*Management and Organization Review* 3:2 171–181
doi: 10.1111/j.1740-8784.2007.00073.x

理組
研織
究管

# Editorial
## What to Study in China? Choosing and Crafting Important Research Questions

The first step in any research is to decide what to study. Choosing the right question is essential for producing relevant knowledge. This is not to say that relevance should be subordinate to rigour. They are both important. A relevant question which is analysed rigorously produces valid knowledge that both advances theory and informs practice. A rigorously performed study on an irrelevant question may advance theory but does not inform practice. The purpose of this essay is to provide a discussion of how to choose and formulate important research questions and to share the results of a survey on important management challenges in China as possible topics to guide future research.

The concern over choosing the right question is not unique to Chinese management. More than 25 years ago, Campbell et al. (1982) enlightened the fields of organizational behaviour, human resources management and industrial/organizational psychology with a monograph titled '*What to study*' in which the authors identified different ways by which scholars identify important questions to study along with a list of interesting topics to guide aspiring scholars. One of the fears of any scholar is to have spent a substantial amount of time on a research project over several years, only to find out at the time of journal submission that reviewers and editors do not care much about the scholar's question. The other extreme is to chase fads by choosing 'hot topics'. The danger in this approach is that fads come and go. They aren't necessarily the most important problems that confront managers. Choosing the right question to study in a context like China is particularly important for scholars both inside and outside of China to ensure that the knowledge produced will not only meet rigorous standards but will also address the urgent needs of managers in search of guidance on how to manage in this dynamic context.

## CHOOSING IMPORTANT RESEARCH QUESTIONS

In the editorial essay in MOR2.1, Tsui (2006) encouraged choosing research questions that are meaningful and important to the local (Chinese) context. She observed two approaches of topic choice in the extant research on China. One is to apply topics that are familiar and interesting to Western scholars whose preferences originate from their familiarity of the extant literature and what they perceive to be important in Chinese firms or multinational corporations in China. Chinese scholars also may do the same by reading popular topics in the Western literature and guessing what topics might be palatable to editors and reviewers of the leading international journals. Tsui referred to this as the 'outside-in' approach which may miss the truly important management or organization issues of the Chinese context. Leung (2007) observed similar behaviour among Asian social psychologists. He concluded from his analysis that frequentlyly cited Asian researchers tend to publish on research topics that are well known in the West. Noting the lack of innovativeness of Asian management research, Meyer (2006) encouraged Asian management researchers to delve into indigenous issues for the greater potential of developing innovative theories.

To develop useful and valid knowledge, Tsui (2006) advocated the 'inside-out' approach in identifying and studying the unique issues within the Chinese context. She refers to this as 'contextualization of the phenomenon' (2006, p. 2) and urged researchers to use this approach as a replacement of the current literature-based or journal-focused approach in identifying research questions for Chinese management studies. This phenomenon motivated research is a key characteristic of the most influential theories in management (Smith and Hitt, 2005). Hambrick summarizes 'those who have a knack for developing theories are astute observers of phenomena; they detect puzzles in those phenomena; and they then start thinking about ways to solve the puzzles . . . puzzles trigger theory development' (Smith and Hitt, 2005, p. 574). The results are influential theories, not only in the academic world but also in practice. In other words, influential scholars do not study what others study or study what they think will be receptive to editors and reviewers. They study interesting phenomena that are important issues in their contemporary contexts. These scholars are keen observers of the world, especially of the management world around them. They notice issues that are puzzling or unusual practices that are yearning for explanation or understanding.

Beyond motivation by interesting and important phenomena, influential scholars are also clear about the audience for their research. They do not define editors and reviewers as the only audience. They themselves constitute the first audience for their work. The question they study not only interests them, but it fascinates them. Indeed, conceiving, researching, writing and publishing a scholarly article can often take up to two years or more, and a dissertation significantly longer. Intrinsic interest in an important phenomenon is critical to sustain the intensive effort over a long time period. If the study did not fascinate the scholars, the research would turn out to be self-inflicted torture – the cause of many unfinished or unsuccessful papers and dissertations.

The scholarly community forms the second audience that should find the question interesting. Huff (1999) uses the metaphor of a 'scholarly conversation'. Which scholarly community or conversation would the new study join? Which scholars will express interest in the questions posed and potential answers provided? Which part of the scholarly community will cite the paper? Answering these questions, however, is often not a simple task. Borrowing another metaphor from the philosopher Karl Popper (1966) – scholarly communities resemble clouds more than they resemble clocks. It is hard to ascertain where one billow, or scholarly area ends, and where the other begins. However, each study needs to have a boundary. The scholar needs to define what is within and what is outside the boundary of the phenomenon on which the spotlight shines. This should determine the academic audience for the question under study and clarify the potential contribution to this scholarly conversation and community.

The third audience that a research question might interest is, of course, the audience of non-scholars or, in our world, the practicing managers and employees of organizations, educators, consultants or government officials. Results of research are the basis of textbooks and the students are often the first recipients of the new findings by the faculty members who teach them. Consultants use the results of our research to help organizations manage better. Our research findings may potentially be of interest to policymakers in government or business. In the Fifth Plenary Session of the Sixteenth Central Committee of the Communist Party of China (CPC) on October 8–11, 2005, the Chinese government identified innovation, entrepreneurship and harmony as the three primary agendas for national development. Research studies that contribute to each of these areas could garner national attention (http://news.sina.com.cn/z/sljwzqh. The complete report of the Fifth Plenary Session of the Sixteenth Central Committee of the Communist Party of China (CPC) on October 8–11, 2005. Accessed April 16, 2006). The Chinese National Natural Science Foundation has awarded grants to several research projects on innovation and entrepreneurship in the past few years.

## CRAFTING INTERESTING RESEARCH QUESTIONS

Having identified an important phenomenon, the next step is to identify how the study will contribute to this body of knowledge. Thomas Kuhn (1962) likens an area of research in normal science to a puzzle. The areas of interest in the puzzle are those that have not yet been filled. To define an interesting research question begins by understanding *all* the existing parts of the puzzle. That is, the scholar must do a very thorough review of what has already been written in a research area. Then find so called 'holes' in the literature, and pose questions whose answers will fill these holes. For this reason, many articles start with a paragraph stating what scholars have studied on the topic or research area. The second paragraph then points out what scholars have not yet studied in that area, thereby providing the justification for the research question.

Filling holes and identifying how the new study would fill the holes is certainly a good first step in convincing the scholarly community of the study's potential contribution. However, more is necessary to determine if others will find the contribution to be important or interesting. The more is whether the research question and the potential answer refute current knowledge or undermine commonly held assumptions. Davis (1971), in an article that is still widely read by all doctoral students, provides us with many ideas on how to increase the 'interestingness' of a research idea or theory. For instance, audiences will find interest in a research question whose answer shows that what they thought was a bad practice (e.g., decrease profit in the short run) is in fact good (increase quality and customer loyalty in the long run). The study may show that, whereas the audience thought (based on past studies that) A caused B (smiling employees increase sales performance), B in fact causes A (sales performance increases the incidence of employees smiling). The book '*Great Minds in Management*' by Smith and Hitt (2005) is rich with examples of how scholars developed influential theories that defy assumptions of existing theories or explanations. Locke and Latham (1990), for example, believed that current theories of human behaviour, which assumed limited human volition, were inaccurate. They proposed the goal setting theory that assumed human agency in influencing outcomes. Bandura (2001) developed the social cognition theory for a similar reason. Pfeffer and Salancik (1978) believed that organizational leaders are much more constrained than current theories seem to assume. They developed the resource dependence theory. These scholars proposed new theories because they observed that some phenomena are not consistent with commonly held knowledge or beliefs. They formulated research questions that defy, if not deny, current or common wisdom.

The international domain offers the greatest potential for discovering interesting research questions that defy common beliefs or challenge the validity of existing knowledge about management. The nation and, in particular, national culture can be an important boundary condition for the generalizability of existing theories of management (Hofstede, 1993; Meyer, 2006; Whetten, 2002). Tsui (2004) discussed three types of global management knowledge: context-free, context-bounded, and context-specific. The first type is also known as 'universal knowledge' (Cheng, 1994) which is intended to apply to all contexts. The same predictors would relate to the same outcomes in the same way across all contexts. Context-bounded knowledge is true in some contexts but not in others. Context-specific knowledge is true in one context but its applicability in another context is either untrue or unknown. The focus of international management knowledge has been primarily in testing the national or cultural boundaries of existing theories which were generated in the Western contexts (primarily the USA and Europe secondarily). This 'outside-in' approach (Tsui, 2006) may be limited when deriving a true understanding of the phenomena in the new context. Tsui (2004, 2006) strongly advocated context-specific or indigenous research in novel contexts. This may require grounded theory building since induction holds more promise for yielding contextually valid theories (Eisenhardt, 1989) than applying, extending, or modifying existing theories (Tseng and Kwan, 1999). Context-specific research in novel locations would contribute to both local and global management knowledge. Meyer encouraged Asian scholars to be more self-confident in 'exploring locally relevant research issues, and in developing theories that explain Asian phenomena' (2006, p. 119). We encourage the same of Chinese management scholars and students.

To facilitate the choice of relevant management issues in China, we conducted a survey of 500 business and academic leaders about important management challenges that they believe should deserve research attention. We offer the results to guide future management research by local and international scholars as well as by thousands of Chinese graduate students who are dedicated to studying and understanding the management and organization of all kinds of firms, for profit or nonprofit, domestic or foreign, state-owned or private, in the Chinese context.

## IMPORTANT RESEARCH ISSUES ACCORDING TO CHINESE BUSINESS AND ACADEMIC LEADERS

Business leaders are the major consumers of research findings and management education programs. Academic leaders can influence the research agenda of their faculty members by how they allocate research dollars to projects. Many of the business school deans in China also serve on the National Natural Science Foundation, the major funding source of academic research in China. The deans teach executive courses and have frequent contact with a variety of business leaders. Given the strategic role of these business and academic leaders, they are reasonable informants of the major management challenges in China.

We first identify the most important issues by asking 30 business school deans and 30 CEOs to respond to an open-ended questionnaire. It contains two questions.

1. In your view, what are the biggest managerial challenges that firms in China face today?
2. What are the biggest managerial challenges that firms in China will face in the next 20 years?

The respondent could list up to five topics for each question. Our purpose at this stage is not to compare the views of the business leaders from that of the deans, nor to compare current to future topics. Rather, the purpose was to generate a wide range of possible topics. Therefore, we consolidated the responses from the two questions and the two groups of respondents. After eliminating repetitions, we obtained a total of 39 topics. These 39 topics became the content of a structured survey. We grouped the topics into seven major areas. The respondents assigned a rank of one to the topic seen to be most important within each area. Lastly, they ranked the seven major areas in terms of relative importance. We used ranking instead of rating to prevent the respondents from rating all items to be equally important. Our network of scholars in China assisted in collecting data from executive education participants. We have a total of 500 respondents, including 57 business school deans, 143 CEOs, 185 senior managers and 93 middle managers in five major cities located throughout China. Their firms vary in size and operate in many different industries. The average firm age is 17 years. Lastly, we held a focus group discussion with 15 CEOs to help us with interpreting the survey findings.

Table 1 summarizes the mean rankings of the seven major areas and the topics within each area. The areas and the topics within each area are presented in the order from the most important to the least important rank. We need to keep in mind that these issues identified in the open-ended survey are all important and that we forced the respondents to rank them. Therefore, the lowest ranked area does not mean that it is not important. The second column lists the ranks for the total sample. The third column is the results of the statistical test (ANOVA) that compares the mean ranking scores of the respondents in firms with different ownership structures: state-owned, private and foreign. The last column is the ANOVA results comparing the mean ranking scores of the respondents in the four positions: CEOs, senior managers, middle managers and deans.

For the major areas, firm strategy received the ranking of most importance, followed by firm management and leadership. The topic of ethics was seen as the least important challenge. The top three rankings and the bottom ranking are identical across the three types of firms. Respondents from the foreign firms ranked the macroenvironment as significantly (statistically speaking) less important but organizational development as significantly more important than respondents from the state-owned firms. Respondents from the private firms ranked human resource management as significantly more important than respondents from the state-owned firms. The rankings on the seven major areas are identical across the four groups of respondents by position, except the area of human resource management. CEOs considered this area as more important than did the middle managers or the business school deans. CEOs ranked this area fourth in importance, below leadership but above macroenvironment, organizational development and ethics.

Overall, the views of the 500 respondents on the relative importance of the seven major areas of management challenges are highly similar. This convergence of opinions suggests reliability in the results. It also reflects the strategic roles of these respondents. They occupy positions that require them to focus on the competitive positions of the firm. Therefore, firm strategy, firm management and leadership are the more important challenges, relative to the other areas.

Table 1. Relative importance of major management challenges in China

| | *Total sample mean score (rank)* | *Difference by firm ownership* | *Differences by position* |
|---|---|---|---|
| **Firm strategy** | **2.28 (1)** | **NS** | **NS** |
| Core competence development | 2.73 (1) | NS | 5.80*** |
| Administrative innovation | 3.63 (2) | NS | NS |
| Technological innovation | 3.83 (3) | NS | NS |
| Competitive strategies | 4.64 (4) | NS | NS |
| Global brand management | 5.71 (5) | NS | NS |
| Globalization | 5.80 (6) | NS | NS |
| International management | 6.01 (7) | NS | 4.14** |
| Political strategies | 6.12 (8) | NS | NS |
| Entrepreneurship process | 6.24 (9) | NS | 3.05* |
| **Firm management** | **3.26 (2)** | **NS** | **NS** |
| Corporate governance | 2.30 (1) | NS | NS |
| Application of modern management in China | 3.08 (2) | NS | NS |
| Integrate Chinese culture with modern management | 3.17 (3) | NS | NS |
| Quality management | 3.77 (4) | 4.06* | 4.14** |
| Knowledge management | 3.93 (5) | NS | 2.69* |
| Corporate privatization | 4.64 (6) | 4.58** | 2.68* |
| **Leadership** | **3.72 (3)** | **NS** | **NS** |
| Leadership style | 2.01 (1) | NS | NS |
| Executive compensation | 2.77 (2) | NS | NS |
| Delegation | 2.84 (3) | NS | NS |
| Global leadership | 3.61 (4) | NS | 2.80* |
| Executive succession and selection | 3.73 (5) | NS | 2.61* |
| **Macroenvironment** | **3.89 (4)** | **3.98*** | **NS** |
| Influence of legal system and regulation on enterprise management | 2.25 (1) | NS | NS |
| Industrial policies | 2.52 (2) | NS | 2.68** |
| Global competition | 3.12 (3) | NS | 3.53** |
| Role of government in business | 3.29 (4) | NS | NS |
| Information technology and security | 3.79 (5) | NS | NS |
| **Organization development** | **4.26 (5)** | **3.77*** | **NS** |
| Organizational design and structure | 2.12 (1) | NS | 5.27*** |
| Corporate culture building | 2.26 (2) | NS | 5.27*** |
| Managing change and development | 2.32 (3) | 3.23* | 6.95*** |
| Development of private domestic firms | 3.28 (4) | 8.04** | 6.38*** |

Table 1. (cont.)

| | *Total sample mean score (rank)* | *Difference by firm ownership* | *Differences by position* |
|---|---|---|---|
| **Human resource development** | **4.36 (6)** | **3.82*** | **3.25*** |
| Reward and incentive systems | 3.27 (1) | NS | NS |
| Employee performance management | 3.30 (2) | NS | NS |
| Teamwork | 3.63 (3) | NS | 3.33* |
| Management talent attraction and retention | 3.70 (4) | NS | NS |
| Trust building | 3.87 (5) | NS | NS |
| Professional management development | 4.90 (6) | NS | 2.69* |
| Management of employee diversity (e.g., values) | 5.14 (7) | NS | NS |
| **Ethics** | **5.92 (7)** | **NS** | **NS** |
| Corporate social responsibility | 1.78 (1) | NS | NS |
| Professional ethics | 1.99 (2) | 3.20* | NS |
| Business ethics | 2.22 (3) | NS | NS |

*Notes:* * $p < 0.05$; ** $p < 0.01$; *** $p < 0.001$, based on ANOVA F values.

Within firm strategy, core competence development and innovation are ranked higher than the other topics. Political strategies and entrepreneurship process are ranked the lowest. Innovation, globalization and entrepreneurship are parts of the firm strategy. Overall, these are the most important management challenges, a theme consistent with China's national agenda for economic development. Within the firm management area, corporate governance is ranked most important and corporate privatization is ranked least important. Leadership style is ranked the most and executive succession and selection the least important. Comparing respondents by their firm's ownership, there were differences on only five topics. Further analysis shows that respondents from the foreign invested firms ranked quality management more important and corporate privatization less important than the respondents from the other types of firms, especially the state-owned firms.

Differences in rankings by respondent position are found on 16 of the 39 topics. In other words, the four groups agree on the relative ranking on 23 of the 39 topics. Further analysis shows that the rankings by the deans are most different from the other respondents. They ranked these topics as more important than at least one other group: managing change and development, development of domestic private firms, international management, global competition and corporate privatization. They ranked the following topics as less important than at least one other group of respondents: core competence development, corporate culture building, quality management and teamwork. It appears that the deans are more attentive to strategic issues of firms operating in a global competitive terrain. Business leaders, on the other hand, are attentive to the operational challenges of managing the firm. These differences notwithstanding, we should keep in mind that all groups ranked firm strategy to be more important than other areas.

Lastly, we asked 15 CEOs in a focus group discussion to help us understand and interpret why these three areas are most the important management challenges from the perspectives of all the respondents, including senior and middle managers. They pointed us to the influence of Chinese culture and thought. Chinese business leaders are influenced by ideas contained in classic books such as the *Book of Changes* (Zhou et al., 1967), or *The Arts of War* (Sun and Samuel, 1963). At present, the Chinese market is not yet mature and there are many opportunities. Strategy helps the firm to identify and exploit these opportunities, both in the local and global markets. This requires finding a unique position or niche to compete. When firms grow bigger, they need a strategy to grow healthily. Strategies help the organization to stay 'young' on a long term basis. Management is important for a firm's growth and development. It is particularly necessary for state-owned enterprises as they transit from state control to self-control and for all firms as the economy transits from government control to market control. Leadership is important for three reasons. In general, Chinese people do not like to be controlled, but they would like to manage others (a possible desire fueled by the perception that leaders have more power in a large power distance culture such as China). Chinese firms do not yet have well-developed systems, so strong leadership is necessary to manage and guide employees. As the market economy strengthens, firms need to develop professional management and have professional managers lead and guide them – all important topics within the leadership area.

## SUMMARY

How to choose important, and craft interesting, research questions is more an art than a science. There are different opinions and experiences. A seasoned scholar offered what he called the 'airport metaphor' of what to study. First, find the airport and the runway where most aircrafts take off from. In other words, start studying phenomena in a field many others scholars study in. Only when one has gained speed, altitude and experience, fly to where others have not gone. That is, after having proved one can study questions in areas where others have studied, should one begin to study questions that do not fall neatly in a well-populated area of research. This appears to be the path taken by most scholars in international management studies. They chose the most popular questions and apply the well-accepted theories to novel contexts. For example, they may seek to find out whether leadership in the novel context is or is not similar to leadership in the West using a Western leadership theory. Using this approach, they would not know in this novel context if leadership has a different meaning or leaders engage in different behaviours from that in the West, simply because the scholars' vision is restricted to the theoretical lens they use in examining the leadership phenomenon.

Studying in crowded areas is like trying to take off on a very crowded runway – a long wait, followed by a race of the swiftest and the occasional collision. It is possible to engage in such a race if one has a favourable starting position – his or her mentor dominates the area of study, for instance – or if the young scholar has some competence that allows him or her to fly faster than the pack. Even with these advantages, however, scholars who fly straight down the dominant academic runway for the former part of their career often lose the capacity to find their own direction in the later part.

An alternative strategy is to find a little airport. Take off on a runway with only a few competing planes. Keep an eye on the path along which most planes fly, but also keep a distance. When one has reached a certain altitude, remain at the distance of the majority of aircraft, and they will take note of the new plane with certainty. In other words, explore questions in areas that others have largely ignored (not because of insignificance) and, over time, the majority of scholars will recognize what this new scholar has studied is unique, original and interesting. International management is like a small airport that provides many opportunities for new questions and contributions. Rather than choosing the well-flown paths, it is wiser to choose the uncharted territories where there is less competition. Charting new territories is clearly more difficult than following the pack. Thus, it is particularly important to choose topics that are meaningful and important to the researcher and to have identified a research community that would appreciate the new study and its contribution. Many scholars (e.g., Meyer, 2006; Tsui, 2004, 2006; Whetten, 2002; White, 2002) offer suggestions on how to conduct research in novel contexts like China that have great potential for adding the much needed novel knowledge to the management literature. True, the work is intense and demanding but the reward is immense. We hope this essay has offered some guidance or shed a light along this exciting new path.

## NOTES

We would like to express our appreciation to all the professors whose assistance is invaluable in collecting the survey data. They are Professors Weizheng Chen at Sichuang University, Wanwen Dai at Nanjing University, Qi Li at Peking University, Yuan Li at Xian Jiaotong University, Yanping Li at Wuhan University, Ker-Wei (Buck) Pei at Arizona State University, Ningyu Tang at Shanghai Jiaotong University, Zhixing Xiao at China Europe International Business School and Renhong Zhu at Sun Yat-Sen University. We also wish to thank Jianwu Jiang for his help with data entry and analyses.

## REFERENCES

Bandura, A. 2001. Social cognitive theory: An agentic perspective. ***Annual Review of Psychology,*** 52: 1–26.

Campbell, J., Daft, R., & Hulin, C. 1982. ***What to Study? Generating and developing research questions.*** Beverly Hills, Calif.: Sage.

Cheng, J. 1994. On the concept of universal knowledge in organization science: Implications for cross-national research. ***Management Science,*** 40: 162–168.

Davis, M. S. 1971. That's interesting! Towards a phenomenology of sociology and a sociology of phenomenology. ***Philosophy of Social Science,*** 1: 309–344.

Eisenhardt, K. M. 1989. Building theories from case study research. ***Academy of Management Review,*** 14: 532–550.

Hofstede, G. 1993. Cultural constraints in management theories. ***Academy of Management Executive,*** 7: 81–94.

Huff, A. S. 1999. ***Writing for scholarly publication.*** Thousand Oaks, Calif.: Sage Publishing, Inc.

Kuhn, T. S. 1962. ***The structure of scientific revolutions.*** Chicago, Ill.: University of Chicago Press.

Leung, K. 2007. Glory and tyranny of citation impact: An East Asian perspective. ***Academy of Management Journal,*** 49(2): in press.

Locke, E. A., & Latham, G. 1990. ***A theory of goal setting and task performance.*** Englewood Cliffs, N.J.: Prentice Hall.

Meyer, K. E. 2006. Asian management research needs more self-confidence. ***Asia Pacific Journal of Management,*** 23: 119–137.

Pfeffer, J., & Salancik, G. R. 1978. ***The external control of organizations: A resource dependence perspective.*** New York, N.Y.: Harper and Row.

Popper, K. R. 1966. ***Of clouds and clocks: An approach to the problem of rationality and the freedom of man.*** St Louis, Mo.: Washington University Press.

Smith, K. G., & Hitt, M. A. (Eds.). 2005. ***Great minds in management: The process of theory development.*** Oxford, UK: Oxford University Press.

Sun, T., & Samuel, B. G. 1963. ***Sun Tzu: the art of war.*** New York, N.Y.: Oxford University Press.

Tseng, E. W. K., & Kwan, K. M. 1999. Replication and theory development in organization science: A critical realist perspective. ***Academy of Management Review,*** 24: 579–780.

Tsui, A. S. 2004. Contributing to global management knowledge: A case for high quality indigenous research. ***Asia Pacific Journal of Management,*** 21: 491–513.

Tsui, A. S. 2006. Contextualization in Chinese management research. ***Management and Organization Review,*** 2(1): 1–13.

Whetten, D. A. 2002. Constructing cross-context scholarly conversation. In A. S. Tsui & C. M. Lau (Eds.), ***The management of enterprises in the People's Republic of China:*** 29–47. Boston, Mass.: Kluwer Academic.

White, S. 2002. Rigor and relevance in Asian management research: Where are we and where can we go? ***Asian Pacific Journal of Management,*** 19: 287–352.

Zhou, G., Baynes, C. F., & Wilhelm, R. 1967. ***The I Ching, or book of changes.*** Princeton, N.J.: Princeton University Press.

*Anne S. Tsui*
*Shuming Zhao*
*Eric Abrahamson*