



Nankai Business Review International

Rethinking the path of business research in the west: what direction is better for business research in China?:

Allen S. Lee Xin Luo

Article information:

To cite this document:

Allen S. Lee Xin Luo , (2014), "Rethinking the path of business research in the west: what direction is better for business research in China?", Nankai Business Review International, Vol. 5 Iss 4 pp. -

Permanent link to this document:

<http://dx.doi.org/10.1108/NBRI-03-2014-0016>

Downloaded on: 12 October 2014, At: 18:38 (PT)

References: this document contains references to 0 other documents.

To copy this document: permissions@emeraldinsight.com

The fulltext of this document has been downloaded 9 times since 2014*

Access to this document was granted through an Emerald subscription provided by 454211 []

For Authors

If you would like to write for this, or any other Emerald publication, then please use our Emerald for Authors service information about how to choose which publication to write for and submission guidelines are available for all. Please visit www.emeraldinsight.com/authors for more information.

About Emerald www.emeraldinsight.com

Emerald is a global publisher linking research and practice to the benefit of society. The company manages a portfolio of more than 290 journals and over 2,350 books and book series volumes, as well as providing an extensive range of online products and additional customer resources and services.

Emerald is both COUNTER 4 and TRANSFER compliant. The organization is a partner of the Committee on Publication Ethics (COPE) and also works with Portico and the LOCKSS initiative for digital archive preservation.

*Related content and download information correct at time of download.

RETHINKING THE PATH OF BUSINESS RESEARCH IN THE WEST: WHAT DIRECTION IS BETTER FOR BUSINESS RESEARCH IN CHINA?

1. Introduction

In this essay, we will not be directly discussing any topics in finance, economics, information systems, accounting, marketing, management, or other substantive areas of interest to professors in business schools. Instead of discussing the topics we do research on, we will discuss *how we do research*. In other words, the subject matter that we will examine is the *knowledge* that we university scholars have and how we develop it. This means that our topic is actually philosophy, insofar as philosophy is the study of knowledge. This is entirely appropriate, given that many or most of the readers of this essay have, or are seeking, doctor of philosophy degrees.

Like all overseas Chinese people, we are proud of China's economic progress and we want China to continue forging ahead on its path to success. At the same time, as Americans, we have seen how American business-school research has succeeded, and failed. Business-school researchers in China should take cues from the successes, but at the same time, they should also learn from and avoid the failures.

For at least the last three decades, the "top" business-school research journals have been publishing articles that are, for the most part, highly theoretical and abstract. In fact, the overall body of published business-school research can readily give the impression that it is so highly theoretical and abstract that no manager, businessperson, or even MBA student can be reasonably expected to read or understand it. Indeed, a person needs to have a Ph.D. in order to properly assess it, much less write it.

Some people call this the "rigor versus relevance" problem: It seems that the more scientifically rigorous that research is, the less practically relevant it is. And likewise, the more practically relevant that research is, the less scientifically rigorous it is. We will avoid using the labels, "rigor" and "relevance." These labels are simplistic and glib. They disguise important points. And the framing of the problem as "rigor versus relevance" is wrong. We will return to this point later in this essay.

Instead, we will frame the problem of business-school research with four questions. First, what have been the successes of American business-school research? Second, what explains these successes? Third, what have been the failures of American business-school research? And fourth, if we are to learn from mistakes already made, what can future business-school research, including future business-school research in China, do differently and do better, so that the mistakes are not repeated?

2. The First Question

To address “What have been the successes of American business school research?”, we turn to the “Final Report of the AACSB International Impact of Research Task Force,”¹ which was released in 2008. “AACSB” is the Association to Advance Collegiate Schools of Business. The AACSB International is one of the major accreditation bodies for business schools in the world and the major accreditation body for business schools in the United States. In this report (pp. 18-19), the AACSB gives credit to what we recognize as *basic research* for the following successes:

In finance, examples of successful research include “the theories of portfolio selection,” “irrelevance of capital structure,” “capital asset pricing,” “efficient markets,” “option pricing,” and “agency theory.” In accounting, examples of successful research are William Beaver’s demonstration, based on efficient market theory, “that the stock market reacts strongly to corporate earnings announcements” and Watts and Zimmerman’s work, based on agency theory, “that addresses how managers choose among accounting methods.” In marketing, examples of successful research are Keller’s contributions to “understanding the construction, measurement, and management of brands” and Green and Rao’s “conjoint analysis approaches to consumer research based on seminal work by Luce and Tukey in mathematical psychology... [where, today, their research] is widely used to test new product designs and assess the appeal of advertisements.” In information systems, the AACSB report mentions “the research of Malhotra [which] has helped companies to understand why knowledge management systems fail” and the report also mentions that “Bass’s Diffusion Model has had practical applications for forecasting demand of new technologies.” Finally, in management, examples of successful research are Hofstede’s “comprehensive study of how values in the workplace are influenced by culture” and Vroom’s “seminal contributions to understanding employee motivation.”

3. The Second Question

These successes, identified in the AACSB report, lead to the next question, “what explains these successes of American business-school research?”

We offer an answer in two parts: First, these particular examples were successful because, as the AACSB report emphasized, they all involved *basic research* and, second, these examples enjoyed conditions that were favorable to *basic research*.

As for what *basic research* is, we describe it as research that attempts to model itself on how research is conducted in the natural sciences. Historically, this way of doing research has been so successful in the study of nature that it has also become used extensively in the

¹<http://www.aacsb.edu/publications/researchreports/currentreports/impact-of-research.pdf>

study of people, organizations, and society. Research that is called “scientific” has become synonymous with research that follows the natural-science model.

In America, university researchers and laypeople alike all presume that “science” is the best, highest, and most objective form of knowledge. Let me share with you a story that has been attributed to Dr. Salvatore March, who is the David K. Wilson Professor of Management at Vanderbilt University and one of the most eminent design-science researchers in the field of information systems. According to this story, the “real” sciences do not have “science” in their names, such as physics, chemistry, biology, astronomy, geology, and so forth. But the fields that really hunger to become known as a science all put science in their names: management science, information science, marketing science, decision science, design science, and so forth. This is a clear sign that the status of being “scientific” is highly prized in Western society.

Business-school researchers who follow the natural-science model of how to do research apply tools that they believe to be used in physics: mathematical equations, including independent and dependent variables; the concept of causality; quantitative data; experiments; and predictions and hypotheses – among other things. The *Encyclopædia Britannica* captures this well:

Out of the ferment of the Renaissance and Reformation there arose a new view of science, bringing about the following transformations: the reeducation of common sense in favour of abstract reasoning; the substitution of a quantitative for a qualitative view of nature; the view of nature as a machine rather than as an organism; the development of an experimental method that sought definite answers to certain limited questions couched in the framework of specific theories; [and] the acceptance of new criteria for explanation, stressing the ‘how’ rather than the ‘why’ that had characterized the Aristotelian search for final causes.²

This description is extremely laudatory of the natural sciences. We agree that the scientific method of the natural sciences deserves much, or even most, of the credit for liberating Western civilization in Europe from the dark ages to modernity, and for enabling the West to advance past the civilizations of China, India, and Japan, not only in science, but also in technology and industry. It is no wonder, then, that most people accept, as an article of faith, that the natural-science model is the best path to good knowledge.

²“physical science.” *Encyclopædia Britannica*. Encyclopædia Britannica Online Academic Edition. Encyclopædia Britannica Inc., 2012. Web. 23 May. 2012. <<http://www.britannica.com.proxy.library.vcu.edu/EBchecked/topic/458717/physical-science>>.

Research that follows the natural-science model is exactly what the term “basic research” means to scholars in general, including what it means to the AACSB in particular. And the research successes identified in the AACSB report all follow the natural-science model.

However, as powerful as it is, the *natural-science model of basic research* does not always work. It only works under certain favorable conditions. We will mention just three of these conditions. First, for the natural-science model of basic research to work, the subject matter needs to be objective, quantifiable, and measurable – and this condition is often, but not always, satisfied in the many different areas of business study. Second, for the natural-science model to work, the problem being studied needs to be recurrent or replicable. Where we have annual budget cycles and economic cycles, this condition can be met in those business research areas where business cycles and economic cycles are important, such as in finance and economics.

But then, other conditions must be met too. For the third condition for the natural-science model to work, human values need to be eliminated or minimized, so as not to “bias” or “contaminate” the scientific findings. However, the subject matter of the social sciences – people – carry their own values, which is different from the subject matter of the natural sciences, such as rocks, plants, and molecules. Rocks, plants, and molecules do not have a consciousness in which they think about themselves, nor do they have complicated values (or any other values) that affect their behavior. Studying human beings, who have values, as the subject matter in a scholarly investigation poses major problems and complications in research. These are problems and complications that the natural sciences do not face when they study physical matter. But in the social sciences, this condition can sometimes be satisfied if people’s values can be captured in a quantifiable form, such as demand curves and utility functions – which is what finance and economics often do. However, not all important human values can be captured in this, or any other, quantifiable form.

Generally speaking, where a clever researcher in any area of business – not just finance and economics, but also marketing, accounting, information systems, and management – can figure out how to satisfy these conditions for the natural-science model to work, the result can be good basic research.

Actually, there are more than just these three conditions that need to be met for the *natural-science model of basic research* to work, but my mentioning just these three will suffice to illustrate the point that the natural-science model, as powerful as it can be, is not always so powerful or even applicable.

Now, what happens when these three conditions are not met?

4. The Third Questions

This brings us to our third question: “What have been the failures of American business-school research?” We will mention two complications regarding the *natural-science model of basic research*.

One complication is that not all business-school research can be “force fit” to the three conditions. We will focus on just the third condition: The problems and subject matters that business-school researchers need to examine are very often filled with human values. For example, in information systems, the implementation of technology often fails because of the conflicting values of opposing stakeholders in the same organization. In management, the administration of human resources is often made unmanageable by the rich presence of human values in organizational culture. In marketing, the human dynamics of buying and selling are likewise filled with human values. And in finance, it was human values that have been at the center of some recent scandals in the United States – the scandal involving the JP Morgan Chase trading loss 2 billion dollars,³ the regulatory problems involving the Facebook IPO (“regulators are concerned that banks may have shared information only with certain clients, rather than broadly with investors”⁴), and the infamous financial meltdown of 2008-2009 related to credit default swaps, derivatives, and government regulations. The *natural-science model of basic research* is extremely powerful, but it is not powerful for examining everything, such as subject matter where human values are located at the core of the problem.

And even if the conditions for the *natural-science model of basic research* are all favorable, there is still another complication: the research can nonetheless turn out to be useless to corporations, government, managers, and business students. To explain this, we now turn your attention to the philosophy of science and the history of science in order to explain how this complication is not easily resolved. In particular, we turn your attention to one of the most famous and seminal books written in the philosophy and history of science: Thomas Kuhn’s *The Structure of Scientific Revolutions*.⁵ This book did not originate the term “paradigm,” but it can be credited for popularizing it not only among researchers, but also among members of the public. There are many themes in this book.

One of the major themes in *The Structure of Scientific Revolutions* is what it calls “puzzle solving.” At the center of a “puzzle” is what a community of scientific researchers accepts as their current theory. A puzzle is something observed that the theory cannot yet

³ Silver-Greenberg, J. and Eavis, P., “JPMorgan Discloses \$2 Billion in Trading Losses,” *New York Times*, 10 May 2012, <http://dealbook.nytimes.com/2012/05/10/jpmorgan-discloses-significant-losses-in-trading-group/>.

⁴ Rusli, E. and De La Merced, M. “Facebook I.P.O. Raises Regulatory Concerns,” *New York Times*, 22 May 2012, <http://dealbook.nytimes.com/2012/05/22/facebook-i-p-o-raises-regulatory-concerns/>.

⁵ Kuhn, T. S. 1996 (originally published, 1962). *The Structure of Scientific Revolutions*, University of Chicago Press.

explain. A puzzle is a mismatch between what the theory predicts and what is actually observed. The history of the natural sciences shows that the primary goal of what Kuhn calls “normal science” is to solve these “puzzles” – to revise and improve the scientific community’s theory so that it can better fit, and predict, the data.

However, for the goal of puzzle solving, no real-world problem necessarily has any importance. The central concern in scientific research is necessarily not any real-world problem. Rather, the central concern is any mismatch between what the scientific community’s theory predicts to be the case and what the scientific community actually observes to be the case. The mismatch could happen in *any* empirical situation, even one that happens to be unimportant and irrelevant to business corporations and government. Now, would it be possible for a scientific community to choose an important real-world problem to apply its current theory to? In principle, this is possible, but in practice, what we see is that scientific researchers, including business-school researchers who do *basic research*, choose problems that are suitable for their theory to examine, *which is not necessarily the same as problems that are relevant to the real world*. Is it possible that there could be an overlap? Yes, but this would be a coincidence. An overlap – where a problem is relevant to the theory and relevant to the real world at the same time – is the exception, not the rule.

To identify the failures of American business-school research, we turn your attention to the research journals that American business-school professors must publish in. These are the journals that they must publish in if they wish to become tenured and if they wish to be promoted from assistant professor to associate professor to full professor. These research journals are filled with research articles that are excellent examples of basic research, but only in a few cases are these research articles also useful to the real world of business and government. The majority of the research articles in the top research journals are examples of failures by the measure of usefulness. To illustrate this point, let us share with you a case study from the experience of the first author of this essay.

This case study is about the top research journal, *MIS Quarterly*, whose editorial board the first author was a member of for 15 years, including 3 years as the editor-in-chief. The first author was always aware of the need for business-school research to be relevant to the real-world of business, so when he was editor-in-chief, he worked hard with some of his colleagues to found a new journal, *MIS Quarterly Executive*, which has indeed become a very successful journal, thanks to the work of people such as John Rockart, Jeanne Ross, Carol Brown, and Alan Dennis. His original vision for the new journal was to identify past articles that had been published in *MIS Quarterly* in order to see which ones could be “translated” or rewritten to be useful for a practitioner audience, so that the translated or rewritten article might be published in *MIS Quarterly Executive*. For this purpose, the first author had initially reviewed five years’ worth of published articles in *MIS Quarterly*, but was able to identify only one suitable article. Only one! Why? Almost all of the articles in *MIS Quarterly* follow the *natural-science model of basic research*, where the authors of the articles chose problems that were suitable for their theories to examine, not problems that were relevant to the real world. On this measure, the research published in *MIS Quarterly* is no different from most of the research published in other business-school research journals.

Before we continue to our fourth and final question, we will draw attention to one additional important lesson from the philosophy of science and history of science. It is a lesson that Thomas Kuhn identifies in his book *The Structure of Scientific Revolutions*. No individual scientific researcher may or can exercise free will, or free choice, in choosing what theory to build and test, or what problem to apply a theory to. Any individual scientific researcher is a member of a larger scientific community – and it is the larger scientific community that places strict bounds on what theories and what problems may be considered. Not even a business-school dean or a university president has the power to tell a research professor what theory or problem to examine. Research journals, which are independent of any given university, hold all the power, and it is the research journals which establish the research agendas – agendas that place restrictions on the theories and problems that these journals will allow to be investigated. The result has been that the majority of business-school research, published in the top journals, has followed the *natural-science model of basic research*, where the authors of the published articles have chosen problems that are relevant to their theory, not problems that are relevant to the real world (except where this might happen by coincidence).

In American business schools, the power of the journals cannot be underestimated. Publishing in these journals is a necessary condition for an assistant professor to become tenured, to be promoted to associate professor and then to full professor. Not publishing in these journals means that an assistant professor will lose his or her job. And not publishing in these journals means that an assistant professor who has lost his or her job will face problems in trying to find a new job.

Earlier, we said that we preferred to avoid the labels “rigor” and “relevance” because they disguise some important issues. Well, perhaps the most important issues that they disguise are the power of the journals and the politics of the tenure-and-promotion process.

So, the problem is about more than just logic, data collection, research methods, experimental design, and site selection. In other words, the problem is about more than the quality of the research itself. The problem is also about the social and the political.

Adding to the social and political aspects of the problem is what we may now appropriately call the tyranny of the journal lists. It is bad enough that individual business schools in the United States are developing lists of journals that their professors must publish in, but now, the problem is getting worse. In recent years, there is new pressure in the form of the UT Dallas list of 24 journals,⁶ the *Financial Times* list of 45 journals,⁷ and the

⁶<http://jindal.utdallas.edu/the-utd-top-100-business-school-research-rankings/rankings-by-journal/>

⁷<http://www.ft.com/intl/cms/s/2/3405a512-5cbb-11e1-8f1f-00144feabdc0.html>

Bloomberg BusinessWeek list of 20 journals.⁸ In these journal lists, the vast majority of the journals emphasize the *natural-science model of basic research*.

Up to this point, we have covered the first three of the total of four questions. We have completed a diagnosis of the successes and failures of American business-school research. Now, we will proceed to the fourth question, where we will offer a prescription for the failures we have diagnosed.

5. The Last of Four Questions

“What can future business-school research, including future business-school research in China, do differently and do better, so that the mistakes of past business-school research are not repeated?”

As for what to do in the future, the response we would give to an American audience is different from the answer we would give to a Chinese audience. In America, the social and political infrastructure of the business-school research community is so well entrenched that there is very little latitude in what one would be able to do in order to solve the problem. It is as if the situation for producing useful theory is stalemated. However, to a Chinese audience, we can say this: you have a choice for how to proceed, so why choose the American situation of stalemate?

Now, if ever there could be a nation on earth that can challenge, rather than merely accept and import, the American model, that nation is China. Unlike the United States, which is captured by the inertia of its own huge, sluggish legacy infrastructure, China has the advantage of only now establishing what its infrastructure will be. And we are not only talking about a nation’s economic, industrial, and physical infrastructure; we are also talking about a nation’s university-research infrastructure, which includes how its scientific research communities are organized.

Let us put this in the historical context. As we described earlier, the *natural-science model of basic research* has its origins in liberating western civilization in Europe from the dark ages to modernity. Now, what possible relevance do the European dark ages have for business-school research in China today? None. So why should China choose the *natural-science model of basic research*?

As for what future business-school research can do differently and do better in China than in the United States, we will mention just three key operational elements: (1) Business-school research in China should develop its own journal lists, where (2) the journals are not

⁸http://www.businessweek.com/bschools/content/nov2008/bs20081113_320726_page_2.htm

dominated by the *natural-science model of basic research*, but where (3) business-school research, as performed by professors and doctoral students, will instead largely follow the *professional-practice model of pragmatic research*.

Of course, this opens up the big question, exactly what is “the professional-practice model of pragmatic research”? We would be more than happy to provide the answer to this in the future; this is a different, additional topic that requires its own dedicated treatment. For now, we will just say that the answer has to do with (1) what the Nobel laureate Herbert Simon calls *The Sciences of the Artificial*,⁹ (2) how certain professions, such as medicine and engineering, have successfully used the natural sciences without themselves becoming natural sciences and have still been able to do pragmatic research all along, and (3) how certain other professions, such as law and architecture, have succeeded in producing research that is pragmatic and truly scholarly, without any recourse to the natural sciences at all.

Before we offer our concluding remarks, we must emphasize that that the *natural-science model of basic research* is, indeed, a valid way of doing business-school research. We have not been criticizing the *natural-science model of basic research*. Rather, we have been criticizing how business-school research has used the *natural-science model of basic research* as if it were the general case (or the only case) of how business-school research should be done – when, in fact, it is just a special case or a subset of good business-school research in general. We have many colleagues who have made great research contributions using the *natural-science model of basic research*. We are not suggesting that they disavow their past research achievements, nor are we suggesting that they need to change their research approach. What we are recommending is for future generations of business-school researchers, beginning with today’s doctoral students, to open themselves up to new ways of doing research, where they would use the *professional-practice model of pragmatic research*, not necessarily instead of, but in addition to, the *natural-science model of basic research*.

6. Conclusion

Earlier, we mentioned that we are overseas Chinese and that we are Americans. We care deeply about both of our countries and we want both to succeed. What we hope will happen is that business-school research in China will reject the single path of the *natural-science model of basic research* and choose to also travel the path of the *professional-practice model of pragmatic research*. The greater progress that China will make in business-school research will then, in the spirit of competition, cause business-school research in the United States to right its own course in order to catch up. And in that way, both of our countries will become better off.

⁹ Simon, H. A. 1996, 1969. *The Sciences of the Artificial*,⁹ Cambridge, MA: MIT Press.

Acknowledgements

This is an edited version of Allen S. Lee's keynote address at the Fourth Symposium on Financial Intelligence and Risk Management and the Fifth International Workshop on Electronic Payment and Electronic Commerce in China (FIRM-EPECC 2012), on June 2, 2012 at the Sichuan Key Lab of Financial Intelligence and Financial Engineering (FIFE) of the Southwestern University of Finance of Economics (SWUFE), in Chengdu, Sichuan Province, People's Republic of China.

About the authors

Dr. Allen S. Lee has been a Full Professor of Information Systems at Virginia Commonwealth University, USA, since 1998 and, in 2012, was named a Dean's Scholar Professor. He has served as associate dean at both VCU and McGill University, as editor-in-chief of *MIS Quarterly*, and as a founding senior editor of *MIS Quarterly Executive*. His publications have typically taken a philosophy-of-science perspective in examining research methodology and designing research methods for the study of information systems. In 2005, he was named a Fellow of the Association for Information Systems. A member of the Circle of Compadres of the Information Systems Doctoral Students Association of the KPMG PhD Project, he was also a founder of the organization Chinese American Professors of Information Systems. He earned his bachelor's degree from Cornell University, his master's degree from the University of California at Berkeley, and his doctoral degree from the Massachusetts Institute of Technology. Born and raised in New York City, he is a graduate of Brooklyn Technical High School.

Dr. Xin (Robert) Luo is an Associate Professor and Endowed Anderson Alumni Professor of MIS and Information Assurance at the University of New Mexico, USA. He is the Associate Director of Center for Information Assurance Research and Education at UNM. He has also been named Bill Daniels Business Ethics Fellow and Anderson Foundation Fellow. He received his Ph.D. in Information Systems from Mississippi State University, USA. He has published research papers in leading journals including *Communications of the ACM*, *Computers & Security*, *Decision Support Systems*, *European Journal of Information Systems*, *Information Systems Journal*, *Journal of the AIS*, and *Journal of Strategic Information*

Systems, etc. His research interests center around information assurance, innovative technologies, and cross-cultural IT management and strategy. He currently serves as an ad-hoc Associate Editor for *MIS Quarterly* and an Associate Editor for *European Journal of Information Systems*, *Journal of Electronic Commerce Research*, and *International Conference on Information Systems*. He can be contacted at xinluo@unm.edu.