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Chinese Management Research at a Crossroads: Some Philosophical Considerations

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ABSTRACT In this commentary, I discuss some philosophical issues related to contextualizing Chinese management research. First, it should be noted that contextualization can be interpreted in different ways by different philosophical perspectives. Second, the Duhem-Quine thesis implies that replications, in the form of empirical generalization, are an appropriate means of testing Western theories in a Chinese context. Third, owing to the difficulty of conclusively falsifying management theories, attempts to create theories that explain unique Chinese management phenomena may lead to theory proliferation and the perpetuation of weak, or even false, theories. Finally, I propose the study of empirical regularities as an alternative to the theory application and theory creation approaches.

KEYWORDS contextualization, Duhem-Quine thesis, empirical regularity, replication, theory proliferation

INTRODUCTION

A critical issue facing researchers concerns contextualizing Chinese management research – whether or not and how far we should apply existing management theories, virtually all of which were developed in a Western context, to the Chinese context. Tsui (2006) distinguishes between two approaches to dealing with this issue, namely theory application and theory creation. The former approach advocates the direct application of existing theories to the Chinese context, with the objective of testing and refining the theories. The latter is a grounded theory building approach and attempts to create theories that explain unique Chinese management phenomena.

The articles by Barney and Zhang (2009) and Whetten (2009) offer some unique contributions. Their core arguments focus on the abovementioned distinction between theory application, which corresponds to Barney and Zhang's theory of Chinese management and Whetten's context embedded theory, and theory creation, which corresponds to Barney and Zhang's Chinese theory of management and Whetten's context effects theory. As described by Barney and Zhang, Chinese management research is now at a crossroads of choosing between these two approaches. The two articles together have provided a number of insightful ideas and practical suggestions. Taking a slightly different perspective, I will elaborate below some philosophical issues related to their discussions and hope to throw light on the topic from a different angle.

AN ALTERNATIVE INTERPRETATION OF CONTEXTUALIZATION

Is Contextualizing Chinese Management Research Really an Issue?

Tsui (2006: 2) defines contextualization as 'incorporating the context in describing, understanding, and theorizing about phenomena within it' and states that 'the need for contextualization is well accepted'. While this statement is true, different philosophical perspectives may perceive contextualization differently. All of the discussions I have read about contextualizing management research in general, such as Barney and Zhang (2009), Meyer (2006), Rousseau and Fried (2001), Tsui (2006, 2007), and Whetten (2009), seem to adopt an implicit ontological assumption that there is an objective reality existing independently of researchers.[1] A major concern of contextualizing Chinese management research in particular is whether or not Western management theories can adequately explain Chinese management phenomena. In other words, the question is whether these theories can accurately reflect the reality of such phenomena. This view is in line with the correspondence theory of truth, the central thesis of which is that 'true propositions "correspond" with reality' (Audi, 1998: 239). Or, to express this thesis in everyday language, a statement is true only if it describes the world as the world really is. The aim of scientific investigation is to produce true or approximately true knowledge, in the form of generalizations, laws or theories, about the world (Faye, 2006).

Though it may seem natural to hold this rough, realist ontological assumption, it is just one of the several that underlie the various philosophical perspectives adopted by management researchers (see Burrell & Morgan, 1979). A moderately strong version of the social constructivist view, for example, would maintain that 'the idea of an independent reality is at best an irrelevant abstraction and at worst incoherent' (Audi, 1999: 855). When this ontological assumption is carried to the domain of empirical research, it gives rise to the epistemological view that the 'empirical data reported in our academic journals are not an objective representation of external reality; they are, instead, a subjective reflection of researchers' theoretical perspectives, values, and intellectual commitments' (Astley & Zammuto, 1992: 448). Without an independent, objective reality, the question of whether Western theories can accurately reflect the reality of Chinese management phenomena makes little sense.

The social constructivist view claims further that empirical research is 'essentially a form of "storytelling" ' (Astley & Zammuto, 1992: 449). Empirical

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observations serve the role of stimulus materials for constructing stories, and the theory so produced is essentially fiction. First, when empirical data, which reflect the context in which the data are collected, are treated as stimulus materials for storytelling, data collection and contextualization are an option, not a necessity, because good storytellers may not need any external stimulus materials. Second, when theory is equated with fiction, an implication is that researchers should adopt the theory creation approach. Consider *Romeo and Juliet* and *A Dream of the Red Mansion*, the apotheoses of Western and Chinese love stories, respectively. Both are highly embedded in their respective cultures. According to this view, a piece of simple advice thus is: write a Chinese novel in a Chinese way.

Moreover, Astley (1985) argues that the body of management knowledge, which includes theories, is a socially constructed product. According to this argument, as long as Chinese communities are satisfied with the theories they have created, they do not have to bother about whether these theories will be accepted or used by other communities, who have their own socially constructed theories. As long as each community is happy with its own theories, to contextualize or not to contextualize is not an issue.

As a critical realist myself, my intention here is not to promote this, in my opinion, nonsensical view of empirical research and theory development.[2] Rather, I want to show that contextualization can be interpreted in a different way and can even be considered a non-issue from certain philosophical perspectives, some of which, in fact, have become more popular during the last two decades because of the so-called postmodern turn for management research (see Kilduff & Mehra, 1997). My discussions below will be based on a critical realist perspective (see Bhaskar, 1978).

Testing Theories Across Contexts

When discussing the theory application approach, Barney and Zhang raise some interesting issues about testing a received theory in a Chinese context, such as the difficulty of generating sufficiently precise predictions that can be identified and tested. The problems associated with testing theories across contexts are related to the Duhem-Quine thesis, which maintains that theories are never tested alone. Rather, they are tested in conjunction with a set of auxiliary hypotheses, including background assumptions, research methods and rules of inference (Sawyer, Beed, & Sankey, 1997). Suppose a researcher tests certain hypotheses derived from transaction cost economics, such as the standard make or buy decision, in a Chinese context. The test involves not only the theoretical relationships depicted in the hypotheses, but also the measurement of constructs, data collection and statistical analysis. The theory and its associated auxiliary hypotheses constitute a test system.

Stated formally, let H be the principal hypothesis derived from a substantive theory of interest, A the set of auxiliary hypotheses and O the observational

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consequence entailed by the conjunction of H and A so that $H \wedge A \Rightarrow O$. A negative observation 'not O' implies 'not $(H \wedge A)$' instead of 'not H' alone. That is, the whole test system, not just the principal hypothesis, is falsified (Grünbaum, 1960).

The presence of auxiliary hypotheses confounds the test of the principal hypothesis. In the case where a negative observation occurs, it would be hard to isolate which elements of a test system are responsible for the result. This problem of underdetermination is especially serious in the social sciences in general and management in particular where researchers are seldom in agreement about how certain auxiliary hypotheses should be independently tested (Meehl, 1978). Consider the abovementioned transaction cost economics study. Researchers may not agree on, for example, how opportunism should be measured. The situation of testing a theory in a national context that is different from the national context where the theory was developed is even more complicated because additional noises, which are caused by cross-contextual differences, are introduced into the test system. For instance, a failure of transaction cost economics to generate an accurate prediction in a Chinese context may be due to respondents' misinterpretation of the relatively unfamiliar concept of opportunism.

Under the theory application approach, Whetten argues that a contribution to theory is predicated on the result that one or more contextual effects alter the theory's predictions. However, the Duhem-Quine thesis indicates that, when a prediction is altered, it would be very difficult to clearly demonstrate that the result is caused by a certain contextual effect. Whetten mentions that comparing research results across contexts draws heavily on an empirical generalization type of replication (Tsang & Kwan, 1999). I elaborate here how empirical generalizations can facilitate cross-context comparisons. For the purpose of our discussion, empirical generalization refers to replicating in a Chinese context a theory-based study that was previously conducted in a Western context, with the procedure, measurement and analysis of the original study being followed as closely as possible. A key merit of empirical generalization is that, by following the original study closely, we minimize noises that are due to differences in research methods and analyses. When a finding from the original study is not supported, it would be easier to identify whether the difference in findings is caused by a certain contextual effect.

For instance, several years ago, I replicated Bettman and Weitz's (1983) study of self-serving attributions (see Tsang, 2002). By conducting a content analysis of US corporate annual reports, their study tested the theory of self-serving bias in the attribution of causality. I applied their coding method to corporate annual reports in Singapore, a predominantly Chinese society, and used the same statistical analysis. The general self-serving pattern of attributions found in the original study was also identified in my study. Nevertheless, the data of the original study did not unequivocally support either the motivational or informational explanation for the existence of self-serving attributions, whereas my data strongly supported the latter explanation. My finding was in line with the growing evidence provided by

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cross-cultural psychological research about East Asians' greater sensitivity to situational influences when making causal attributions (Choi, Nisbett, & Norenzayan, 1999). My study contributes to the theory of self-serving bias by suggesting that cross-cultural differences in sensitivity to situational influences could be an important moderating factor. In brief, by following the procedure of the original study, I managed to keep the differences in the set of auxiliary hypotheses associated with the two studies to the minimum. This, in turn, helped me work out an explanation for the differences in results. For the theory application approach, I would, therefore, suggest more replications, in the form of empirical generalization, of important Western-based studies in a Chinese context, provided that the measurement or procedure of the original study is not so context specific as to defy meaningful replication.

Theory Creation and Theory Proliferation

The theory creation approach is surely attractive to Chinese management researchers. What is more exciting than having what Barney and Zhang call a Chinese theory of management, which uses uniquely Chinese concepts to explain unique Chinese phenomena? The feeling is like wearing a tailor-made suit rather than a mass-produced one. Putting aside the excitement, though, I here offer a cautionary note.

According to the logic of the theory creation approach, management researchers based in other non-Western countries should also break out of the straitjacket of Western management theories and develop their own theories. Note that indigenous people of a country often think that their motherland is unique. Russians, for example, are not likely to view a Chinese theory of management as capable of explaining their unique phenomena, though both countries are emerging economies, share a socialist legacy and are collectivistic in cultural orientation. In fact, Michailova and Hutchings' (2006) study indicates some interesting differences in individual knowledge sharing behaviour in Chinese and Russian organizations. Thus, if the theory creation approach is rigorously pursued worldwide, we shall soon see not only Chinese theories of management, but also management theories of various nationalities and sub-nationalities (such as Shanghainese and Scottish). Would this be a healthy development for the management discipline as a whole?

Koontz (1961) used the term 'management theory jungle' to describe the various schools of management theory that existed in the discipline more than four decades ago. Similarly, more than two decades ago, Pfeffer (1982: 1) exclaimed that 'the domain of organization theory is coming to resemble more of a weed patch than a well-tended garden'. Proliferation of theories may lead to conceptual confusion, which arises when a number of theories (strong, weak, or even false) provide incompatible or even contradictory explanations of the same phenomenon. Moreover, managers may find the disparate explanations or conflicting

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recommendations derived from various theories perplexing (Koontz, 1980). Worse still, the trend of globalization aggravates the confusion. Suppose a researcher conducts an intensive case study of the human resources management system of a joint venture in Brazil formed by a Chinese firm and an Indian firm. Should she apply a Brazilian, Chinese or Indian theory of human resources management, assuming that such theories exist? Following the spirit of the theory creation approach, perhaps she should develop a Brazilian–Chinese–Indian theory of human resources management.

While I doubt the feasibility of developing grand, universal theories in social science, as Marx and Freud attempted, that are applicable to the whole of humanity, I have serious reservation about creating highly indigenous theories that have very narrow domains of application. In addition to the problems associated with theory proliferation mentioned above, the latter approach is not conducive to the development of a global research community in which people communicate, interact and share ideas. Highly indigenous theories would lead to a highly fragmented global research community, if there is such a community at all. Researchers of Chinese theories of management may not be motivated to interact with their counterparts of, say, Estonian theories of management if both believe that their theories have little in common. (If they believe otherwise, why should there be two sets of theories in the first place?) At the end of the day, we are all *Homo sapiens*. We eat when we are hungry, drink when thirsty and sleep when tired. After all, are we so different from each other that highly indigenous theories are needed?

The harm of theory proliferation can be alleviated if theories are rigorously tested and those that repeatedly fail a test are rejected and eliminated from the discipline. Unfortunately, the fact is: 'The theories of the 1970s, for instance, continue to hang on independent of empirical confirmation, and efforts at disconfirmation are both rare and relatively ineffective. To our knowledge, no organizational theory has ever been "rejected" (as opposed to "falsified")' (Davis & Marquis, 2005: 340). In other words, once a management theory is created, probably it will never die, even in the face of mounting negative empirical evidence.

There are a number of reasons why falsification is difficult in science in general and in management in particular. Because of space limitations, I only discuss two major reasons here. First, the Duhem-Quine thesis states that it is impossible to falsify single hypotheses because a test system always consists of conjunctions of hypotheses which are being tested (Cross, 1982). As discussed, if a principal hypothesis, H, that is derived from a theory is found to be in conflict with some empirical evidence, all we can say is that the conjunction of H with a set of auxiliary hypotheses, A, is falsified. Supporters of the theory may attribute the cause of the falsification to certain auxiliary hypotheses and save the principal hypothesis and, thus, the theory from being falsified (Losee, 2005). Conclusive falsification of a theory, therefore, often requires the accumulation of negative empirical evidence to the extent that supporters of the theory can no longer justifiably attribute all the

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incidents of falsification to auxiliary hypotheses. It can be a long, daunting process.[3]

Note that the Duhem-Quine thesis originates from the natural sciences where refutation of theories does occur from time to time. For instance, nowadays, no one would accept the theory that the sun revolves around the earth. Within a social science discipline such as management, falsification of theories is more difficult. A second main reason for this difficulty is that most tests of management theories are conducted under the conditions of an open rather than a closed system. Bhaskar (1978: 70) defines a closed system as 'one in which a constant conjunction of events obtains; i.e. in which an event of type a is invariably accompanied by an event of type b'. Scientists need to conduct experiments precisely because of the open character of the world in which events are subject to diverse causal variations. Conditions of closure are rarely possible in the social sciences, as illustrated by the artificiality of laboratory experiments performed by social psychologists (Harré & Second, 1972). When studies are conducted in open systems, the problems associated with isolating the relations of interest from other confounding effects are aggravated. In case a result does not support the principal hypothesis, it should not be difficult to come up with explanations that can save the related theory from falsification.

To summarize, the theory creation approach begets theory proliferation, which further fragments our already fragmented discipline. I have no intrinsic objection to developing indigenous theories but would take a cautious stance. Before we start creating a Chinese theory of management, we should seriously ask ourselves, 'Is the phenomenon in question really so unique that only a novel theory can provide an adequate explanation?' As I elaborate below, focusing our attention on studying empirical regularities can help answer this question. Once a theory is created, we have to adopt a highly critical attitude toward the theory and subject it to rigorous tests (Popper, 1959). The aim is to make sure that the theory deserves to remain in the field rather than to be eliminated from existence. The persistence of weak, or even false, theories hinders the development of good theories (Arend, 2006) and obstructs scientific progress (Pfeffer, 1993). Hence, a false Chinese theory of management will do more harm than not having the theory in the first place.

Finally, contextualization should refer to identifying not only differences, but also commonalities across contexts. While we engage in context sensitive indigenous theorizing, we should also pay attention to the possibility of integrating indigenous concepts and theories. As Tsui (2007: 1359) argues, 'separating out the contextual explanations (those that might generalize) from the idiosyncratic explanations (those that are truly local or indigenous) may be the starting point of new universal theories.' An example of such an integration attempt is the development of the concept of cronyism by my colleagues and me (Khatri, Tsang, & Begley, 2006). We believe the conceptual domain of cronyism has the potential to encompass similar concepts in various cultures, such as *guanxi* in Chinese communities, *blat* in Russia, *compadrazgo* in Mexico and the 'old boys' club' in the UK and the USA.

From Theory to Empirical Regularity

The concerns of Barney and Zhang (2009) and Whetten (2009) focus on theory testing and development. Recently, there have also been arguments that editors and reviewers need to reconsider their single-minded focus on contributions to theory when evaluating journal submissions (Hambrick, 2007; Helfat, 2007; Miller, 2007). Journals should welcome studies that uncover important empirical regularities, which may not fall neatly within the domain of any existing theory, without the usual requirement of linking findings to theory. This suggestion is especially relevant to Chinese management research; when a phenomenon in a Chinese context cannot be explained by any Western theories, this does not necessarily imply that a new theory has to be created immediately.

A good example is my exploratory study of the relationship between superstition and business decision-making. It is a well-known fact in Chinese business communities that some managers try to seek supernatural sources of information, such as fortune-telling and praying to God, when they make strategic decisions. In fact, when I worked as a corporate banker in HSBC, Hong Kong, before my career change, some of my clients frankly admitted that they engaged in superstitious activities. This is surely an important phenomenon for Chinese management. There have been numerous reports in newspapers and business magazines about how superstition has affected strategic decision-making.[4] A simple yet illustrative example is that the opening ceremony of the Beijing Olympics was scheduled to be held at 8 PM on August 8, 2008, because eight is a lucky number in Chinese culture. It is also likely that Beijing did not compete to host the 2004 summer Olympics after losing the 2000 summer Olympics to Sydney because four is an unlucky number. Moreover, the phenomenon is not limited to the Chinese. For instance, after the former Thai prime minister, Thaksin Shinawatra, became the new boss of the Manchester City Football Club, crystals were buried under the pitch and symbols placed at various spots around the club's stadium. Garry Cook, who is the chief executive of the club and was formerly with Nike, said, 'We were talking about *feng shui* and I thought it would be great to bring it to Manchester City' (Keegan, 2008). In short, superstitious beliefs do matter and matter a great deal.

When I embarked on my study by searching the literature, much to my surprise, I failed to find a single academic study of superstition and decision-making.[5] Nevertheless, my literature review and conversations with other colleagues indicate that Indian, Japanese, Korean and Thai managers are also rather superstitious. That is, as shown by the abovementioned example of the Manchester City Football Club, superstitious decision-making is not unique to Chinese culture. While there could be various reasons for this lack of research interest, one major reason is aptly reflected by the knee-jerk reaction of a Taiwanese scholar to whom I mentioned I was studying this topic – 'Oh no! This sort of topic can't get into a good journal.'

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Fortunately, I was able to publish my study in *Organization Studies* (Tsang, 2004a) and a practitioner version of it in the *Academy of Management Executive* (Tsang, 2004b), both of which are well-respected journals. *Organization Studies* was the first journal to which I submitted my manuscript. I did not even bother to try journals such as the *Academy of Management Journal* and *Administrative Science Quarterly* because I knew very well that these so-called top journals would not have any room for an exploratory study that did not fit well into any existing theoretical framework, no matter how important the empirical regularity the study managed to identify.

In my study, I systematically described and analysed the phenomenon. Although existing theories of decision-making failed to adequately explain the phenomenon, I tried to link it to two important concepts of the decision-making literature – rationality and uncertainty. According to the theory creation approach, my study presented a golden opportunity for developing a new theory to explain how superstition affects decision-making in Chinese business communities. Yet I thought my research was exploratory in nature and, at that stage of research, the priority should be to carefully present my findings. While the temptation of becoming a theory creator was great, I was contented with contributing to the empirical foundation that paves the way for future theory creation, whether by me or by other researchers.

One of the three anonymous *Organization Studies* reviewers saw the potential of my study and commented that it 'could become a classic *Organization Studies* piece in the spirit of its founder, David Hickson, who believed that rigor and boredom did not need to go together. It is pieces like [this] that often make a single *Organization Studies* issue more interesting to me than the entire year's crop of *AMJ*s.' After my study was published, I received e-mails from researchers in Brazil and Mexico, saying that they encountered a similar phenomenon in their countries. Moreover, the comment was made in one e-mail that my 'observation that "superstition" is used to resolve choices among apparently equivalent (by rational standards) alternatives is insightful' (James March, 2006, personal communication). In spite of these encouraging comments, the current theory straitjacket imposed by many management journals only stifles this type of research.

Studying empirical regularities represents a viable alternative to either the theory application or theory creation approach. A key advantage of this alternative concerns the theory-ladenness of observation. Theories often 'shape the kinds of questions we ask, the sorts of data we deem acceptable and the way we interpret our findings' (Miller, 2007: 179). Hanson (1958) and Kuhn (1962) proposed very influential arguments that the theory held by a researcher would influence his or her observation. Although this view has some defects (see Hunt, 1994), it is likely that the pressure of fitting one's study into a theory straitjacket will compromise the accuracy of observation. Not yielding to such pressure, the alternative of studying empirical regularities may result in more accurate observations. When I collected data on superstitious decision-making through interviews with managers and fortune-tellers, I focused on analysing the meaning of their responses. Not worrying whether my data were consistent with an existing theory or whether they converged on the possibility of a new theory enabled me to present, I believe, an unbiased view of the phenomenon.

Another advantage of this alternative is that researchers who focus on empirical regularities are not hurried into creating new theories prematurely, thereby reducing the chance of adding a weak theory to the discipline. Multiple studies of the same phenomenon are required to confirm that it truly represents an empirical regularity (Helfat, 2007). Replication again plays a critical role here. 'Only by such repetitions can we convince ourselves that we are not dealing with a mere isolated "coincidence," but with events which, on account of their regularity and reproducibility, are in principle inter-subjectively testable', argued Popper (1959: 45). A danger of the theory creation approach is the real possibility that, without a solid empirical foundation, a new theory is based on a transient, idiosyncratic phenomenon instead of an empirical regularity. This possibility, coupled with the fact that we seldom subject theories to rigorous empirical tests (Tsang, 2006), may lead to the perpetuation of a weak theory.

Studying empirical regularities is consistent with Tsui's (2007: 1359) suggestion that '[t]hrough the process of deep contextualization – identifying how context enhances or modifies understanding of a common phenomenon across contexts – the possibility emerges of discovering context-free regularities.' If superstitious decision-making also exists in other cultures, it makes sense to conduct deep contextualization research in these cultures and carefully compare the findings across cultures before contemplating the creation of one new theory to explain the phenomenon as a whole, drawing on the commonalities observed across cultures. This approach would be better than separately creating, say, Chinese, Indian, Brazilian and Mexican theories of superstitious decision-making.

CONCLUSION

A discussion of contextualizing Chinese management research would be incomplete without taking into account the current situation of Chinese management researchers. The business schools of East Asian universities, with which most of these researchers are associated, are under increasing pressure to publish in top journals (Leung, 2007). Unfortunately, many management journal editors, especially those of top journals, have two distinct yet related obsessions – theory based research and innovative research. These obsessions have unwittingly hindered theory development. The former obsession pushes researchers to create a theory or theoretical framework prematurely, while the latter discourages replicated studies that are essential for testing existing theories. As a result, we have a plethora of fancy theories, most of which have not been subjected to rigorous empirical tests.

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Against this background, I have to admit that some of my abovementioned suggestions, such as conducting replications and studying empirical regularities, are not likely to become popular among Chinese management researchers. It all boils down to the question: do we have the courage to go against the current?

NOTES

This article originated as a commentary on the papers by Barney and Zhang and by Whetten in the '*MOR* special symposium – Exploitation or exploration: The future of Chinese management research' session at the third biennial conference of the International Association for Chinese Management Research, Guangzhou, China, June 2008.

I would like to thank Anne Tsui and Kwok Leung for their helpful comments; I remain responsible for any errors in the commentary.

[1] By objective reality, I mean here a reality that is independent of researchers' perception and cognition (Fay, 1996). This reality can be physical, such as planets, or social, such as culture. Although social reality is constituted by human interactions and is intricately related to researchers themselves, it can still be considered independent (Sayer, 2000).

[2] The fact that Astley (1985) and Astley and Zammuto (1992) were published in *Administrative Science Quarterly* and *Organization Science*, respectively, suggests that constructivism has considerable influence in the management discipline. Interested readers may refer to Kwan and Tsang's (2001) critique of constructivism.

[3] Another possibility is that a new theory, which provides a better explanation of the same phenomenon, comes along and replaces the existing theory. Even in this case, judging which of the two theories provides a better explanation can be a lengthy process requiring a series of empirical tests.

[4] Superstition has also affected crucial personal decisions for the Chinese. An excellent example is the fact that more babies are generally born in the Year of the Dragon, a supposedly auspicious year. This personal decision has far reaching impacts on the government in terms of education and healthcare planning.

[5] Later Chi-Nien Chung and Chung-Ming Lau assisted me with searching the Chinese academic literature in Taiwan and also failed to locate any such studies. In early 2005, after my study was published, a graduate student of library science searched the English literature for me and reported that there were no studies, other than mine, on this topic.

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