



The Future of Chinese Management Research: A Theory of Chinese Management versus A Chinese Theory of Management

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ABSTRACT Two approaches to the evolution of Chinese scholarship are possible. The first – developing a theory of Chinese management – focuses on applying and refining theories developed elsewhere in a Chinese context. In this sense, the emergence of the Chinese economy represents an important natural experiment for the test and refinement of general management theories. The second – developing a Chinese theory of management – focuses on creating explanations for the existence of Chinese management phenomena that are uniquely Chinese. This approach rejects a research agenda created by Western scholars in favour of a research agenda created by Chinese scholars in order to understand Chinese phenomena. The implications of choosing either of these approaches for the future of Chinese management research and possible relationships between them are discussed.

KEYWORDS Chinese management research, Chinese theory of management, theory development, theory of Chinese management

Two roads diverged in a yellow wood,
And sorry I could not travel both
And be one traveler, long I stood
And looked down one as far as I could
To where it bent in the undergrowth;
Then took the other, as just as fair,
And having perhaps the better claim,
Because it was grassy and wanted wear;
Though as for that the passing there
Had worn them really about the same,
And both that morning equally lay
In leaves no step had trodden black.
Oh, I kept the first for another day!

Yet knowing how way leads on to way,
 I doubted if I should ever come back.
 I shall be telling this with a sigh
 Somewhere ages and ages hence:
 Two roads diverged in a wood, and I –
 I took the one less traveled by,
 And that has made all the difference.

Robert Frost, 'The Road Not Taken' (1921)

INTRODUCTION

In the poem, Frost beautifully describes the challenges associated with making decisions under uncertainty. Often, the advantages and disadvantages of alternative roads are unclear – 'really about the same' – it is often difficult, especially for a single person, to follow more than one road – 'sorry I could not travel both' – and often these decisions are costly to reverse – 'how way leads on to way'. But, the poet reminds us that, as difficult as it is to make these decisions, they must inevitably be made, or we will never travel through 'the yellow wood' and that, in retrospect, in a classically path dependent way, the choices we make end up making 'all the difference'.

Like Frost's traveller, Chinese management scholars face choices about which road to take to develop this field of work. Will Chinese scholarship focus on deductive theory development and testing, or will Chinese scholarship focus on inductive, thick description (Geertz, 1973) approaches to research? Will Chinese scholarship focus on knowledge for knowledge's sake, or will it focus on application and improving firm performance? Will Chinese management professors be trained largely in Western methods and approaches, or are there some uniquely Chinese approaches that will evolve and develop?

However, the options facing Chinese management scholars are, if anything, even less clear than the choices facing Frost's traveller. Frost imagined two pre-existing roads through the wood, while the options facing Chinese management scholars do not currently exist. Instead, these futures are being created right now, by Chinese management scholars, by the Chinese managers and organizations they study, by the reviewers and editors who respond to this research and by management scholars around the world who read it. The choices made by these individuals and institutions now will have a huge impact on Chinese management research in the future. In this sense, the future of Chinese management research is emergent (Mintzberg & McHugh, 1985), the result of a path dependent and evolutionary process whose outcome is difficult to predict, *ex ante* (Arthur, 1990). Studying the emergence of Chinese management research is much like studying the emergence of a new firm or industry – its future is not written in stone, but is written by those who are part of its creation (Alvarez & Barney, 2007).

However the future of Chinese management research evolves, it seems likely that one fundamental question will need to be addressed. That question has been described in many ways by different authors (March, 2005; Meyer, 2006; Peng, 2005; Tsui, 2006; Whetten, 2009). Here, it is posed by presenting two alternative roads for the evolution of Chinese management research, described by the same four words, but in slightly different order. Is the future of this field to develop a 'theory of Chinese management' or a 'Chinese theory of management?'

The purpose of this paper is to describe these alternate roads, to describe some of the implications of proceeding down either one and the challenges of forging links between them. It is suggested that it is unlikely that a single scholar will be able to tread both these roads – the skills required by each are quite different. It is also suggested that powerful institutional forces may make one of these roads appear to be more valuable than the other but that Chinese management research, altogether, would be likely to benefit from some scholars choosing the former road, while other scholars choose the latter.

THEORY OF CHINESE MANAGEMENT

We must learn to do economic work from all who know how, no matter who they are. We must esteem them as teachers, learning from them respectfully and conscientiously.

Mao Zedong, 'On the People's Democratic Dictatorship', June 30, 1949 (1967)

In counseling the people to learn about economic work from anyone, Mao Zedong was implicitly recognizing the advantages associated with taking established theories and approaches and applying them in a Chinese context. Extended to the evolution of Chinese management research, this advice leads to a 'theory of Chinese management'.

Such an approach would apply established theory. This would be the case if a Chinese scholar were studying at the individual, group, inter-group, functional, firm, or inter-firm units of analysis, although the theories applied in these different settings would vary. To this end, a deep familiarity with received theory in organizational behaviour, organization theory, strategic management and international business – at a bare minimum – is a prerequisite for developing a theory of Chinese management.

Of course, it is possible to study Chinese management without any theory. And, given the importance of the Chinese economy to the world, there may be some interest, for a period of time, in the description of Chinese phenomena, *per se*. However, such descriptive work only becomes part of the scientific enterprise if it is connected to theory – either received theories or newly developed theories (Hubbard, Vetter, & Little, 1998; Tsang & Kwan, 1999).

Most of the received theory that can be applied in a Chinese context will have been developed in a Western context (Li & Tsui, 2002; Tsui, Schoonhoven, Meyer, Lau, & Milkovich, 2004). However, applying this theory in a Chinese context may provide some important opportunities to substantiate, extend, or even revise received theory (Tsui et al., 2004). Differences in the Chinese context may ultimately enable management scholars to identify unstated assumptions in their largely Western-based theories of organization and management. Identifying those unstated assumptions, making them explicit and then broadening the theory to incorporate them will have the effect of helping to develop more general theories – theories that, in particular, are applicable outside the Western context within which they were originally developed.

Types of Research in Developing a Theory of Chinese Management

In principle, three kinds of research could be done applying received theory in a Chinese context. First, scholars could identify Chinese phenomena that seem to exemplify some received theory or theories. Studying these phenomena will tend to reinforce the generalizability of these theories since they are apparently able to explain the existence of a particular phenomenon, regardless of the particular cultural or institutional context of that phenomenon.

Much of the research in Chinese management so far has been of this kind – finding Chinese phenomena that exemplify some received theory or theories. For example, the research of *guanxi* generally applies social capital theory (Luo, 2003; Park & Luo, 2001; Peng & Luo, 2000; Tsang, 1998; Xin & Pearce, 1996). And while there are some unique characteristics of *guanxi* as it exists in China, overall, it functions in much the same way as social capital functions in other societies around the world (Park & Luo, 2001; Redding, Norman, & Schlander, 1993; Tsang, 1998). Thus, in this sense, the research on *guanxi*, overall, tends to exemplify most of the basic propositions of social capital theory. Indeed, one could argue that this research is among the best ways yet identified to study the implications of social capital (Park & Luo, 2001).

Second, scholars could identify Chinese phenomena that apparently contradict theories taken from a Western context and then show that such apparent contradictions are not real and that, in fact, the phenomena in question follow the same rules and principles as discussed in previous theory. Some recent work on overseas acquisitions by Chinese firms is an example of this second kind of research (Perkins, Morck, & Yeung, 2006). At first, these acquisitions were difficult to understand using traditional theoretical approaches to the analysis of mergers and acquisitions, applying either agency theory (Jensen & Meckling, 1976) or resource based theory (Barney, 1991), for example. However, when some more detailed information was obtained about how these Chinese firms were funded (through various government subsidies) and the incentives that senior managers in these firms faced when

making acquisition decisions, suddenly the acquisition choices made by these firms became understandable (Perkins et al., 2006) in traditional theoretical terms – in this case, agency theory. In this sense, this research ended up increasing the generalizability of a particular received theory, even though, in the beginning it was not obvious that this would occur.

Of course, the irony behind this second kind of research is that its core effort is to show that apparent differences between China and the rest of the world are just that – only apparent differences. At some point, efforts to extend received theory to explain uniquely Chinese phenomena may have to distort that theory so much, that it may be necessary to develop a new theory (Kuhn, 1962). This, of course, is the third kind of research that can be done in a Chinese context.

This third type of research identifies Chinese phenomena that apparently contradict received theory and then demonstrates that, in fact, it does. This work is hard to do for several reasons. First, relatively few of the current (Western) theories generate sufficiently precise predictions to say, for sure, that what happens in China actually contradicts them. Second, even if some such predictions can be identified and contradictions demonstrated, the review and publication process is very slow to change and conservative in nature. Many reviewers will do their best to show that apparent contradictions between Chinese phenomena and received theory are just that: apparent. That is, reviewers will try to turn this third type of research into the second. For these reasons, this work is difficult to get published.

The difficulty of publishing this work is ironic, of course, since this third kind of research is scientifically the most important that could be conducted in China, at least from the point of view of developing a theory of Chinese management (Kuhn, 1962). Indeed, it could be argued that what makes China important scientifically is that it seems likely that there will be some Chinese phenomena that cannot be explained with received theory and, thus, that require the development of a new theory to explain them (McKinley, Mone, & Moon, 1999).

Of course, if this ‘theory of Chinese management’ road is taken, this new theory could not be applicable in only a Chinese context. In particular, any new theory would have to subsume received theory and apply equally well in a Western and a Chinese context. This is because the goal of developing a ‘theory of Chinese management’ is to create theory that, ultimately, is independent of place. Thus, any revisions to current theory generated by ‘theory of Chinese management’ would necessarily be more general than prior theory and applicable in a variety of specific cultural and institutional settings. Chinese theories that only apply to Chinese phenomena in a Chinese context are unimportant if the goal is defined as developing a theory of Chinese management, because such context specific theories have little to say about general laws and propositions about the behaviour of individuals, groups, or firms. In contrast, as will be shown shortly, this is precisely the goal of efforts to develop a ‘Chinese theory of management’.

Other Implications of Efforts to Develop a Theory of Chinese Management

The decision to develop a theory of Chinese management has some additional implications as well. First, the goal in doing this work would be to publish it in the highest status academic journals in the world. Currently, these are published in English and include such journals as the *Academy of Management Journal*, the *Academy of Management Review*, *Administrative Science Quarterly*, *Journal of International Business Studies*, *Management Science*, *Organization Science*, and *Strategic Management Journal*. These seven journals have been consistently rated as premier outlets in the field (Peng, Lu, Shenkar, & Wang, 2001). Writing papers in Chinese to be published in Chinese journals is of limited value when trying to develop a generalizable theory of Chinese management since the audience for this research is not Chinese scholars, per se, but rather, all scholars throughout the world that work in a particular theoretical domain.

Second, this work would have to follow the conventions of current scientific practice and publication. For example, while in principle this research could be inductive or deductive in character, in practice most of the research published in top journals is deductive. In a similar way, most of this work is quantitative in character rather than qualitative. This does not suggest that inductive and qualitative research will never be published in these top journals, only that most of the work published in these journals over the last several decades has been deductive and quantitative in nature.

Third, in terms of base discipline, most of the work published in the best journals applies either psychology, sociology, or economics or integrates insights from these multiple disciplines.^[1] Efforts to develop a theory of Chinese management that fell outside these institutional norms would probably be unsuccessful.

Fourth, from a practical point of view, scholars, especially junior scholars, committed to developing a theory of Chinese management would be well advised to receive at least some training from senior scholars experienced in Western theories. This could be accomplished in several different ways – everything from visiting for a period of time to receiving their Ph.D. training at a Western school. However, at the beginning of this new millenium, probably the best way to learn the skills needed to develop a theory of Chinese management is to learn those skills through international collaboration with individuals who have been involved in developing the theory of management as it currently exists. And, for the most part, these are senior scholars from the West.

Of course, over time, as more Chinese scholars became skilled at publishing in top journals and developing a theory of Chinese management, the need to learn from Western scholars or to be associated with Western schools will begin to decline.

Limitations of Searching for a Theory of Chinese Management

Focusing one's efforts on developing a theory of Chinese management has several advantages. It gives scholars a clear road forward in their research activities, it presents significant opportunities to broaden and deepen received theory by adopting the 'borrow with the intent to improve' strategy (Whetten, 2009), it helps integrate Chinese scholarship into the broader international 'invisible college' (Price, 1963). But it has potential limitations as well.

In particular, choosing this road lets the research agenda for Chinese scholars be set by the theories and research of prior generations of Western scholars. Even if there is much to learn from Western scholars and theories originally developed in a Western context, there is no reason to believe, *a priori*, that the best way to understand Chinese phenomena is with theories developed out of a Western experience. Indeed, it may well be the case that there are some Chinese phenomena that can only be understood in uniquely Chinese ways. This suggests a second road to the development of Chinese management scholarship – instead of developing a theory of Chinese management, this road focuses on developing a Chinese theory of management. If such a theory is viable – a proposition to be examined below – failure to explore this approach may lead Chinese scholars to fail to study some of the most unique and interesting aspects of the Chinese economy and Chinese organizations.

CHINESE THEORY OF MANAGEMENT

One ought to concentrate on the comparison of China and the West and choose from abroad what is useful at home. However, something even more important is Chinese studies. General knowledge of Chinese studies is most crucial.

Mao Zedong, 'Letter to Xiao Zhixing', September 6, 1915 (1967)

In this quotation, Mao recognizes the importance of learning from the West, but not letting that learning overwhelm what is distinctly Chinese. As applied to the development of management scholarship, Mao is calling for the development of a Chinese theory of management.

In a sense, other countries have developed their own theories of management. This is particularly the case for the USA. Since much (but certainly not all) of the early research on management was done in the USA by US scholars, the resulting theories were, in a sense, a US theory of management. This 'first mover advantage' in the development of management theory, in a sense, forced scholars from around the world to respond to the US version of theory, to either extend it or refute it.

For example, efficient capital market theory in the field of finance is based on the assumption that capital markets are characterized by large numbers of

independent owners (Brealey & Myers, 2003). With the rise of institutional investors, this characterization of capital markets no longer applies even in the USA.^[2] But it has never applied to most capital markets in the world, capital markets that in most countries are dominated by a small number of family owned companies (such as Samsung, LG and Hanjin in Korea). If the first research on capital markets had been done, say, by Chilean scholars, they may well have developed a theory of capital markets that began with the assumption that capital markets are dominated by a small number of family owned businesses (e.g., Copec, Falabella and CMPC in Chile) and the US market could be seen for what it is – an exceptional outlier – rather than what the theory of efficient capital markets has made it – the prescriptively preferred way of allocating capital among firms.

In this way, US based theory that was developed to explain management and organizational phenomena as it existed in the USA set the terms of the discussion in the field of management research for several decades. But, given the unique history and evolution of the Chinese economy, it seems unlikely that a research agenda originally created to understand US managers and organizations would be particularly well suited to understand Chinese managers and organizations (Tsui, 2006). At the very least, this is a proposition that those seeking to develop a ‘theory of Chinese management’ would need to defend and a proposition about which those seeking to develop a ‘Chinese theory of management’ would be very sceptical.

But developing a uniquely Chinese approach to Chinese management is challenging. A Chinese theory of management is likely to be most fruitful in examining Chinese phenomena that can only be understood in Chinese terms and, in particular, cannot be understood using theory derived from Western experience. At first, this might appear to be the same as the ‘third type’ of research described in discussing the development of a theory of Chinese management, i.e., when Chinese phenomena cannot be explained by currently extant Western theory. However, where the goal of this ‘third type’ of research in developing a theory of Chinese management is to develop theory that is ultimately applicable in both a Chinese and non-Chinese context, those seeking to develop a Chinese theory of management focus less on generalizing outside of the Chinese context and more on explaining Chinese phenomena in uniquely Chinese ways. Whether or not these theories apply more broadly is simply not relevant to those seeking to develop a Chinese theory of management.

Such a Chinese theory would almost certainly emphasize the unique history, culture and traditions of China. It would study phenomena that were either unique to China or uniquely important in China. It would focus on managerial and social prescriptions generalizable to only a Chinese context. Thus, this research would generally be published in Chinese in Chinese journals and would only be accessible to the few Western scholars who are fluent enough in Chinese and familiar enough with Chinese institutions to appreciate this work’s unique contributions. For the

rest of the scholars in the world, it might be as if this research and the theory it developed had not occurred; publishing in top tier English journals will increase dissemination of the knowledge.

Occasionally, when this work was first being done, it was not clear if the theory being developed would contribute to a Chinese theory of management or to a theory of Chinese management. This was because, when any work is first done, it may not be clear whether or not it can be integrated into the broader theoretical traditions in the field. If it can – and if that is the objective of those doing this research – this work is part of the effort to develop a theory of Chinese management. If it cannot – and if the objective of those doing this research does not include contributing to broader theories of organization and management – this research is part of the effort to develop a Chinese theory of management.

Consider, for example, the work of Cheng, Chou, Wu, Huang, and Farh (2004) on paternalistic leadership in Chinese organizations. These authors argue that this theory of leadership applies very well in a Chinese context and better than traditional Western theories of leadership. This line of reasoning is consistent with developing a Chinese theory of management.

In contrast, if paternalistic leadership does exist in Western organizations, at least some of the time – and if research on Chinese organizations can help inform research on paternalistic leadership in Western organizations, research that ultimately evolves into a more generalizable theory of leadership – then this research is consistent with developing a theory of Chinese management.

Xiao and Tsui (2007), in another exemplary study, argue that the benefits of structural holes would be less likely to be realized in collectivist national cultures; they bring structural holes theory, originally developed in Western contexts, to the Chinese context to test their arguments. This line of reasoning is consistent with developing a theory of Chinese management as the ultimate goal of this research is to extend the theory of structural holes to be applicable beyond the Chinese context. However, if research in other similar contexts does not offer as much support as this China based study to the cultural boundary of structural holes, these efforts may result in the development of a Chinese theory of management.

However, even if a scholar's objectives do not include developing more broadly applicable theories of organization and management, there may still be some scientifically valid reasons for developing a Chinese theory of management. Indeed, a failure to let go of Western theories in favour of developing Chinese theories when those Western theories simply do not apply reminds one of the individual who lost his keys down the street, but searches for them under the lamp post because that is where the light is. To study uniquely Chinese management issues, it may be necessary to leave the 'light' of Western theory, to develop new sources of light that illuminate previously dark parts of the Chinese economy and Chinese organizations. And going into intellectually dark spaces,

with little prior theory to guide, requires not only intellectual but often personal courage.

Practically speaking, many of the skills required to develop a Chinese theory of management are the same as those required to develop a theory of Chinese management. After all, developing a Chinese theory of management is just as scientific an endeavour as developing a theory of Chinese management. Scholars seeking to develop uniquely Chinese explanations for unique Chinese phenomena must still be very familiar with non-Chinese theories – to avoid the ‘reinventing’ theories already in the literature as well as to ensure that the theories that are developed are actually uniquely Chinese. In this sense, experience with senior Western management scholars may still be a valuable part of the training of Chinese scholars, at least for the next several years. However, the research skills developed through this training would be focused on understanding Chinese phenomena in uniquely Chinese terms. Currently, Chinese theories of management are mainly published in Chinese in Chinese journals. In addition, publishing in English in international scholarly journals would be desirable. 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.

CHOOSING A ROAD

The fundamental cause of the development of a thing...lies in the contradictoriness with [it]. Contradictions within a thing is the fundamental cause of its development.

Mao Zedong, ‘On Contradiction’, August, 1937 (1967)

So, in the ‘woods’ of Chinese management, which road should be taken? Is the evolution of this field like the roads in Frost’s poem – that as ‘way leads on to way’ a road once not chosen is unlikely to be available for choice again? Or, unlike the poet, do we have the unique opportunity to choose one road now, another road later, or even both roads now? As a central premise of dialectal theory, Mao understood the value in pursuing two contradictory roads simultaneously. It is in the tension between the two that new insight is most often generated. This may also be the case with the evolution of Chinese management research.

A Single Scholar Choosing Both Roads

Of course, as there are advantages in pursuing both a theory of Chinese management and a Chinese theory of management, it might seem tempting for a single scholar to try to choose both roads, to enter into both the broader

conversations of a theory of Chinese management and the more focused conversations of a Chinese theory of management. Whether this is possible is ultimately an empirical question. There certainly may be some scholars with the ability to conduct both kinds of work. However, overall, this does not seem likely to be a common choice.

In particular, the conversations that characterize these two roads used to be quite different, even contradictory. Those in the Chinese theory of management conversation may see the efforts of theory of Chinese management scholars as perhaps worshipping foreign things and thus of little value in contributing to indigenous knowledge and managerial practice; theory of Chinese management scholars, however, may not possess the language – both theoretical and actual – required to appreciate Chinese theory of management work and thus discount its importance. Fortunately, these discords have been narrowed considerably with increased dialogue among scholars inside and outside China. A good example is the creation of the International Association for Chinese Management Research (IACMR) and its official journal, *Management and Organization Review (MOR)*, in 2004 and 2005 respectively. As excellent platforms for scholarly conversations on Chinese management research, *MOR* and IACMR's biennial conference have helped, and will continue to help, scholars to better understand what is occurring on both roads. Scholars on both roads have gradually come to the consensus that these two roads, though quite different, are both necessary and important.

Apparently, to become skilled at contributing to one of these conversations will require enormous dedication and focus. To actively contribute to both seems very challenging. Of course, this does not mean that scholars, at different points in their career, might not specialize in these different types of work. These individuals might be particularly vital in facilitating learning between these two scholarly roads.

Learning Across Roads

That, as a practical matter, it is likely to be rare for a single scholar to make contributions to both the theory of Chinese management and the Chinese theory of management literatures, a variety of institutions will need to be developed to facilitate whatever learning might take place between these two types of scholarship. One could imagine, for example, conferences with two tracks – a theory of Chinese management track and a Chinese theory of management track – where scholars are free and even encouraged to attend sessions that are inconsistent with their primary research focus.

What can be learned in such sessions is, of course, difficult to anticipate. At the very least, Chinese theory of management scholars may learn from their theory of Chinese management colleagues what theories, approaches and phenomena they

do not want to study. Theory of Chinese management scholars may become aware of some uniquely Chinese phenomena that may enable them to generalize their theories. While the specifics of what might be learned on one road vs. the other cannot be easily anticipated, that there is potential for significant learning is not difficult to anticipate.

Institutional Pressures

Currently, enormous institutional pressures are building, especially for young Asian scholars, to focus their efforts on contributing to a theory of Chinese management. This is indicated by a growing emphasis to publish in Western journals in English. While it may be easy to criticize the abandonment of the development of a Chinese theory of management as a manifestation of the lack of intellectual self-confidence on the part of Chinese scholars (Meyer, 2006), a longer-term view suggests that contributing to a theory of Chinese management first, in the early stages of one's career, could provide the legitimacy required to help develop a Chinese theory of management later in one's career.

However, given that each research road may be valuable and that those who travel these different roads can have much to learn from each other, what is important for the field of Chinese management is that there are multiple scholars each pursuing one of these roads. To abandon the one – the development of a Chinese theory of management – in favour of the other – a theory of Chinese management – may ultimately put at risk much of what can be learned by studying Chinese organizations and management.

NOTES

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- [1] Debates about whether or not management, per se, is an academic discipline are beyond the scope of this paper. However, even if management is a separate academic discipline, its intellectual roots are typically found in the social sciences listed.
- [2] In 2003, US institutional investors controlled \$19.634 trillion in assets in the US capital market and have continued to hold 19 percent to 20 percent of total equity assets. See The Conference Board and Institutional Investment Report 2005: US and International Trends – Report 1376, September 2005 (available at <http://www.conference-board.org>) (Brancato & Rabimov, 2005).

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