mainstream US-trained scholars entertain the possibility that the 'Chinese theory of management' path can bear fruit, especially for helping Chinese firms improve domestic and international performance. This is particularly true for knowledge that is relatively more sensitive to culture – knowledge that requires insight into the nuances of human behaviour, which tends to be context-dependent. Leadership studies are an example. Cheng, Wang, and Huang (2009: 94) note that 'Leadership studies conducted by Chinese scholars in Chinese settings and published in international journals . . . mainly focus on Western leadership concepts, such as transformational leadership or leader–member exchange.' It is probable that there are uniquely Chinese leadership characteristics and behaviours that remain to be discovered given the focus on using Western leadership concepts. Cheng et al. (2009) give a good example of the development of an indigenous theory of Chinese paternalistic leadership. Discovery of this knowledge would certainly be relevant to the practice of Chinese management.

Barney and Zhang (2009: 20) assert that the goal in developing a theory of Chinese management '. . . would be to publish it in the highest status academic journals in the world.' Tsang (2009) concurs and notes that young Chinese scholars face pressure to publish in these top journals. This leads us to ask two questions, both relating to the publication of research. Our first question: Was there science or knowledge creation before there were journals? We believe there was: many Chinese organizational improvements such as the formalized use of civil servants, Mandarins, and their related civil service examination system, which preceded Weber's (1947) classic work on bureaucracy, improved performance but were not published in the highest status academic journals. Such journals did not exist in the Qin (221 BCE–206 BCE) and Han (206 BCE–220 CE) dynasties, yet we doubt one would seriously argue that these dynasties were devoid of knowledge creation. Our second question: Is it possible to have scholarship that creates knowledge which contributes to application and improving firm performance but that is not published? We believe that it is possible; certainly the proprietary knowledge created and applied inside companies around the world often contributes to improvement in a firms' performance. This research is simply not published because the firm believes it is a resource that confers competitive advantage (Barney, 1991) so long as it remains proprietary and relevant. To summarize these two questions in another way, is not knowledge creation and application valuable in and of itself, regardless of whether or not it is published? While we don't wish to downplay the value of sharing knowledge through published media or the very real emphasis on and importance of publishing for management scholars, we do think it is necessary to avoid overlooking knowledge creation in its own right.

Barney and Zhang (2009) also question whether it is possible for a single scholar to conduct both types of work. We agree with their implication that it is rare for a *single* scholar to have sufficient depth or expertise in multiple cultures to conduct both types of research (Teagarden & Von Glinow, 1997; Teagarden et al., 1995; Teagarden, Drost, & Von Glinow, 2005; Teagarden, Von Glinow, & Drost, 2007). Solving this problem is one of the benefits of using academic international research teams (AIRTs), a suggestion echoed by other commentators in this forum (e.g., Cheng et al., 2009; Whetten, 2009; Zhao & Jiang, 2009). Academic international research teams combine the knowledge held by US scholars and Chinese scholars, knowledge that otherwise would have been 'unknown' to both, and, in the process, quite possibly assist in the discovery of new knowledge previously unknown (see cell 4, Fig. 1).

We agree, as do Zhao and Jiang (2009), with Barney and Zhang's (2009) suggestion that knowledge created from the synthesis of ideas generated from the two roads they identify can make a contribution to Chinese management research. We have argued in our earlier writing the value of dialogue and dialectic debate in cross-cultural management research and have provided recommendations for how this might best be accomplished (Shapiro, Von Glinow, & Xiao, 2007; Teagarden & Von Glinow, 1997; Teagarden et al., 1995; Teagarden et al., 2007). We suggest that dialectic is most effectively produced within a team-based, intellectual joint venture process through the use of academic international research teams. Academic international research teams utilize a dialectic approach *by design* as contrasted with the possible potential for dialectic recommended by Barney and Zhang (2009). Academic international research teams are particularly useful for moving both isolated US-based knowledge and indigenous Chinese knowledge toward integrative knowledge held by all through knowledge sharing and for providing or creating new knowledge through cooperative discovery.

These academic international research teams are a superior path to research rigour, which we define, following Staw (1995: 89), as 'the strength of inference made possible by a given research study.' It is more than the statistical power of the study and more than the study's quantitativeness: the vast majority of the issues in cross-cultural research are logical or perceptual rather than statistical. Strength of inference is the result of the exploration, probing, blending, and contrasting of diverse perspectives to understand a study's multiple contextual elements – most often through a qualitative, inductive approach, and sometimes through quantitative, inductive, or deductive approaches – in order to produce cross-cultural research rigour. The North American positivist approach focuses on deductive theory development and testing. It emphasizes rigorous quantitative methods and measurement precision as well as internal and external validity. This sharply contrasts with the need to accommodate the powerful and most frequently unseen contextual influences found in global cross-cultural research studies, including those that focus on Chinese management (Drost, 2001; Shapiro et al., 2007; Teagarden et al., 1995; Teagarden et al., 2005; Von Glinow, Teagarden, & Drost, 2002). The intentional dialectic of international research teams enhances the likelihood that research will at least be rigorous by our more inclusive definition, if not more relevant.

Finally, we question Barney and Zhang's (2009) assertion that the field of Chinese management scholarship is emergent. American and European trained scholars have been studying Chinese organizations since the People's Republic of China normalized relations with the West more than thirty years ago, around 1978. They have followed the theory of Chinese management path that Barney and Zhang (2009) identify. There are many examples, and below are just a few. They include Bennett and Allen's (1979) work on small business opportunity, Tung's (1981, 1982) work on motivation and Chinese industrial society, Fisher's (1983a, 1983b) work on R&D in China, Warner's (1985, 1987) work on training and management reforms, Campbell's (1986) work on China strategies, Conley and Beamish's (1986) work on joint ventures, and Frankenstein's (1986) work on Chinese business practices. Further examples are Redding and Wong's (1986) work on the psychology of Chinese organizational behaviour, Shenkar and Ronen's (1987) work on work goals, Boisot and Child's (1988, 1996) work on bureaucratic failure, Von Glinow and Teagarden's (1988) work on human resource management and joint ventures, and Simon and Rehn's (1988) work on innovation. The contributions of these pioneers provide solid examples of the foundations of a research stream that has evolved for more than thirty years.

As the stream of research on Chinese management has evolved and matured, methodological approaches have diverged from the early focus on inductive analysis and thick description to more deductive theory development and testing. Because China is a new research context, thick description of novel phenomena and articulation of context are essential for both foreign and Chinese scholars' understanding. Much of this understanding was generated in part through conversations with local Chinese scholars who had been trained very differently but, nonetheless, understood their local reality. There are now foreign scholars trained outside of

China and Chinese scholars trained in China or abroad who can and do study Chinese management phenomena. This stands in sharp contrast to nearly three decades ago when 'capitalist roaders' were scorned, capitalism was suspect at best, and organization behaviour was discounted as a 'bourgeois science' best shunned by scholars conducting research on organizations in China.[1] In addition, accessing research sites is much more possible today, and research scope can be much broader. China scholars – from China or abroad – no longer need 'minders' or sponsors to ensure that they remain focused on their narrow scope of sanctioned or approved research. Accessible research subjects have grown beyond studying joint ventures, and now it is possible to pursue many more topics with many more types of organization. For example, state owned enterprises (SOEs) are privatizing and entering the competitive world of business and provide interesting subjects for study. Indeed, there are many exciting changes for Chinese management researchers.

Unfortunately, in our roles as editors and reviewers for academic journals, we have seen the scope of Chinese management research evolve from a focus on the relevance of the big picture and important issues facing firms operating in China, such as those studied by the pioneers, to a relatively reductionist, albeit quantitatively elegant, focus on applying or extending US-based theories. Following this particular path (a theory of Chinese management) destines us to trade relevance for rigour and, in so doing, prevents us from discovering new knowledge. We have gone from discovery and description – probing and understanding boundary conditions and Chinese processes – to an overemphasis on theory and hypotheses testing. This is not to suggest that the deductive, rigorous, quantitative studies are bad when it comes to discovering new knowledge. It is rather that we observe with some trepidation Chinese scholars primarily using theories developed in the West, validated on Western subjects and relevant in the West as the basis for their research questions. Cheng et al. (2009: 94) go even further when they note that 'Chinese researchers themselves also pay surprisingly less attention to Chinese culture's impact on the leadership phenomena than to the validation of Western leadership theories.' Such studies contribute little to creating knowledge and instead fall into what we believe are type III errors, or solving the wrong problem well (Mitroff, 1998). The preponderance of type III errors is striking and rooted in Chinese scholars' lacking understanding of foreign theories' boundary conditions. Regardless of how well the research is executed or how high the quality of the outlets in which it appears, research with type III errors – research that asks the wrong questions or solves the wrong problems – does little to create knowledge for knowledge's sake or important applied knowledge that can contribute to improvement in Chinese firm performance.

We agree that there are two possible paths for Chinese management scholars, but we suggest they may not be the two identified by Barney and Zhang (2009). Rather, the two alternative roads for Chinese management research are the one leading to applied management research relevance and the one leading to research

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rigour through positivist reductionism. Relevant Chinese management research will be that research that first asks the right research questions to improve the practice of Chinese management. If that relevant research also has the spillover benefit of improving Western management theory, this is even better. If a dialectic between relevant Chinese management scholars and Western management scholars sheds light on the domain of undiscovered knowledge, better yet. The value of dialectic cannot be underestimated in Chinese management research, especially for its potential role in moving rigorous scholarship toward relevance. Barney and Zhang (2009) were right in highlighting its importance. Dialectic is perhaps the first best step in a long journey toward the future of Chinese management research.

Reflections on the Whetten Perspective

> It does not matter if the cat is black or white so long as it catches mice.
>
> Deng Xiaoping, 1962[2] (Marti, 2002: 31)

Whetten (2009) presents a recapitulation of the emic/etic debate that has been addressed by cross-cultural management researchers since 1960 (cf. Berry, 1969; Pike, 1960) and extends it with recommendations for making research more context sensitive. Whetten cautions that it appears that currently there is a tendency to conflate single context theorizing with context-sensitive theory. There is a long-standing debate in the social sciences about the usefulness of working intensively within one country to try to understand the issues purely pertinent to that culture. This is 'emic' or indigenous research versus research working across cultures to reveal 'universal' generalizations called 'etics' (Berry, 1990; Drost & Von Glinow, 1998; Peterson, 2001; Peterson & Pike, 2002; Teagarden & Von Glinow, 1997; Teagarden et al., 2005; Tsui, 2004). The most basic question that must be answered before this debate can be joined is what is the purpose of the research?

Whetten distinguishes research to answer 'why' questions – implicitly rooted in inductive research – from 'what' or 'how' questions – implicitly rooted in deductive research. He suggests that context sensitivity is important because it contributes to both inductive and deductive research. We agree that the specification of boundary conditions is essential for either inductive or deductive research, and this specification requires being aware of the context and, beyond that, *understanding* the context. We further concur with Whetten that both contextual assumptions and conceptual assumptions should be specified and that 'we need to give special attention to the challenge of identifying theory relevant context effects and determining what role they should play in context sensitive theory applications' (2009: 39). However, most US (non-Chinese) scholars are not likely to be able to understand or interpret Chinese contextual assumptions, especially at the behavioural or cultural levels. Whetten also recommends that those outside the mainstream research conversation, in this case Chinese scholars, 'develop a native understand-

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ing of the borrowed theory' (2009: 46). We believe it is equally unlikely that most Chinese (local) scholars, even if trained in the West, will *a priori* fully understand the conceptual assumptions surrounding a borrowed theory, given that many of these conceptual assumptions are likely to be tacit. We concur with Whetten about the use of cross-context research teams since, without debate and dialogue, it is difficult to know what we don't know.

Whetten (2009: 36) aligns with scholars who argue that research is 'an iterative, recursive process, linking extant comprehensive theory (etics) with information drawn from new locations (emics)' (cf., Peterson, 2001; Pike, 1960, 1990). Such a process may or may not extend a theory. To the degree that an extant theory is applicable in a new location, the extant theory can extend existing knowledge and lead to what we call 'integrative knowledge'. However, this presupposes that the extant theory is indeed applicable in the new location and that the purpose of the research is to extend extant theory. Nevertheless, if this is the case, a research design must include dialogue or dialectic debate between the extant theory perspective and the local perspective to ensure the outcome of research is planned and not random.

We support Whetten's (2009) suggestion that scholars intending to publish in high quality Western outlets should consider participating in cross-context research teams, which corresponds with our earlier discussion of the value of academic international research teams. Participation in such cross-context research teams is useful for scholars in discovering undiscovered knowledge or in seeking to more deeply understand their own contexts. Dialogue and debate between our blind men has more potential for generating understanding and creating knowledge about the elephant than the independent view of any one blind man.

What is 'the context' that scholars should describe to contextualize their research? Whetten did not provide much discussion on this, but we believe it is worth exploring. Cheng (1994: 165) suggests that context-embedded research ought to include '. . . a nation's social, cultural, legal, and economic variables as predictors and organizational attributes as dependent variables.' Peng (2000) argues for explicit consideration of the institutional context when examining strategy in transition economies. Although such contextualization will strengthen scholars' understanding of the phenomenon under study at varying levels of analysis (i.e., individual, group, organizational, institutional, and national levels), this type of contextualizing reveals only one type of context – the context that can be historically verified, seen, and/or measured. The contextualization that scholars have called for and used previously is limited because context is treated as a singular and qualitatively similar phenomenon (Shapiro et al., 2007). This type of contextualization fails to capture the multiple qualitatively different layers of context that are inherently present in any setting, particularly those encountered in cross-cultural research. For example, if one states, 'I am dead serious' and then winks his or her eye, this embeds a non-verbal behaviour with a verbal one. Most observers from the US would understand that the eye wink negates the verbal statement. However, even those with profound cultural knowledge sometimes may not be able to read such signals. Those outside the US might miss this altogether.

The contextualization descriptions that scholars typically provide, whether at the individual, group, organizational, or institutional level of analysis, tend to favour descriptors that are immediately recognizable. These may include people's gender or race or a nation's social, cultural, legal, and economic variables (e.g., Kirkman & Shapiro, 2001; Peng, 2000; Shapiro et al., 2007; Tinsley & Brett, 2001; see Shenkar & Von Glinow, 1994, for a review). These variables are demographic descriptions. While there are many of these demographic contextualizations, they are all of a similar kind. In other words, they do not cross boundaries into qualitatively different contexts, such as cognitive, sensory, emotional, aesthetic, or spiritual contexts – the stuff of which deep understanding and nuance are made. Shapiro et al. (2007: 133) 'use the term "polycontextuality" to sensitize [indigenous scholars] to the fact that contexts can include verbal- and non-verbal-nuances that [may be] difficult to observe' but are critically important nonetheless in the conduct of research. Furthermore, Child (2009) reminds us that these multiple contexts are undergoing contextual evolution, including material, ideational, and institutional changes. Once again, international research teams can serve to clarify such contextual nuances through dialectic debate and discourse.

Thus, we repeat the concern raised by Shapiro et al. (2007) and shared by Tsui (2004) that our typical methodological toolbox, handed to us by mostly Western-trained scholars, may be ill-suited for making the non-obvious obvious, as is often required in indigenous research. This is not a new research mandate: making the non-obvious obvious has been described elsewhere as making the unfamiliar seem familiar (cf., Whetten, 2002, 2009). One problem is that the contextual descriptors people 'see' come from the cognitive schemas built up over time from their experiences, and people's schemas direct what they will notice and interpret (Kelley, 1972). In sum, context matters, and it must be made explicit – in all its complexity, observational difficulty, contextual embeddedness or polycontextuality, and messiness – to ensure research rigour and improve accuracy of understanding. The use of academic international research teams will be especially useful for decoding the nuances of the Chinese cultural context otherwise unknown to US or to any non-Chinese scholars.

CONCLUSION: CLARIFYING THE PURPOSE OF CHINESE MANAGEMENT RESEARCH

> Letting a hundred flowers blossom and a hundred schools of thought contend is the policy for promoting progress in the arts and the sciences and a flourishing socialist culture in our land.
>
> Chairman Mao Zedong, 1957 (Spence, 1999: 131)

The future of Chinese management research depends on the purpose of that research. Exploration will be necessary if the purpose of Chinese management research is to inform the local practice of Chinese management. This exploration can be done two ways, mirroring Barney and Zhang's two roads – either by localizing borrowed theory or by creating local theory. This type of research is likely to be highly relevant, and the challenge is to improve its rigour. Exploitation, on the other hand, will be necessary if the purpose of Chinese management research is to refine the extant theory. Chinese management practice can and will be exploited as a natural experiment through which we can test the limits and better understand the boundaries of theory produced outside of China. This theory testing can be done using either an inductive or a deductive research design. This type of research is likely to be highly rigorous, and the challenge is to improve its relevance for Chinese managers. One is not inherently superior to the other, they are simply different tools for different jobs in different contexts, and the success of both relies on avoiding the type III error.

A middle of the road approach – integrating exploration and exploitation, rigour and relevance, emic and etic – is where the potential to discover or make evident unknown knowledge is most likely. This area, depicted in cell 4 of Figure 1, holds great promise for scholars interested in management in the Chinese context. Management issues that are more behavioural or cultural will require a level of insight and understanding that is highly context sensitive and requires deep inside knowledge. Those research issues that are more global will require a level of insight and understanding that reflects non-Chinese context sensitive or outside knowledge.

Using the right tools and approaches to uncover knowledge is good science and depends on the reason the research is conducted. The future of Chinese management research will need to encompass the spectrum (rather than dichotomy) of research from rigour to relevance, emic to etic, and exploration to exploitation, as well as any approaches in between. Wise scholars will work hard to understand, debate, and discuss what one another means regardless of their perspectives. This will also let 'a hundred schools of thought contend' as Chairman Mao proposed, and some of these will surely produce important new knowledge. Knowledge that can contribute to the practice of Chinese management would be one important outcome, and knowledge that will be useful to scholars in and outside of China would be a positive additional benefit. Looking through each other's eyes and sincerely trying to grasp one another's meaning will lead to a better understanding and interpretation of our elephant, Chinese management.

NOTES

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[1] The concept of 'capitalist roader' was developed by Mao Zedong and refers to Chinese Communist Party members who attempted to pull the Party toward Capitalism. Marxist-Leninist thought differentiated 'bourgeois science' from 'proletarian science', the latter representing the official ideology and the former clearly pejorative. A comprehensive treatment of this topic can be found in Fu (1993).

[2] Deng Xiaoping actually said 'black or yellow,' but the 'black or white' translation is the one we know best in the West.

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