



## Perspectives—From China Strategy to Global Strategy

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**Abstract.** This article argues that starting with substantial divergence, China strategy research and global strategy research are now converging. This scholarly transformation is largely driven by the recent rise of the Chinese economy, which has emerged from being a peripheral member of the global economy to a core contributor. I trace some of the early roots of China research in our field, outline the beginning of my own China research, and discuss my more recent research which has become more global incorporating substantial China and non-China elements. In addition, I use the emergence of the institution-based view of strategy, which has been largely propelled by China research, to shed light on how China research may make more global theoretical contributions beyond the immediate context of this research. Finally, to further push China research to the center stage of global strategy research, I recommend that scholars “act local, think global.”

**Keywords:** China strategy, global strategy, institution-based view

*The fundamental problem of China research is that the concepts and theories favored in the disciplines were developed out of assumption about systems which operate quite differently from China, which is more a civilization pretending to be a nation-state. In fact, the very concept of “system,” that is, a set of interacting variables in which there is a close fit or matching of cause and effect, has to be greatly modified in the case of China*

—Lucian Pye (1992: 1162)

How to conduct and publish China research in the global community of scientific research? For the same reason that China competes with numerous competitors in the global economy for precious foreign direct investment (FDI) dollars, China research has to compete with numerous pieces of non-China research for precious journal space. Although some may argue that China research is inherently interesting, given the high rejection rates of major journals,<sup>1</sup> there are significant opportunity costs for editors and journals to publish China research—that is, more non-China research will have to be rejected.<sup>2</sup> Therefore, the challenge confronting all researchers interested in successfully pursuing and publishing China research is how to make contributions which are significant enough to justify publication in the highly competitive global marketplace for ideas.

My goal in this article is not to address the “how to” question head on, because I believe that I still have a lot more to learn in this journey and that I do not have a reasonably complete answer (or formula). What I would like to share here is how I have approached this question since the early 1990s. This article, therefore, is essentially a one-person case study based on highly idiosyncratic data (my own experiences). However, I do believe that tracing the evolution of some of my own China (and non-China) research can help us reflect on the evolution of the larger literature and speculate about its future directions. My central thesis in this article is that starting with substantial divergence, China strategy research and

global strategy research are now converging. Therefore, a global perspective is imperative when endeavoring to push China research ahead (Peng, 2006; Tsui et al., 2005).

Before proceeding, it is useful to clarify that in this article, “strategy research” is defined broadly. A more accurate label would be “strategy, organization, and international business (IB) research.” Given the obvious need for compositional simplicity, in this article the “strategy research” label is used.

### **China is China, business is business?**

There is no doubt that the modern literature on strategy, organization, and IB was dominated by Western (primarily U.S.) research in the second half of the 20th century (Boyacigiller and Adler, 1991; March, 2005). Until very recently, China had simply not been on the “radar screen” of such research. This, of course, is not only a problem of our field, but also a problem affecting most social sciences (Pye, 1992). In the past, to the extent that social science inquiry on China existed, it was largely relegated to the domain of area studies—as exemplified by the journal *China Quarterly*, which is an excellent journal in itself but is not widely read and cited outside the “China studies” field. As a result, China research was set apart from mainstream, discipline-based research (Lau, 2002). Even when scholars publish China research in mainstream outlets, such as Tung (1981a) in the *Academy of Management Review*, such work tends to be ignored and marginalized. As of this writing (February 2005), according to the Social Sciences Citation Index, Tung (1981a) has generated a grand total of only nine citations.<sup>3</sup>

Social science research of course does not take place in a vacuum. Research in our field is especially interested in its relevance. Therefore, China’s long-time isolation from the outside world until the late 1970s and its more limited participation in the global economy in the 1980s contributed to the perception (rightly or wrongly) that China research was not relevant. My friend and colleague, Oded Shenkar, shared with me that when he proposed a Ph.D. dissertation on “China business” in the late 1970s (his dissertation was eventually finished in 1981), his advisor pointed out: “China is China, business is business—Choose either one, but don’t mix the two!” While this perception made sense in the late 1970s, it perpetuated into the 1990s. In 1994, when I proposed a “China business” dissertation to my own Ph.D. advisor, Charles Hill at the University of Washington, he similarly persuaded me to give it up. I have no doubt that these advisors acted in the best interest of their students—I am personally grateful for Charles Hill’s vote of confidence in my ability to pursue and complete doctoral studies and for the training he provided which I believe is second to none. Nevertheless, the two remarkably consistent pieces of advice, provided over a span of over 15 years (the late 1970s—the mid 1990s), speak volumes about the field’s “conventional wisdom” with regard to China research.

In general, “the most talented scholars gravitate to the conventional and the paradigmatic where their talents lead to reliable success,” according to March (2005:10), who continues: “Talented individual scholars who, either by choice or by necessity, identify with a regional fragment become unwitting altruists, sacrificing their clearest chances for recognition in order to participate in unlikely exploratory gambles that serve the field rather than themselves.” Because (1) I was selfish (or at least not “altruistic” in the words of March), (2)

I received a clear piece of “no go” advice, and (3) I wanted to hedge my bets, I decided to pursue a non-China dissertation, which applied transaction cost, agency, and resource-based theories to explore the performance determinants of U.S. export intermediaries. It was eventually completed in 1996 and published as a book (Peng, 1998) and three journal articles. To give Charles Hill some credit, he did encourage me to pursue my China research “on the side”—outside the scope of my dissertation. However, over time, it is very clear that my China research, originally regarded as a “sideline” project by my advisor, has now become the mainstream of my research. What is heartening is that the academic and professional market for ideas has clearly spoken for itself: There is a significant demand for knowledge on China business, which my work has helped to partially fill.<sup>4</sup> To be sure, my initial journey was frustrating and challenging, because I had to be on top of two essentially unrelated literatures, between which there was relatively little synergy. In retrospect, however, simultaneously dealing with two unrelated major research projects as a graduate student, by my own choice, forced me to become a broader-based and more efficient scholar, thus significantly enhancing my research career afterwards.<sup>5</sup>

Looking back, my research in this area started with a China focus, and then “diversified” to incorporate other transition economies in Central and Eastern Europe (CEE)—the words “transition economy(ies)” thus often appear in the title of my publications (e.g., Peng, 2000). More recently, I have further broadened the scope of my inquiry by endeavoring to cover other emerging economies which are neither China nor CEE (such as Hong Kong, South Korea, and Thailand)—consequently, the title of my more recent publications increasingly sports the term “emerging economy(ies)” (e.g., Tan and Peng, 2003). As my research becomes more globalized, it seems natural to label such work *Global Strategy*, which is the title of my new textbook (Peng, 2006). Overall, tracing the evolution of my own research, there is a pattern of moving from focused China strategy research toward more global strategy research. To the extent that my own research does not operate in a vacuum, I believe that this journey is part of the evolution of the larger field, which increasingly appreciates and incorporates China research into the global strategy research enterprise.

I believe that the main reason behind this scholarly transformation of our field—of no longer marginalizing China research and now incorporating China research into the center stage of global strategy research—is the rise of the Chinese economy in the last decade or so. As the Chinese economy emerges from being a peripheral member of the global economy to a core contributor, researchers and journals in our field have become more interested in China research.<sup>6</sup> Otherwise, they run the risk of being “irrelevant.” For example, Peng et al. (2001c) report that China-related articles published in ten leading journals increased from 22 during the entire 1980s to 61 during 1990–97. Similarly, Li and Tsui (2002) find that 60% of the 226 China-related articles published in 20 leading journals during the period 1984–99 appeared during the 1990s. In other words, as a new generation of China scholars, we are blessed by the time in which we are developing our career. Otherwise, if we attempt to push and market China research when it is ahead of “its time,” the example of Tung (1981a) mentioned earlier gives us an idea of what we could have been facing. So what led China research to move from being “in the wrong place at the wrong time” not long ago to becoming “in the right place at the right time” more recently? The next two sections trace some of this journey and outline some of my own participation.

### **Early China research**

During the 1980s and early 1990s, early China research in our field focused on a key question: Can we apply mainstream theories in a radically different environment? The answers provided during the time between the publication of Tung (1981a) and Shenkar and von Glinow (1994) were usually “Doubtful!” The very first piece of China research published in the *Strategic Management Journal* (Tan and Litschert, 1994) reported an interesting finding: Strategic management as we know it in the West actually exists in China! To the extent that journal editors require theoretical and empirical value-added in the papers that they publish, these findings, which seem primitive today, were regarded as breaking new ground at that time.

Because (1) mainstream theories in our field are all developed from the experience of Western (primarily U.S.) firms and (2) the United States is unique even among developed economies in that the role of state-owned enterprises (SOEs) has always been minimal, it is not surprising that early China research, which focused on the dominant organizational form in the country at that time—namely, the SOE—had a hard time being accepted for publication and becoming influential and widely cited even when published. In other words, U.S. dominance of the field (Boyacigiller and Adler, 1991; March, 2005) led to China research in the 1980s and early 1990s (mostly on SOEs) falling outside the radar screen of mainstream journals. The only other major organizational form in China during the 1980s, “collective enterprise,” was even stranger in the eyes of mainstream theories.

Since the 1990s, China research has gained more legitimacy in part because of substantial FDI and of significant entrepreneurship in the Chinese economy, thus making it easier to apply and extend IB theories (Luo and Peng, 1999) and entrepreneurship theories (Peng, 2001), respectively, which mainstream researchers can more easily relate to. In addition, SOE research in itself has gained more legitimacy. This is primarily due to the privatization movement elsewhere in the world—but not in China. Overall, the SOE share of global GDP has declined from more than 10% in 1979 to approximately 5% at the dawn of the 21st century (Megginson and Netter, 2001). Correspondingly, a voluminous literature on privatization has grown. However, both scholars and policymakers quickly realize that in the absence of a solid understanding of the nature of SOEs, privatization policies may be irrelevant, counterproductive, or, in the worst case, disastrous (Peng, Buck and Filatotchev, 2003). Therefore, more SOE research is needed (Peng, 2000). In other words, although China has not officially “privatized” any SOE and only “informal privatization”—in the form of management leasing, buyouts, and public listing—has taken place, research on Chinese SOEs has benefited from the global interest in privatization and in SOEs themselves (Tan and Peng, 2003).

### **The beginning of my own China research**

Born and raised in Shanghai, I had always been interested in China before I embarked on my undergraduate studies in the United States in 1989. After I entered the Ph.D. program in strategy at the University of Washington in Seattle in 1991 (I never had a master’s degree), I started to more systematically search for opportunities for China research. It was a very

lonely experience, in part because of the paucity of published China research in mainstream journals I scanned.<sup>7</sup> After going through *each* issue of the mainstream journals in our field, I was startled by how few China articles were published. During the entire 1980s, a total of nine China-related articles appeared in the six mainstream journals<sup>8</sup>—on average slightly more than one article for one journal in one decade (Peng et al., 2001c: 97–98). In addition, another reason why my experience in pursuing China research as a Ph.D. student was lonely was because of the lack of like-minded faculty and doctoral students. Being the only Chinese student in my department did not help promote my “exotic” interest in China.

The clearly identifiable triggering event which later led to a stream of my published China research was the very first week of my first strategy seminar taught by Charles Hill in winter 1992. The first reading was Penrose’ (1959) book, *A Theory of the Growth of the Firm*. While as usual in Seattle, it rained very hard in January, Penrose’ work (probably in combination with the rain and Charles Hill’s lecture) struck a chord in me. From what I had known, the Chinese economy, consisting of thousands of firms, was rapidly growing at that time. While numerous economists had studied economic growth at the country level, firm growth—that is, at the firm level—had been studied by fewer scholars. To the best of my knowledge, firm growth in China had never been investigated before.

How do firms in China grow? This seemed to me to be a theoretically interesting, empirically unexplored, and practically important question. Not having an established prior literature to guide this research is of course both a threat and an opportunity. Despite Penrose’ (1959) more recent influence in the 1990s (primarily because of the rise of the resource-based view), Penrose-style research on the growth of the firm had not been among the list of “safe” topics for researchers in both economics and strategy in the first three decades since the publication of the 1959 book. Applying the Penrose-style thinking to a non-mainstream context such as China was even riskier. Nevertheless, I immediately fell in love with Penrose’ theory. In retrospect, I now believe that “path dependencies” matter. Most people would probably have the strongest memory of their first dates and readings. For example, I no longer remember the fifth or sixth topic we covered in my first strategy seminar but continue to have a vivid recollection of how the first topic (Penrose) was covered—perhaps this was “love at the first sight.”

Despite this initial excitement associated with the discovery of Penrose, I quickly realized that simply applying the Penrose-style thinking would not be sufficient for my China research, because, after all, China was China whereas Penrose’ work was grounded in the West. To truly develop a relevant theory for the growth of the firm in China, additional theoretical tools would be necessary. The challenge, of course, was *which* additional theory(ies). As the seminar progressed, each week Charles Hill led our discussion to cover a new theory and I mentally attempted to apply every one of these newly introduced theories to my yet-to-be-written term paper, which, after the first week, I had already decided to focus on Penrose and China. In addition to Penrose, I was also deeply impressed by North’s (1990) book, *Institutions, Institutional Change, and Economic Performance*, which Charles Hill introduced in a later week.<sup>9</sup> At the same time, I selected my minor to be sociology and took a number of sociology theory courses. Through these studies, my thinking started to strongly resonate with institutional theory. Although institutional theory started to gain legitimacy in our field in the early 1990s, it had become a more established theory in sociology about 10 years

ago (DiMaggio and Powell, 1983). Consequently, the cross-fertilization of the economic version (through Charles Hill's seminar) and the sociological version (through sociology courses) of institutional theory led to a moment of inspiration approximately several weeks into the quarter (in January or February 1992) that institutional theory—regardless of its disciplinary roots—would generate the best theoretical mileage for my China research.

Like an undergraduate student taking a multiple choice exam, I also examined other possible theories one by one. Among the leading theories, I found transaction cost theory to be static, resource-based theory to be underdeveloped (bear in mind that Barney [1991] had barely been published when I started my doctoral studies), and population ecology theory to be irrelevant (considering the visible hand of the Chinese government in shaping the birth and death of firms there).<sup>10</sup> In contrast, institutional theory excels in its ability to explain and predict complex, changing dynamics such as institutional transitions and firm responses. Overall, it seems imperative that if we endeavor to enhance our understanding of a rapidly moving beast such as the growth of the firm in China, we need to take advantage of the most powerful theoretical cage that can best help us capture the beast. I felt that institutional theory represented such a good cage in 1992, and I continue to think so as I write the present article. More importantly, I believe that institutional theory is not only helpful for China research, but also helpful for advancing global strategy research. In my recent work, I have endeavored to develop an institution-based view of strategy (more on this later in the section on “Theoretical Contributions”).

Back in winter 1992, I wrote my term paper for Charles Hill's seminar, which received some very encouraging and critical feedback—in the middle of my first year of doctoral studies. These theoretical ideas were later published in my first academic publication, Peng (1994) in the *Advances in International Comparative Management*, an annual volume of research papers. With the help of a fellow doctoral student, I pushed these ideas further and eventually published Peng and Heath (1996) in the *Academy of Management Review*. An empirical follow-up, drawing on three case studies of Chinese firms, was published as Peng (1997) in *Organization Studies*. In retrospect, pursuing these papers was risky. China research, as discussed earlier, was not regarded as mainstream in the early 1990s. A Ph.D. student pursuing such research in the absence of faculty blessing would be especially vulnerable. My solo and first authorships on these pieces reflected a sad reality that I had nobody to work with on such a non-mainstream topic (Peggy Heath, who helped me mostly with copyediting on the 1996 paper, was a fellow doctoral student who later dropped out). For me, given my agreement with Charles Hill to write a non-China dissertation, working on these China papers, which Charles called a “sideline project,” had a significant opportunity cost. They directly took time away from my dissertation<sup>11</sup>—this is something which I now advise my own Ph.D. students to *avoid*. As an academic entrepreneur (or, if we may, a risk-taking “maverick”), I persisted because, having agreed to do a non-China dissertation, I wanted to follow my passion and also because I had fun doing China research.<sup>12</sup>

Since my graduate school days, I have deliberately sought to expand my China interests to cover other countries and regions, the first of which is other transition economies in CEE (such as Poland and Russia) which have gone through some similar market-oriented transitions away from central planning. This was in part driven by my inherently “imperialistic” trait as a scholar endeavoring to have a wider influence in the world beyond China, because

I have always been of the opinion that China-only work will have limited appeal outside the community of “hard core” China scholars. In addition, my interest in addressing CEE was also in part driven by my defensive needs to make my work more generalizable so that editors and reviewers cannot easily reject my papers by arguing that “We are not interested in papers dealing with a single country (other than the United States!).” Therefore, my research has often been relentlessly comparative, not only comparing China with the United States, but also comparing China with CEE (and other regions). These efforts eventually culminated in my second book, *Business Strategies in Transition Economies* (Peng, 2000), which probably was the first book in our field to explicitly compare and contrast business strategies in the two major regions of transition economies, China and CEE. This strategy of regional diversification and expansion has paid off, since my papers, despite their strong China roots, are not only read and cited by China scholars, but also by scholars interested in other regions such as CEE.<sup>13</sup>

While I certainly felt lonely when embarking on China research as a graduate student in the early 1990s, by the end of the decade, I no longer felt so, having served on the faculty of two schools which deliberately focus on Asia/China research, the University of Hawaii (1995-97) and the Chinese University of Hong Kong (1997-98). In fact, China research became *hot* (!) by the end of the 1990s. An *Academy of Management Journal* special issue on business strategies in emerging economies attracted a record number of 75 submissions, of which 27% dealt with China, the most studied area (Hoskisson, Eden, Lau, and Wright, 2000:260). I was pleasantly surprised to find that I authored four out of the 100 references cited by the guest editors in their editorial (Hoskisson et al., 2000), thus becoming the most cited author in this group of contributors to the references.<sup>14</sup> There was no question at that time that my “sideline” interest had become the mainstream of my research.

At the turn of the century, I felt that despite the challenges and frustrations, China research—just like the booming China business—would be rich in opportunities and that I made the right choice. Now with 20/20 hindsight, I can use the six questions for choosing research topics suggested recently by Tung (2005) to help explain this crucial strategic choice for my career (see Table 1). Although in reality my thought process at that time was not so systematic, it did touch on all these six components.

### **My recent China and non-China research**

After firing my first salvo—projects initiated during my Ph.D. studies—I started to plan my next round of China research in the late 1990s. I was fortunate to work at the right place at the right time. At the University of Hawaii, I met Oded Shenkar and Yadong Luo, with whom I coauthored four articles. At the Chinese University of Hong Kong, I teamed with David Ahlstrom, Kevin Au, Yuan Lu, Denis Wang, and Michael Young. Our collaboration has resulted in five publications. Other than my colleagues at the same institutions, I collaborated with two like-minded China scholars, Chao Chen (Rutgers) and Justin Tan (Creighton), generating four articles. Finally, since joining the Ohio State faculty in January 1999, I have attracted a talented group of Ph.D. students, Yi Jiang, Seung-Hyun Lee (now at UT Dallas), Tony Tong (now at SUNY Buffalo), and Qi Zhou. Thus far, my joint research with my own Ph.D. students has resulted in five publications. Overall, such

Table 1. Why choose to conduct China strategy research? Answers to Tung's (2005) six questions.

Questions	Answers
1. What are the significant and important trends that have broad implications for theory and practice in the future?	The rise of the Chinese economy as a more integrated and more important part of the global economy has significant implications for theory and practice in our field
2. Is the topic sustainable over an extended period of time and not just a fad?	Since the late 1990s, the topic appears sustainable over an extended period of time (although not necessarily indefinitely)
3. Will the topic be broad enough to generate interest among a sufficiently large group of researchers?	From a small group of devoted researchers, more and more scholars previously not interested in China are now becoming interested
4. How much research attention has the topic received thus far?	Despite the rising interest, overall the topic has not received a great deal of attention (due to the field's historical neglect), thus affording more opportunities to contribute to the literature
5. What is my competitive advantage in this area?	In addition to my Chinese roots (which are not rare in our field now), my earlier investment in the early 1990s gave me some first mover advantages (which are relatively rare).
6. Am I truly passionate and excited about the topic?	Yes!

Source: The questions are from R. Tung, "New era, new realities: Musings on a new research agenda . . . from an old timer," *Asia Pacific Journal of Management*, 2005 (this issue). Cited with the author's permission.

collaborations have not only led to a series of new publications, but also some very rewarding friendship.

Since my graduate school days, I have always been very *programmatic* in planning for and executing my research. Specifically, I would always try to write a theory paper when probing into a new topic (e.g., Peng and Heath, 1996), then engage in some qualitative, case-based research (e.g., Peng, 1997), and culminate in some more systematic quantitative studies (e.g., Peng and Luo, 2000). Beyond this linear progression, I would either write a book to leverage the learning from this series of papers (e.g., Peng, 2000), or author new journal articles in neighboring areas (both substantively and geographically). Often, through these activities, I become exposed to multiple streams of the literature and learn from the insights of my coauthors, who are often a source of my inspiration. Therefore, I would start a new round of theorizing (e.g., Chen, Peng and Saporito, 2002; Peng, 2003; Young et al., 2002), which would inform the next round of empirical inquiries. This research strategy, while linear during the planning stage, becomes messy in actual implementation, mostly because of (1) the need to accommodate multiple coauthors' concerns and schedules, (2) the interest in flexibly going after some previously unknown data, and (3) the reality of multiple rounds of reviews (and delays and rejections!) at different journals. Nevertheless, I do believe that programmatic and systematic efforts, to the extent possible, are helpful in pursuing a stream of research and making an impact.

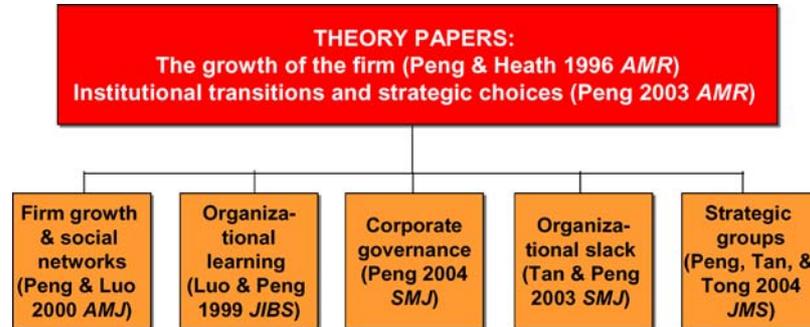


Figure 1. China research, mainstream topics.

In terms of topic selection, I am always of the opinion that if China research aspires to be accepted by mainstream journals, addressing mainstream topics—while invoking a China perspective—is imperative. In other words, topics which are perceived to be too China-specific may often fail to generate sufficient interest among journal editors and reviewers. As noted earlier, I started my China research with a focus on the growth of the firm (Peng, 1994, 1997; Peng and Heath, 1996), which is a central problem confronting firms of all stripes around the world (Penrose, 1959). More recently, I have worked on firm growth and social networks (Peng and Luo, 2000), organizational learning (Luo and Peng, 1999), corporate governance (Peng, 2004b), organizational slack (Tan and Peng, 2003), and strategic groups (Peng, Tan and Tong, 2004) (see Figure 1 for a summary). Each of these topics is theoretically driven by a stream of the mainstream literature. Yet, often before my studies, there was no study on these topics using China data. Therefore, by adding a China flavor, my research has endeavored to inform and enrich the important global debate on some of these topics, such as (1) whether outside directors on corporate boards help improve financial performance (Peng, 2004b), (2) whether organizational slack improves or inhibits firm performance (Tan and Peng, 2003), and (3) whether ownership can be used to predict strategic group membership (Peng et al., 2004).

In addition to China research, I have also been increasingly involved with non-China research, because of the combination of push and pull effects. The push effect is mainly due to my own China research, in which I not only seek to become well versed in the theoretical literature in the field and empirical realities on China, but also keep my eyes open for similar phenomena in other countries in order to provide a sound basis for comparative work. In other words, my intellectual curiosity in knowing more about “What is going on?” in many parts of the world has pushed me to “diversify.” For example, my teaching, which often uses Japanese examples and cases, led me to become curious about the Japan literature, which eventually resulted in a paper on “The *Keiretsu* in Asia” (Peng, Lee and Tan, 2001b). The pull effect is primarily due to my interactions with many colleagues around the world, who have read some of my China research. They often propose to do joint research with me—or vice versa—based on some of their data collected from other parts of the world.

Consequently, I have worked on a variety of interesting topics on a more diverse set of countries with several groups of like-minded colleagues. Other than those named earlier,

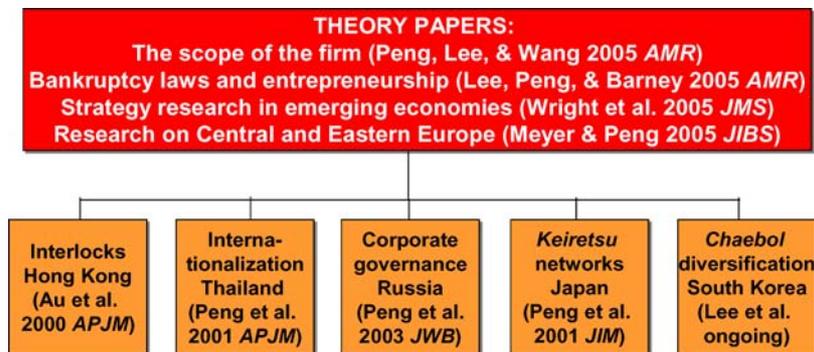


Figure 2. More global research beyond China.

my collaborators include Garry Bruton (Texas Christian University), Trevor Buck (Loughborough University), Igor Filatotchev (King's College London), Bob Hoskisson (Arizona State University), Keonbeon Lee (Korea Institute of Finance), Keun Lee (Seoul National University), Klaus Meyer (Copenhagen Business School), and Mike Wright (University of Nottingham). Shown in Figure 2, empirically, I have worked on interlocking directorates in Hong Kong (Au, Peng and Wang, 2000), internationalization of firms in Thailand (Peng, Au and Wang, 2001a), privatization in Russia (Peng, Buck and Filatotchev, 2003), Japanese *keiretsu* networks in Asia (Peng, Lee and Tan, 2001b), and corporate diversification in South Korea (Lee, Peng and Lee, 2004).

Relative to my China research, this group of non-China research is not as programmatic, in part because of (1) my relative lack of systematic knowledge of these countries and (2) my more limited involvement in some of the collaboration, which was often driven by my coauthors' interests and their data. Nevertheless, I have always endeavored to maximize my learning theoretically, by taking advantage of these collaborative opportunities to inform my new series of theory papers. Three of my four most recent theory papers shown in Figure 2 are *not* significantly driven by my China research: Peng, Lee and Wang (2005) deals with corporate diversification, Lee, Peng and Barney (2005) focuses on institutions and entrepreneurship, and Meyer and Peng (2005) concentrates on CEE research. Instead, these papers are the result of my more global research interests.

Overall, after completing a series of papers initiated during my Ph.D. studies, I have pursued a variety of China and non-China research opportunities in the last ten years or so. Just as China business itself increasingly takes on a more global flavor (as China becomes the main battleground for multinational enterprises of all stripes), my own research has become more global. It was especially heartening when the executive editor at the world's largest college textbook publisher, John Szilagyi, who was in charge of the leading strategy textbook in the field (Hitt, Ireland and Hoskisson, 2003), came to invite me to author a new textbook titled *Global Strategy* in 2002. He read a number of my previous publications (especially Peng, 2000), spoke with a large number of colleagues, and believed that I was "it." Evidently, Szilagyi and his colleagues thought that my work was more than about China and was global enough. Inviting a scholar who is best known for China strategy

research to author a new global strategy text is indicative of the convergence of China strategy research and global strategy research, as far as the “market” is concerned. Now having written this new *Global Strategy* textbook (Peng, 2006), I sincerely hope that this book will help promote this convergence.

### Theoretical contributions

Having undertaken a variety of research projects, as sketched above, a bigger and more important question that I have never addressed in all previous publications is: What are the contributions to theory development in general? Writing this article has given me an opportunity to reflect on this question. While dealing with Asian management research more broadly, Lau’s (2002: 171) comments, which I agree, pertain to China research as well: Although the number of studies is not small now, “the quality of these studies has not always been of a sufficient standard to influence mainstream research. Most of these studies are explorative, descriptive, and comparative in nature, and do not make substantial theoretical contributions.” I am the first one to admit that some of my own work shares these problems identified by Lau (2002)—and also by White (2002).

The question of theoretical contributions is especially important for China research, because the urge to build China-specific theories, fueled by a mentality about “Chinese exceptionalism” (that is, China is unique in the world), is often felt both inside and outside of China—and both inside and outside of academia (Chen, 2002; Pye, 1992; Shenkar, 2005). Is China unique in the world? The answer is of course *both* “True” and “False” at the same time, depending on one’s perspective. A more relevant question is whether China justifies a unique body of theories which differs substantially from mainstream (that is, primarily Western) theories (Pye, 1992). My answer is “No,” although existing (Western) theories often need to be adapted within a Chinese context. To use March’s (2005: 10) eloquent words: “A missionary group that isolates itself from society in order to protect its distinctiveness maintains its purity but finds itself handicapped in its efforts to penetrate that society with its message.” To the extent that China research relies on and thrives within the global society of scholars in our field (March, 2005), China-specific theories will probably be appreciated more within China (and within the “missionary group” of strong believers of Chinese exceptionalism), but will probably fail to make much of an impact elsewhere. To use the jargon in our field, in theory-building, while the limitations of a “global strategy” (in principle one set of best theories for the entire world but in reality these theories tend to be U.S.-centric) are increasingly exposed (Boyacigiller and Adler, 1991; March, 2005), a “multidomestic strategy” (China should have its own body of theories, so should Japan, Russia, France, etc.) probably will not go very far either. Challenging as it is, we need to search for the elusive but ideal “transnational solution” (Bartlett and Ghoshal, 1989) in theory-building, which combines both context-free and context-embedded elements.

I would like to share some personal experience on this. I was once approached by a group of friends who invited me to work with them on a “theory of *guanxi*.” I do agree that *guanxi* has become the most influential Chinese business word, which now often appears in mainstream media such as the *Wall Street Journal* and *The Economist* with no explanation provided in brackets—in fact, I often tell all my undergraduate and MBA students to know

this word, even if they would only pick up one Chinese word. However, scholarly, I disagree with the rationale for the development of a “theory of *guanxi*,” which presumably would be unique to China. This is because we already have such a theory in the literature, although it is not labeled a “theory of *guanxi*.” It is known as a theory of interpersonal networks or interfirm relationships—or both (Peng and Luo, 2000). Despite the recent visibility of the Chinese word *guanxi* in the media, almost every culture has a word or two to describe what the Chinese call *guanxi*. The Russians call this *blat*, the Vietnamese name this *guan he*, and the English-speaking peoples label this “old boys’ (or girls’) networks.” In other words, it would be very difficult to market a paper developing a “theory of *guanxi*” to mainstream journals, which would probably not view this theory to be unique. Based on this reasoning, I turned down the proposal.

Although this isolated incident took place in the late 1990s, I think it is useful, in post-hoc justification, to cite Lau’s (2002: 177) more recent remarks: “there is a need to guard against uncritical acceptance of all ideas originating from Asia as being unique, without explaining their broader existence and applicability.” White (2002: 296) has gone further by arguing that the use of Chinese words such as *guanxi* in theory-building “may serve as convenient shorthand to refer to complex phenomena particularly relevant in these contexts, the danger is that they may foster conceptual parochialism and create cognitive barriers to seeing relationships with similar phenomena in other contexts.” In addition, White (2002: 306, added italics) argues that the “use of local terms such as *guanxi* can be *dysfunctional* when it blinds researchers to conceptually equivalent phenomena in other contexts, and also perpetuates stereotypes of individuals and organizations in the region with which such terms are linked.” Given the difficulties one experiences when turning down a collaborative proposal from trusted friends (I hurt my *guanxi*!), in retrospect, I wish I could have had read and cited Lau (2002) and White (2002) when explaining my decision to my friends.

On the other hand, I share views with Child (2000), Lau (2002), March (2005), and White (2002) in that China research can and should strive for making theoretical contributions that cut across different contexts with global ramifications beyond the Chinese world. “Research in the Chinese context plays a role in clarifying context dependencies. It helps to expose the limitations of ideas that are accepted as context-free but that reflect a particular political or cultural history” (March, 2005: 14). In addition to exposing limitations of some existing theories, I believe that China strategy research (and more broadly, strategy research on emerging economies) has made a significant theoretical contribution by identifying an institution-based view as a new leg of the “strategy tripod” (Peng, 2002, 2006). In my view, the institution-based view of strategy, which is first formally proposed in Peng (2002), can be positioned in parallel with the traditional industry- and resource-based views. Industry- and resource-based views, grown largely out of research in developed economies, essentially assume institutions as given and institutions, thus, never emerge from the “background.” In emerging economies such as China, the importance of institutional conditions and their transitions is magnified, thus bringing institutions in the foreground and affording scholars with an opportunity to capitalize on this context not only as a test site for existing theories but also as a breeding ground for new theoretical perspectives (Peng, 2003, 2006).

There is no doubt that strategy research, thus far, is underpinned by the industry- and resource-based views. What is largely missing is the influence of formal and informal institutions as the “rules of the game” (North, 1990) on strategic choices. To be sure, the influence of the “environment” has long been featured in the literature (Lawrence and Lorsch, 1969). However, what has dominated this research is a “task environment” view, which primarily focuses on economic variables such as market demand and technological change. Until recently, scholars had rarely looked beyond the task environment to explore the interaction among institutions, organizations, and strategic choices (Oliver, 1997; Peng, 2003; Peng and Heath, 1996). Instead, a market-based institutional framework has been taken for granted and formal institutions (such as laws and regulations) and informal institutions (such as norms and cognitions) have been assumed away as “background” conditions.

Today, especially as demonstrated by research on China and other emerging economies, the field has become much more conscious of the importance of the relationships between institutions and organizations (Boisot and Child, 1996; Peng and Heath, 1996). Treating institutions as independent variables, an institution-based view of strategy, therefore, focuses on the dynamic interaction between institutions and organizations and considers firms’ strategic choices and performances as the outcome of such an interaction (Peng, 2002, 2003). Specifically, firms’ strategic choices and performances are not only driven by industry conditions and firm capabilities but are also a reflection of the formal and informal constraints of a particular institutional framework that managers confront (Khanna and Palepu, 2000; Lee, Peng and Barney 2005).

In other words, institutions are much more than background conditions. Instead, “institutions *directly* determine what arrows a firm has in its quiver as it struggles to formulate and implement strategy and to create competitive advantage” (Ingram and Silverman, 2002:20, added italics). This proposition is certainly valid in developed economies. However, it is research on emerging economies such as China that has pushed the institution-based view to the cutting edge of strategy research, which is becoming the third leg in the strategy “tripod” (see Figure 3). This is because the profound differences in institutional frameworks between emerging economies (notably China) and developed economies force scholars to pay more attention to these differences in addition to considering industry- and resource-based factors (Doh, Teegen and Mudambi, 2004; Makino, Isobe and Chan, 2004).

The rise of the institution-based view as a dominant perspective in strategy research on emerging economies can be seen in the collection of papers in the two influential special issues on such research. In 2000, seven out of 13 papers (54%) in the *Academy of Management Journal* special issue, edited by Hoskisson and colleagues (2000), rely primarily on institutional theory. Consequently, institutional theory is viewed by Hoskisson and colleagues (2000) as one of the top three most insightful theories when probing into emerging economies (the other two are transaction cost economics/agency theory and the resource-based view).

However, Hoskisson and colleagues (2000: 263) predict that the importance of institutional theory may *decline* as emerging economies become more developed, a prediction with which I disagree. Interestingly, Hoskisson and colleagues’ (2000) prediction has indeed been refuted by the increasingly voluminous research drawing on the institution-based

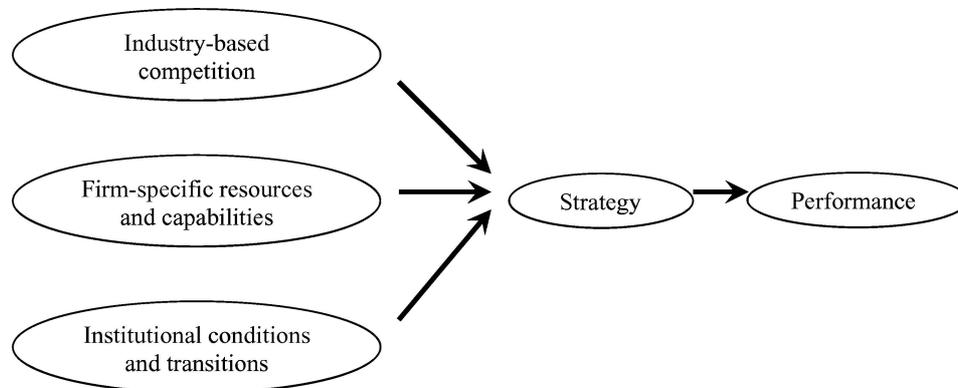


Figure 3. The institution-based view: A third leg of the strategy tripod.  
 [Source] M. W. Peng, *Global Strategy* (p. 115). Cincinnati: Thomson South-Western, 2006.

view to tackle strategy problems in emerging economies. Five years after their prediction, in 2005, seven out of eight papers (88%) in the *Journal of Management Studies* special issue, edited by two of the same editors for the *AMJ* special issue and two new editors (Wright et al., 2005), are institutional papers. It is important to note that the two *AMJ* and *JMS* special issues on emerging economies have no pre-conceived preference for any particular theoretical perspective. Most strategy scholars would not label themselves as hard-core “institutional theorists” (as some institutional sociologists and economists may). Instead, there is a rich and diverse repertoire in the theory tool bag for strategy researchers, who are usually trained to draw on the most relevant and insightful tools to solve theoretical and empirical problems at hand (and not become slaves to any particular school of thought). The fact that institutional theory becomes the most frequently drawn upon theoretical tool speaks volumes about the particular usefulness of this perspective when seeking to better understand the unfolding competition in emerging economies such as China. Such research, in turn, contributes to the larger field beyond the more specialized work on emerging economies by articulating the emergence of a third leg of the strategy tripod (Peng, 2006).

### Conclusion: China is business now

At the dawn of the 21st century, China has become the world’s largest recipient of FDI and the country with which the United States has the largest trade deficit. For multinational enterprises of all stripes, it was hazardous to venture into China 20 years ago, but it is hazardous not to have a strong China presence now. To the extent that our field aspires to be “relevant,” a similar transformation has occurred. At one point, it was possible to exhaustively review all the published China articles in mainstream journals, because there were so few (Peng et al., 2001c). It is now almost impossible to do so, given the rising volume of China research (White, 2002). Not too long ago, China was regarded as an exotic but largely irrelevant place in which to address the fundamental questions of the field.

Today, increasingly, many of the answers to the most fundamental question that our field seeks to address, “What determines the international success and failure of firms?” (Peng, 2004a), will have to be found in domestic and foreign firms competing in and/or out of China (such as outbound FDI from China). If our field truly aspires to become a *global* science of organizations and the 21st century indeed holds some potential to become the “Chinese Century” (Shenkar, 2005), it is imperative that more research be devoted to China in the years to come (March, 2005; Tsui et al., 2005; Tung, 2005).

Although I clearly believe that China strategy research has much to offer to global strategy research, I agree with Lau (2002:174), using his comments on the broader Asian management research, that “the work done so far is inadequate.” Despite the rising number of China studies, a majority of them are empirical in nature with relatively simplistic comparisons, and there has been a lack of theory development and contribution to the conceptual literature beyond an audience specifically interested in China (White, 2002). While the field’s fundamental transformation from being relatively China-hostile to more China-friendly is a cause for celebration, three challenging questions we need to ask ourselves are:

1. Are research and findings from China research cited by anyone besides those doing work in or on China (and Asia)?
2. Is China research being cited in mainstreams textbooks on strategy, organization, and IB?
3. Have China scholars set for themselves the goal of context-free theory-building rather than China-specific theory-building or simple theory-testing using theories developed elsewhere?<sup>15</sup>

While I believe that the institution-based view of strategy is likely to become a more established part of the global strategy literature beyond China (Peng, 2006), I am confident that many other areas of China research may hold the same potential.

Returning to the opening question for this article, “How to conduct and publish China research in the global community of scientific research?,” while I do not have any definitive answers, I do believe that a research strategy which taps into Chinese realities while endeavoring to remain globally relevant may pay off. As an intermediate step, making China research more relevant within the broader Asian context would be helpful (Ahlstrom and Bruton, 2004; Delios and Singh, 2005; Lau, 2002; White, 2002). This is similar to the international expansion strategy of many Chinese (and Asian) firms, which usually venture to neighboring countries first. Overall, if this article can stimulate more discussion and debate on how to take advantage of the golden era of China research and integrate China work with global strategy research, then my purposes will have been well served. In conclusion, if this article can only contain one message based on my own experience and my understanding of other scholars’ work, I would recommend that researchers “act *local* (focusing on China), but think *global*.”

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## Notes

1. For example, the rejection rate of the *Asia Pacific Journal of Management* during 2002–04 was greater than 70%.
2. This assertion is true if we assume that pool of articles published remains constant. However, this assumption is subject to debate. For example, the Academy of Management is talking about alternative outlets for publishing. I thank Rosalie Tung for sharing this thought with me (personal communication, February 28, 2005).
3. In my view, this depressingly low number of citations does not indicate that Tung (1981a) is a poorly crafted piece of work. Since the early 1980s, the author, Rosalie Tung, embarked on a very successful career culminating in the presidency of the Academy of Management. One interesting comparison is with Tung (1981b), which appeared *at the same time* in a less highly ranked journal, the *Columbia Journal of World Business*. However, it dealt with a mainstream (and non-China) topic, namely, expatriate management. As of this writing (February 2005), Tung (1981b) has generated 107 citations (based on the Social Sciences Citation Index). This comparison leads me to believe that Tung (1981a) was ahead of its time. An exhaustive survey of the literature finds that Tung (1981a) was not only the first China-based article in *AMR*, but also the very first such article in nine mainstream North American and European journals (Peng et al., 2001c: 97). The next time *AMR* published a China-related piece would be 15 years later—Peng and Heath (1996). In other words, in the 1980s, there was not a critical mass of scholars in our field who would be able to benefit from and build on Tung (1981a)—hence, the relatively low number of citations. Although China research has taken off since the 1990s, most of the new China scholars do not bother to trace and consult this “old piece.”
4. In addition to the publication as a book (Peng, 1998), my dissertation generated three journal articles (two in the *Journal of International Business Studies* and one in the *Journal of Management Studies*). However, as of this writing (February 2005), the three journal articles have only generated a *combined* total of 22 citations. In comparison, Peng and Heath (1996) has 82 citations.
5. For example, by the time I wrote a comprehensive textbook *Global Strategy* (Peng, 2006) during 2003–04, I had conducted and published research broadly related to 10 of the 12 chapters in the book.
6. Thomas, Shenkar, and Clarke (1994) report that the volume of research on any country published in the *Journal of International Business Studies* can be best predicted by the volume of trade and investment that country has with the United States. Given *JIBS*’ standing as the flagship journal in IB and *JIBS*’ reputation as being more open minded than other mainstream journals, this interesting finding indeed speaks volumes about the U.S. dominance of our field (see also Boyacigiller and Adler, 1991; March, 2005). This finding can be used to explain the explosion of China research now, not only in *JIBS* but also other journals—China’s trade and investment volume with the United States has grown exponentially in recent years (Shenkar, 2005).
7. In my first quarter of doctoral studies, autumn 1991, I was pleasantly surprised to discover the *Asia Pacific Journal of Management* in the library. However, during the 1980s, *APJM* did not publish a single piece of research focusing on mainland China (Peng et al., 2001c:98).
8. The six journals are the *Academy of Management Journal*, *Academy of Management Review*, *Administrative Science Quarterly*, *Journal of Management Studies*, *Journal of International Business Studies*, and *Organizational Studies*. During the 1980s, *Management Science* and *Strategic Management Journal* did not publish a single China article (Peng et al., 2001c).
9. Charles Hill at that time worked on a paper on Japan (Hill, 1995), that drew heavily from North (1990).
10. My conclusion on the lack of applicability of population ecology theory in China was later confirmed by Shenkar and von Glinow (1994).

11. Despite my best efforts, I was not able to complete my dissertation by the time I left the University of Washington and started at the University of Hawaii as an assistant professor in autumn 1995. I finished my Ph.D. in 1996.
12. This is consistent with one of Rosalie Tung's criteria for research topic selection: "Am I truly passionate and excited about the topic?" (Tung, 2005). A friend who is familiar with both my dissertation and non-dissertation work commented that he could see more passion in my *non*-dissertation research.
13. I am pleasantly surprised that as of December 31, 2004, Peng and Heath (1996) was the number one most cited paper in our field on CEE (Meyer and Peng, 2005). In retrospect, it is not surprising because the strategy, organization, and IB literature on CEE, just like that on China, has a similar paucity for solid theoretical work and Peng and Heath (1996) enjoys some first mover advantage in this literature.
14. Tarun Khanna of Harvard, who authored three of the 100 cited references, was the second most cited author in that bibliography (Hoskisson et al., 2000).
15. These questions are inspired by White (2002:302).

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