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Editor's Introduction – Autonomy of Inquiry: Shaping the Future of Emerging Scientific Communities

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ABSTRACT Over two decades, research in Chinese management has exploited existing questions, theories, constructs, and methods developed in the Western context. Lagging are exploratory studies to address questions relevant to Chinese firms and to develop theories that offer meaningful explanations of Chinese phenomena. Framed as a debate between pursuing a theory of Chinese management versus a Chinese theory of management, this forum, through the voices of thirteen scholars, provides an analysis of the reasons for the current status of Chinese management research and offers alternatives to shape the future of Chinese management studies. Based on the principle of autonomy of inquiry and heeding the warning of the constraint of normal science, the Chinese management research community can shape its own future by engaging in research that may contribute to global management knowledge and address meaningful local management problems.

KEYWORDS autonomy of inquiry, Chinese management, etic versus emic, exploitation versus exploration, indigenous research, quality versus quantity

INTRODUCTION

Chinese management research is at a critical juncture. Framed as a choice between taking the road well-trodden (a theory of Chinese management) or the road less travelled (a Chinese theory of management), the present Editors' Forum focuses on the development and prospects of Chinese management research by engaging a group of leading scholars in a dialogue and debate on this issue. The two articles and six commentaries also explore alternative options that Chinese scholars can choose in order to study important topics and to pursue the development of valid management knowledge for local consumption while contributing to global knowledge and scientific progress. The developmental journey of Chinese management research is not unique, and lessons from the Chinese experience can provide useful insight or guidance for the development of international management research in general. In Whetten's words, 'the results of this analytical examination of theory and context are widely applicable' (2009: 30) to other contexts.

The increasing globalization of commercial activities has fuelled the need for knowledge on management not only on multinational firms operating in the global context, but also on management practices of firms in new economies (such as Russia, India, and China) as they become important players in the global market. Alongside these developing economies, there is also an active development of management research in many developed regions as evidenced by the creation or rejuvenation of academic management associations in Asia (e.g., Asia Academy of Management, Australia–New Zealand Academy of Management, International Association for Chinese Management Research), Europe (e.g., European Academy of Management, British Academy of Management), and South America (Brazilian Academy of Management), to name just a few. Providing direct evidence on the strong interest in the creation of global management knowledge is the increasing number of international members in the Academy of Management, the largest professional association of management scholars and students in the world. Over the past twenty years, international membership rates have risen from 13 percent in 1990 to 39 percent by the end of 2008 (with a total membership over 18,500). The International Association for Chinese Management Research (http://www.iacmr.org), dedicated to advancing research on organizations operating in the Chinese context, was founded in 2002 and had grown to 4,500 members by the end of 2008. Its official journal, *Management and Organization Review*, published its fourth volume in 2008 and, in the same year, received admission into the Social Science Citation Index of Information Scientific International.

Amidst this exciting growth in the interest of global management research, some scholars (Leung, 2007; Tsui, 2006; White, 2002) have observed a clear preference among international researchers to adopt existing theories, constructs, and methods from the dominant management literature (which tends to be based on studies in North America and, particularly, the USA) in examining their local phenomena. This results in what March (2005: 5) refers to as 'the homogenizing tendencies' of organizational studies. This tendency is a cause for concern since it may slow the development of valid global management knowledge and hamper scientific progress. This trend clearly exists in Chinese management research as well (Tsui, 2006).

In this introductory essay, I first summarize the current nature of Chinese management research as observed by the contributors of this forum (along with some observations of my own) and the alternatives they propose for the future. Their observations of the current status include a dominance of exploitation over exploration in scientific studies by scholars, Chinese scholars' inadequate understanding of the philosophy and purposes of science, and an under-development of institutional norms for evaluating and rewarding scientific efforts. I then synthesize their suggestions on shaping the future of Chinese management research. Their discussion revolves around two major themes. The first is the need to seriously consider relevant aspects of the national context for an accurate understanding and a valid explanation of management or organizational phenomena in China. The second is the need for changes in the institutional arrangements (at the national, university, and professional levels) to encourage scholars to pursue important and relevant topics that may not be popular in the Western literature but are important and meaningful to China. I conclude this essay with a discussion of the principle of 'autonomy of inquiry' (Kaplan, 1964) and a reminder of the constraint of the current normal science paradigm in addressing novel questions and in seeking new and accurate explanations of unusual phenomena (Kuhn, 1996). It is time for the Chinese management research community to chart its own course in developing valid knowledge and contributing to scientific progress, thus achieving both rigor and relevance, the dual criteria of good applied science.

THE CURRENT STATUS AND FUTURE DIRECTION OF CHINESE MANAGEMENT RESEARCH

In 2006, at the second biennial conference of the International Association for Chinese Management Research (IACMR) in Nanjing, China, Jay Barney and David Whetten offered their advice for the healthy development of Chinese management research in their respective keynote presentations. Their remarks at that conference form the basis of the two leading articles in this forum (Barney & Zhang, 2009; Whetten, 2009), which were also the focus of a special symposium at the third biennial IACMR conference of 2008 in Guangzhou, China. We invited six commentaries on these two articles. Two of the commentaries are by three Western scholars (Child, 2009; Von Glinow & Teagarden, 2009), who are authors of Chinese management studies and have served or are serving as editors of journals that publish international management research. Child is a past editor of *Organization Studies* and a current senior editor of *Management and Organization Review*. Von Glinow is a current consulting editor of the *Journal of International Business Studies*, and Teagarden is the current editor of *Thunderbird International Business Review*. These three authors offer 'outsiders' views' of the development of Chinese management research. Two commentaries are written by Chinese scholars who live in greater China. One essay is by three authors in Taiwan (Cheng, Wang, & Huang, 2009), and another essay is by two authors in mainland China (Zhao & Jiang, 2009), both of which offer 'insiders' views'. The remaining two commentaries are by Chinese scholars who are familiar with China but obtained their doctorates in the West and have published primarily in English journals. Leung (2009) lives in Hong Kong and Tsang (2009) in Texas, USA. They may be regarded as 'in-outsiders', who may be able to analyze the situation from more than one perspective. We did not ask these authors to take any particular perspective but

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simply requested that they comment on the two leading articles and offer any additional ideas or perspectives of their own on this issue.

The Current Nature of Chinese Management Research

There is a consensus among the contributors that deductive theory testing using theories developed in the West dominates Chinese management studies. Barney and Zhang (2009: 17) refer to these efforts as pursuing a 'theory of Chinese management'. Studies following this approach tend to be published in international journals, and many have appeared in top tier journals. The major outcome of this approach is the validation of existing theories or extension of the theories' contextual boundaries. This type of research is valuable since it generally meets the rigor criterion by having passed the high standards imposed by the editors and reviewers of the leading journals. Such studies provide a good illustration of contributions *of* extant theory in explaining unique phenomena in new contexts (Whetten, 2009). However, such studies tend to offer limited contribution *to* theory (Whetten, 2009) since their goal is not to discover new explanations of local phenomena. Not surprisingly, Cheng et al. (2009) and Zhao and Jiang (2009: 110) believe this approach 'limit[s] the discovery and understanding of phenomena important in and to China' and urge more studies that aim to develop a 'Chinese theory of management'.

Most of the research using this borrowing approach also tends to be under-contextualized. According to Whetten (2009: 49), 'all organizational studies theories are context dependent, in one way or another.' By not considering the new context when applying a theory originally developed in a different context, researchers risk discoveries that only fall within the domain covered by the theory. Potentially highly relevant and important knowledge resides in the dark areas which remain 'undiscovered' (Von Glinow & Teagarden, 2009: 78). Whetten (2009) describes two ways that context can be meaningfully used. One is to 'contextualize theory' by identifying the boundary conditions when a theory's predictions may have to be modified. The result is a context-embedded theory. The other is to 'theorize about context' by identifying aspects of the context that may provide meaningful predictions of individual or firm behaviour. The result is a context-effects theory. I refer readers to Whetten (2009) for examples of each type of contextualization. The fact remains that most Chinese management research tends to exploit existing theories and constructs (Li & Tsui, 2002) with limited contextualization. Furthermore, there are very few examples of what Tsui (2007) refers to as 'deep contextualization' or exploratory research that develops new context-rich theories. Tsui, Schoonhoven, Meyer, Lau, and Milkovich (2004) analyzed 106 China-focused organizational studies published between January 2000 and June 2003 and found only two studies proposing a new theory. I am not suggesting that the goal of scholarship is

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necessarily to develop new theories. Rather, it is the overwhelming proportion of Chinese studies that apply existing Western theories in explaining Chinese phenomena that is surprising and concerning.

The tendency to engage in exploitative research is understandable for many reasons. The first is the lack of advanced training in scientific methods and of a good understanding of the goal of science. Some (perhaps many) may have mistaken the goal of science as publishing papers rather than seeking valid understanding and explanations of important phenomena. Chinese scholars are smart and quickly learn how to apply research methods, but this does not mean that they have necessarily understood the underlying epistemology or ontology. They also tend to apply theories without a 'native understanding of the borrowed theory' – including its 'historical roots' and 'contemporary treatments' (Whetten, 2009: 46). Zhao and Jiang (2009) reported a study (Xu & Zhou, 2004) which reviewed all the strategy papers published in a leading Chinese management journal in one year and which concluded that the authors in these papers showed a poor understanding of the borrowed (Western) theories. However, understanding the contextual assumptions for theories developed in Western countries is easier said than done. These theories were developed in a specific place with unique political, economic, and social conditions at a particular time in the history of that location. Further, such contextual assumptions are rarely specified in the theory and are difficult for outsiders or latecomers to understand (Barney & Zhang, 2009; Whetten, 2009). This lack of deep understanding about the scientific method and the philosophy of science, as well as inadequate knowledge about the contextual assumptions of the theories being borrowed, may lead to limited and possibly erroneous explanations of Chinese management phenomena and, at best, a modest incremental contribution to knowledge.

Another reason for the tendency to employ received theories and methods is the strong institutional pressure (with its corresponding reward system) that encourages publication in international journals, mainly those listed in the Social Science Citation Index, and, ideally, in leading journals such as the *Academy of Management Journal*, *Journal of Applied Psychology*, *Journal of International Business Studies*, or *Strategic Management Journal*, all based in the USA. Zhao and Jiang (2009) reported that many Chinese business schools have adopted a 'journal list' similar to those used by American business schools. They observed that most Chinese scholars take the popular road due to such increasing institutional pressures and that 'the current academic environment in China has a distinct bias toward a theory of Chinese management' (Zhao & Jiang, 2009: 114). These institutional pressures signal publications as the major purpose of research rather than important scientific discoveries, and they encourage the study of topics, choice of theories, and use of methods popular in these journals, regardless of such topics' or theories' relevance for Chinese firms. Funding agencies like the National Natural and National Social Science Foundations in China also tend to favour publications in English journals over domestic outlets (Zhao & Jiang, 2009). Leung (2009: 127) makes a similar observation: 'To maximize publication success, it is all too tempting for Chinese management researchers to pursue an incremental route by heralding the refinement of well-known Western management theories as the primary contributions of their work.' Progress in scientific development is further hampered by the absence of a body of qualified reviewers and the lack of an objective review process by journals (Zhao & Jiang, 2009). Such absences of enough qualified reviewers and an objective review process also exist in Taiwan (Cheng et al., 2009). In brief, the institutional arrangements have incentivized Chinese scholars to take the road more popular – the theory of Chinese management – and to make the road more important – the Chinese theory of management – less attractive on which to travel.

A second consensus by the authors in this forum is that there is a need to encourage more scholars to take the road less travelled. Below, I synthesize the alternatives offered by this forum's contributors to advance Chinese management research that will contribute to scientific progress on the one hand and improve management practice on the other.

Alternatives for Advancing Chinese Management Research

'Would you tell me, please, which way I ought to walk from here?'
'That depends a good deal on where you want to get to,' said the Cat.
'I don't much care where,' said Alice.
'Then it doesn't matter which way you walk,' said the Cat.
'So long as I get somewhere,' Alice added as an explanation.
'Oh, you're sure to do that,' said the Cat, 'if you only walk long enough!'

Lewis Carroll, *Alice's Adventures in Wonderland* (2000: 64–65)

Without a goal or a destination in mind, any road is as good as another, but scientific research is not a random pursuit. There is a purpose to scientific inquiry. It is to seek reality or to pursue 'truth' (Babbie, 2007: 6; Kaplan, 1964: 3). The scientific method ensures that scientists' discoveries are good approximations of truth, which is the criterion of rigor that all scientific studies are expected to meet. However, for management, as an applied science, truth alone is not enough. Thus, a second goal for management research is to develop knowledge that is useful for improving practice, which is the criterion of relevance that all management scholars are also expected to satisfy. As pointed out by many scholars in this forum, most Chinese management scholarship based on extant literature would score higher on the rigor criterion but lower on the relevance criterion. It is likely that the review process of international journals favours those works that meet the rigor criterion more than the relevance criterion. However, Von Glinow and Teagarden (2009) warn us that studies with rigor but not relevance are prime candidates of the Type

III error (Mitroff, 1998) – solving the wrong problem well. They encourage us to 'first answer the "purpose" questions before embarking on the research design' and to be clear whether the purpose is '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)' (Von Glinow & Teagarden, 2009: 75). They believe that 'the strength of applied management research allows us to create knowledge that can meet the criteria of both rigour and relevance' (Von Glinow & Teagarden, 2009: 75). The road forward suggested by the contributors in this forum should help future Chinese management studies to be both rigorous and relevant. Their advice can be consolidated into two major points. The first is to take context seriously in all Chinese studies, and the second is to revise the institutional conditions for research.

Take the context seriously. Child (2009) defines context as comprising of a material system and an ideational system. He elaborates on the implications of context for both theory development and research methods. Context can change the meaning of constructs and the relationship between constructs. Context is necessary for comparative analysis and has the potential to aid in the identification of universal theories. Child, along with Von Glinow and Teagarden, points to the dynamic nature of context. Therefore, there is a need to develop a dynamic theory of Chinese management that captures the 'co-evolution of organizations and their contexts' (Child, 2009: 69). As a transition economy, change is a defining characteristic of China. Major changes are occurring in the legal and economic institutions (Krug & Hendrischke, 2008) that influence firm behaviour, along with noticeable changes in organizational culture (Cooke, 2008) when Chinese firms absorb the practices of Western firms. In fact, China is a big social experiment in process and provides an ideal context to develop dynamic theories of management and organizations.

A different kind of dynamic interplay between theory and context is suggested by Leung (2009). Leung shows how the dynamic learning between Western and Chinese theories can occur with the potential of producing 'innovative, culture-general theories' (2009: 121). This interplay, however, will depend on the development of Chinese theories. Pointing out the emic nature of the original American theories, Leung sees no reason why some Chinese theories developed within China and initially unique to the Chinese context could not become universal theories upon subsequent application and refinement in non-Chinese contexts. As he pointed out, Nonaka's 1994 knowledge creation framework is an excellent example of a theory developed in Japan based on Japanese firms, and it has become widely influential in the West.

Thus, attention to context is important in the future development of Chinese management research. Whetten offers four practical suggestions for scholars in new research contexts who intend to borrow received theories to develop contextualized research. Because the starting point of Whetten's analysis is existing theories, the kind of contextualization he discusses has the potential to aid in the development of context-embedded theories, context-effects theories, or context-free theories, but not context-specific theories. In fact, he does not offer much discussion on context-sensitive context-specific studies, which is the definition of indigenous research and theory (Cheng et al., 2009; Tsui, 2004, 2006, 2007; Zhao & Jiang, 2009).

The two sets of 'insider' commentators (Cheng et al., 2009; Zhao & Jiang, 2009) strongly express the need for context-specific indigenous research for the discovery of a Chinese theory of management and encourage Chinese scholars to take this less travelled road. Cheng et al. (2009) offer a five-step process that is similar to the inductive grounded theory development approach in the extant literature (Eisenhardt, 1989; Glaser & Strauss, 1967). However, by deliberately avoiding the influence of existing theories, there is a potential danger of reinventing the wheel. Therefore, it is necessary to check the literature after the new theory is developed. Even if the new theory is similar to an existing theory, such work, if done well, should be published because it contributes to the development of possible universal theories. During this development process, it is advisable to keep Tsang's (2009) concern in mind: we should avoid the proliferation of poorly developed theories. A constructive and developmental review process along with institutional rules that reward quality over quantity should serve to prevent the publication of weak theories.

Revise the institutional conditions. Changes in institutional arrangements are desirable at the national, university, and professional levels. At the national level, Zhao and Jiang (2009: 117) urge that 'funding agencies such as the NSFC should balance the support between research pursuing a theory of Chinese management and a Chinese theory of management. In fact, given the NSFC's mission for creating basic knowledge, it should devote more funds to projects that pursue a Chinese theory of management.'

Cheng et al. (2009: 100) discuss the need for high-quality journals in the Chinese language that would facilitate 'sophisticated theorization of Chinese management phenomena' in the native language before introduction of such theories to the global audience. Novel Chinese theories of management would be a welcome addition to the international research community. As Barney and Zhang (2009: 24) mention, 'In fact, we invite and encourage scholars to have their insights of a Chinese theory of management published in English language journals so scholars around the world could learn from such theories and consider the relevance to their own context.' Cheng et al. (2009) also expressed a desire for international journals to be more appreciative of Chinese studies aiming for a Chinese theory of management. Borrowing Barney and Zhang's (2009) phrasing, Cheng et al. (2009: 101) suggest that, 'If possible, editorial staffs should include at least one management scholar who is fluent enough in Chinese and familiar enough with Chinese institutions to appreciate this work's unique contributions.'

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At the university level, criteria for promotion and tenure of faculty as well as requirements for doctoral graduation should be revised to focus on quality rather than quantity (Zhao & Jiang, 2009). As Zhao and Jiang put it (2009: 115): 'We need a new evaluation mechanism that emphasizes quality over quantity and values developing locally valid theories as much as, if not more than, testing of Western theories.' Cheng et al. (2009) call for collaboration between Western and Chinese business schools in doctoral education in the development of future scholars. This is two-way learning with Chinese students learning about Western theories and methods while Western students learn about Chinese history, culture, and management systems.

Changes at the national and university levels are externalities. The most important changes should come from within the scientific community itself. Each scholar can choose which road to walk, depending on his or her own interests, skills, and aspirations. Chinese scholars are encouraged to resist the temptation of taking the easy road and, rather, to pursue a research career that will be more rewarding than following the well-trodden trail. In the opening of their essay, Cheng et al. offer a famous Chinese poem, which describes a traveller getting caught in the rain. As they interpret the poem, though it was 'a difficult journey, and his companions grumbled about the heavy rain . . . the traveller showed no regret over their path, even delighting in the experience' (Cheng et al., 2009: 92). Tsang (2009) is a good example of a successful scholar who has chosen his own path. He selected a topic (superstition and business decision-making) that is interesting to him and to the Chinese context even though it is not in the mainstream. He had the courage to conduct his study despite discouragement from colleagues, and he wisely selected an appropriate journal to give his idea a fair chance of exposure to the academic community. Creating good theory is hard work, and creating weak theory is not fruitful. I concur with Tsang on the value of observing and documenting empirical regularities and not rushing into the premature creation of a new theory. This is the same advice Hambrick (2007) offers, even to mainstream researchers.

For scholars in an emerging research community like China, there are multiple challenges to overcome. They need to learn the most advanced research methods along with an understanding of the philosophical foundations of such methods. They need to be well versed in the dominant theories along with having an understanding of the historical background when such theories were developed. Even with the best knowledge of methods and theories, high quality research and theorization cannot occur unless scholars also have a deep knowledge of their objects of study. They should thoroughly understand the problems that organizations in their local contexts experience in identifying the most important or relevant questions and develop deep insight about the nature of the problems. As Robert Park advised sociologists trying to understand social problems, 'Go and sit in the lounges of luxury hotels and on the doorsteps of the flophouses; sit on the Gold Coast settees and on the slum shakedowns; sit in the Orchestra Hall and in the Star and Garter Burlesque. In short . . . go and get the seat of your pants dirty in real research' (quoted in Bulmer, 1984: 97). Interestingly, deep knowledge of the phenomena is a defining characteristic of the most influential management and organization theories developed by many US scholars over the past decades (Smith & Hitt, 2005). Mintzberg (2005) reflected on his own theory development journey and concluded that there is no one model of or approach to theory development. However, there are some common themes, and these include a deep curiosity about an unusual phenomenon that defies common sense, a keen eye to see beyond the obvious, moving between developing rich descriptions around the question and abstracting simple patterns, and repeated iterations of this process. Theory development is an idiosyncratic process because intuition (which underlies most great theories) is a personal attribute. Theory development is not for everyone but can be a highly rewarding activity for some scholars, especially if the pressure to take the popular road is lessened.

Hence, the major challenge for Chinese scholars is not whether they can or should take the road less travelled but whether they 'have the courage to go against the current' (Tsang, 2009: 141). Along the same line, Leung (2009: 127) remarked, 'It takes wisdom as well as courage to conduct indigenous and integrative research.' Yet, all the contributors of this forum, whether explicitly or implicitly, hold the view that Chinese management scholars should take the road less travelled if they want to make a valuable contribution to both scholarship and practice.

AUTONOMY OF INQUIRY: SHAPING THE COMMUNITY'S OWN FUTURE

> The various sciences, taken together, are not colonies subject to the governance of logic, methodology, philosophy of science, or any other disciplines whatever, but are, and of right ought to be, free and independent. Following John Dewey, I shall refer to this declaration of scientific independence as the principle of *autonomy of inquiry*. It is the principle that the pursuit of truth is accountable to nothing and to no one not a part of that pursuit itself.
>
> Abraham Kaplan, *The Conduct of Inquiry* (1964: 3, original italics)

According to Kaplan (1964) the pursuit of science is not accountable to anyone but the scientific community, and the norms of scientific inquires are derived from inside the community and not from outside. Enterprises outside science (e.g., universities' tenure and promotion committees, funding agencies, professional associations) have the right to govern science only by the consent of the governed. In other words, the scientific community must define the rules of scientific conduct. The community must initiate the necessary changes in the institutional arrangements that support its scientific pursuits. These include defining the important or relevant questions to be studied, the rules of conduct to guide scientific inquiries, and the criteria of rigor used in evaluating the merit of the scientific findings. Therefore, it is up to the Chinese management research community (i.e., management scholars in China and international scholars in any region of the world) to chart the course of the community's scientific pursuits. Scientific progress comes from the autonomy of inquiry of those engaging in the scientific endeavours (Kaplan, 1964). Lest the reader be confused, I am not refuting the normal science research paradigm (Popper, 1968), which governs North American research practices, although concern has been expressed on the potential limit of this paradigm in advancing organizational science in the US context by US scholars/editors (Daft & Lewin, 1990). The normal science paradigm specifies the need of both logic and data in the process of seeking true explanations of phenomena. It is also known as empirical science in that the knowledge is a result of the repeated testing or falsifying of theoretical statements about logical relationships among facts or phenomena by using empirical data. Popper (1968) is less concerned about how and where the theories come from as long as they are reasonable *and* falsifiable. Along this line, most Chinese scholars are 'Popperians', a term coined by Mintzberg (2005), in that they focus on using existing theories without a parallel level of concern for the usefulness of the theories or the development of relevant theories. It is the latter that is necessary, based on the insights of this forum's contributors, to advance scientific progress in general and develop locally relevant knowledge in specific.

How can a Chinese management research community realize this ideal in order to chart its own course for the future? The contributors of this forum offer many practical and valuable suggestions. These suggestions are for everyone in the community to implement, not for university administrators or some external regulatory bodies independent of direct involvement from the scientific community. The community has to define the research paradigm that will provide the necessary institutional conditions to support the pursuit of the two roads suggested by Barney and Zhang (2009) or additional options identified by Leung (2009) and Tsang (2009). This cannot and must not be delegated to anyone else for doing so would mean letting others determine the community's destiny. Due to the path-dependent nature of decisions and investments, 'the choices made by these individuals and institutions now will have a huge impact on Chinese management research in the future', and, for Chinese scholars, their 'future is not written in stone, but is written by those who are part of its creation' (Barney & Zhang, 2009: 16). The future of Chinese management research is in the community's own hands.

In essence, the Chinese management research community currently has uncritically accepted the normal science paradigm (Kuhn, 1996). The normal science paradigm consists of three major inter-related activities: articulation of a theory, determination of significant facts, and matching of facts to theory. By building new research on the current body of knowledge, a particular scientific community acknowledges that past scientific achievements, including the major questions, prevalent theories, and popular methods, are the foundation for further practice or study. Yet Kuhn observed, 'the most striking feature of the normal science paradigm is how little they aim to produce major novelties, conceptual or phenomenal' (1996: 35). This is because normal science directs scholars to the phenomena and theories that the paradigm already supplies. The paradigm determines the choice of topics, theories, instruments, and methods. Therefore, scientists in a particular paradigm don't aim to invent new theories and may even be intolerant of those invented by others. A paradigm can insulate the community from important problems (or puzzles) that cannot be stated in terms of the conceptual frameworks and research methods the paradigm specifies. In this way, scientists become slaves rather than masters of the paradigm. The message I wish to convey here is not that the Chinese management research community should discard the normal science paradigm; in fact, it should not. My purpose is simply to encourage scholars to be critical of the usefulness of the major questions, theories, and methods that the current Western paradigm supplies in addressing important phenomena in the Chinese context or elsewhere. Perhaps a 'scientific revolution' in the Kuhnian sense may well be useful. Perhaps the Chinese management research community could learn from poet Shi Su in taking the road less travelled: 'Let me go along!/ Impervious to wind, rain or shine, I'll have my will' (Xu, 2007, as quoted by Cheng et al., 2009: 92).

CONCLUSION

Management and Organization Review offers this forum as a model of the sort of dialogue in which scholars in any emerging scholarly community should engage. There is no basis to believe that the Chinese case is unique. The experiences of the Chinese management research community and the ideas discussed in this forum should offer important implications and relevance for pondering the future of management research in any countries or regions where such research is also emerging.

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