HAVING IT ALL: RIGOR VERSUS RELEVANCE IN SUPPLY CHAIN MANAGEMENT RESEARCH*

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Congratulations to Craig Carter, Lisa Ellram and Lutz Kaufmann as they embark on their ambitious repositioning of the *Journal of Supply Chain Management*. Their quest brings to mind the age-old discussion about rigor versus relevance in academic research, which is alive and well in the area of supply chain management research. The discussion goes something like this: supply chain management, by definition, is an applied research field, which is based in the real world. Without problems in real supply chains to study, there would not be a need for supply chain management research. Therefore, because supply chain management researchers study real problems and develop recommendations that are useful for supply chain professionals, their research is somehow of lesser quality than "pure" research that is based on advancing theory. Conversely, if academic researchers focus their efforts on "pure" and theoretically based research, they are rewarded by being able to publish it in geeky academic journals, and the perception seems to be, "the geekier, the better." Of course any self-respecting supply chain professional would not be caught dead reading a geeky academic journal, so any notion of relevance is sacrificed if we strive to do high-quality supply chain management research.

This raises the intriguing question of who it is that we are targeting with our academic research? Is our goal to simply entertain other academic researchers? I hope not. I once heard a manager describe most academic research as "esoteric research written in arcane language and published in obscure journals, with no relevance for the real world." Is this the sort of goal that we had when we entered the academic world as idealistic Ph.D. candidates? What happened along the way to change our perspective?

I would like to put forth the notion that we can have it all. The trick is to marry research questions that are important to supply chain professionals with a strong foundation of theory and rigorous analytical methods. We then need to learn how to translate what we have done into a language and medium that provides an effective conduit to supply chain professionals. Of course, this does not preclude also publishing the geeky academic version in a journal that will be rewarded at our institutions. Is this approach easy? Certainly not. However, it can be done with some careful thought.

Let us start by thinking about journals. There are many journals that at least purport to publish research that is relevant to managers. These journals fall into several categories. First are the very few highly respected journals that target practicing managers, such as *Harvard Business Review*. Second is the large number of what are perhaps best termed "magazines," which we sneer at because they have pictures and shiny pages. Third is a large group of lower-tier journals that publish academic research that does not make it into the top-tier academic journals. Other than the first category, most of these are not journals that we aspire to send our best work to. These journals are not well-respected and sometimes publish findings that are not supported by research or are supported by dubious research.

So what is the answer? Is it necessary to create a journal that is somehow highly respected, while publishing research that is relevant to the business community? Must rigor be sacrificed when studying issues that are relevant to the business community? Do we need to pander to supply chain professionals by feeding them obvious findings that they already know? Do incentives for academics need to be changed so that publication in the sorts of journals described above is more strongly rewarded? To all of these questions, my response is an emphatic no!

GEEKS, EDITORS AND SCIENCE ENVY

What's wrong with basing our best, most rigorous research on questions that are important to the community of supply chain professionals? It is tiresome to hear the argument that such research will not be published in top journals; in fact, I once had a colleague who told me that any journal that used the term "applied" in its statement of purpose could not be considered a top-tier journal. Such statements are just plain wrong! I have never met a journal editor who said that relevant research need not be submitted. Journal editors want to publish research on exciting and interesting topics. What they do not want to publish, however, is *bad* research. There is a big difference between relevant research and bad research, and this is the gap that we need to address, as supply chain management researchers. What is interesting is that

*Invited Comment.

supply chain management practitioners do not benefit from dissemination of the results of bad research, either! If research findings are of dubious validity, no self-respecting supply chain manager will want to apply them in his or her organization. Thus, both the editors of top journals and supply chain management practitioners are interested in the same thing: rigorous research that is based on relevant and important topics. Why, then, is it so difficult for us to actually execute this vision? There are several reasons that come to mind.

First is the perception, among academic researchers, that the more theoretical a paper is, the better. Do not misunderstand — I am not knocking theory. Theory provides the foundation for the best research, and I have always been a proponent of grounding all research in a strong foundation of theory. However, I believe that business researchers, and to some extent, all behavioral researchers, suffer from "science envy." We are trained that the more "scientific" a research project is, the better. Because business research is relatively new in the grand scheme of things, we look to our colleagues in the hard sciences and mathematics as a benchmark, striving to make our research as tightly controlled as their research is. The problem is that it is much easier to control extraneous forces in a chemistry or physics lab than it is in a business organization. We can express business issues as elegant mathematical equations, but not without assuming away some of the very characteristics that make business issues a challenge for supply chain professionals.

Does this mean that science labs or mathematical models are bad benchmarks for us to follow? I do not believe that this is the case. A clean, tightly controlled research design is important, if we are to have any hope of attributing cause and effect. Boiling down a complex problem to its underlying structure is important in developing an understanding of the problem. However, if we talk like scientists or mathematicians when we explain our research to supply chain professionals, they will understandably have a hard time seeing how it is relevant to the problems that they face in their day-to-day work. And if we only study research questions that lend themselves to study in a clean, tightly controlled research design or elegant mathematical formulation, we are missing the boat on some of the most relevant and interesting research questions.

A more likely explanation has to do with how we learn about the issues that are important to supply chain management practitioners and how we study them. There is the perception that relevant issues do not make good research questions and that practitioners are mostly concerned with the latest fads or narrow, short-term issues. Although there are certainly examples of such issues and research, this is by no means a universal truth. Particularly in the area of supply chain management, there are many academically interesting issues that are highly relevant to supply chain professionals. In fact, looking at recent newspaper headlines can provide many fruitful areas for rigorous academic research that deals with issues that are of critical importance to supply chain professionals.

TAKEN FROM THE HEADLINES: THE CASE OF CHINESE SUPPLY CHAINS

A good starting point is the numerous recent stories about lead paint and other quality problems with components that are sourced from Chinese supply chains. It is difficult to overestimate the importance that this issue has had for large U.S. companies and their supply chain professionals. However, is this a research topic? Could it be used as the basis for high-quality, rigorous research? I strongly believe that it can. The key is to think about how to frame this topic in a way that makes it both amenable to high-quality academic research and conducive to findings that are potentially important and relevant to supply chain professionals. What are some interesting research questions related to the recent quality problems in Chinese supply chains? More importantly to academic researchers, is there a strong theoretical foundation that would lead to good research questions in this area? Here are some examples that come to mind.

First, how pervasive is this problem? Although the public seems to be suggesting that all China-made components should be avoided, we know that it is difficult to find a supply chain that does not pass through China at some point. Are particular industries more affected by this problem than others? Are the problems from particular regions in China more than others? What about ownership of the plants supplying the bad parts — do they tend to be multinationals, joint ventures, privately owned, state owned, communally owned, etc.? Because China is an emerging economy, there is a body of literature on emerging economies, as well as on unique ownership structures in China that is relevant to this line of research.

How is this problem related to competitive priorities? U.S. customers may convey the impression that cost is their top priority when sourcing in China. However, how well do they convey that the order qualifier is safety? When the only message sent is about cost, the Chinese suppliers may incorrectly assume that anything they can do to lower cost is desirable to their customers. This is related to the theoretical literature on competitive priorities and order winners and qualifiers in the operations management literature. How competitive priorities and order winners and qualifiers are communicated may be of critical importance, particularly in an international context.

How is this problem related to relationship commitment between the U.S. and China supply chain partners?

Is it more common among instrumental, transactional relationships, rather than those characterized by normative relationship commitment? There is a vast body of good theory on normative and instrumental relationship commitment in the marketing literature that can be drawn upon in studying this question.

How is this problem related to trust between U.S. and Chinese supply chain partners? The foundations for this line of research are in the national culture literature. Because of the high level of collectivism in Chinese national culture, there is strong trust of members of the in-group, combined with readiness to take advantage of those who are not members of the in-group. This might suggest the importance of having a go-between who is perceived as being part of the in-group of the Chinese suppliers.

How is this problem related to contractual agreements between the U.S.–China partners? Contracts are not perceived in the same way in China as they are in the United States, owing to its long history of weak government enforcement of contracts, combined with the perception that contracts exist so that foreigners can take advantage of their Chinese partners. The organizational theory literature on relational governance suggests that, in environments where the government does not take a strong lead in enforcing contracts, relationships are much more important than contracts in governing partnerships between organizations.

How is the relative balance of supply chain power between the U.S. and China partners related to this problem? The organizational behavior literature provides a very strong theoretical foundation on power and how it is perceived and used in the relationship between organizations.

In each of these cases, there is a body of strong theory to guide research. This is important to both academics and practitioners. It ensures that academic researchers are drawing upon past work that is relevant and not reinventing the wheel, as well as helping to develop research that will be rigorous and thorough. Interestingly, the literature bases that were cited above come from a variety of areas, including operations/supply chain management, marketing, strategic management, organizational theory, organizational behavior and law. By building on these foundations, we can develop the high-quality academic research papers that we value as academics, while answering questions that are of critical importance for supply chain professionals.

OTHER EXAMPLES OF SUPPLY CHAIN RESEARCH TOPICS FROM THE HEADLINES

A couple of other examples come to mind, but there are many others that could be listed. The topic of supply chain authentication and security has important implications for many industries. For example, in the pharmaceutical industry, counterfeit medications affect both corporate profitability and consumer safety. A recent news story described the case of a woman in England who unknowingly received a bottle of counterfeit pain medication when she filled her prescription. Because the pills did not alleviate her pain, she became accustomed to taking larger and larger doses. When she refilled her prescription and received the real medication, she continued taking her usual dose and quickly died of an overdose. The supplier of the medication, of course, was liable, even though it had not produced or sold the ineffective medicine. The problem was in its vulnerable supply chain, which had been compromised, leading to issues of supply chain authentication.

Supply chain authentication is a pervasive problem. For example, Roche reports receiving an average of 20 calls per day about counterfeit diabetes test strips, which each require logging and follow-through. Although there have been some recent conferences related to supply chain security, there is a dearth of academic research on the topic. In addition, much of the work on supply chain security focuses on terrorism, rather than authentication. While both are important topics, research on supply chain authentication is truly in its infancy. Finding a literature base to support this line of research will require a multidisciplinary perspective. Potential candidates include the literature bases related to sociotechnical systems theory, double-loop learning, IT security, agency theory, governance, redundancy, preventive maintenance, contracts and the chain of custody literature from both criminal justice and medicine. This is an area where a combination of empirical research and modeling could potentially be beneficial. For example, examination of modeling of self-repairing mechanisms for leaky supply chains, through approaches such as simulated annealing, may be beneficial.

Another example of a topic that arises directly from the news and is highly relevant to supply chain professionals deals with relationships between supply chain partners. It is exemplified by a recent news story about Ford and the engines that it sources from Navistar. The story discussed the power play that Ford used, in order to force Navistar to extend longer payment terms to Ford, which dragged its feet paying its bills, gradually moving way beyond the payment terms that had been jointly agreed to. Because of the amount of business that Ford provides to Navistar, its assumption was that Navistar would be forced to comply with Ford's demands. In an interesting turn of events, however, Navistar simply shut down its production lines and stopped supplying Ford the engines that it needed for its trucks. Because of the limited inventory used in Ford's JIT process, this had very serious implications for Ford, and the dispute eventually had to go to arbitration. There is a wealth of theoretical literature, which could be used to model

and study this sort of situation, which is all too common. This includes the organizational behavior literature on power, the organization theory literature on the resource dependence model and the resource-based view of the firm, and the marketing literature on relationship commitment.

There are numerous other examples that could be listed. The point is that there are many, many research questions that are very interesting and important from both an academic and supply chain professional perspective. However, it is incumbent upon academic researchers to frame supply chain professionals' issues in a theoretically sound manner and view the issues in a way that lends itself to being studied using rigorous academic research. This requires being eclectic and knowledgeable about the literature in a variety of areas. It may also be very useful to work with partners from academic backgrounds other than operations and supply chain management.

LOST IN TRANSLATION?

In addition to having a strong theoretical foundation, high-quality research must use the most rigorous research methods possible. I get weary of hearing about how research that is relevant to practitioners must use weak methods — this is simply not true. Research that is not methodologically sound is a waste of time, for both academic researchers and practitioners. Of course, we do not expect practitioners to understand the research methods that we employ, but that is a different story. Our responsibility and challenge is to study relevant problems using the most rigorous methodologies, but then be able to translate our research into terms that are meaningful and useful to supply chain professionals. How can we do this?

First, we need to find ways of disseminating our research so that it reaches supply chain professionals, who are not going to spend a whole lot of time reading geeky academic journals. Does that mean that we should not publish in geeky academic journals? Of course not! These are the sorts of publications that are valued in the academic incentive system, and, more importantly, the rigorous review process helps us to validate the research. However, we can take the same research and also disseminate its findings through additional means, in order to ensure that it reaches every audience for which it is relevant. This may mean speaking with the external relations staff at our universities, who can help to develop press releases about our findings. Speaking to industry groups, such as ISM, CSCMP and APICS, is another important way of disseminating the findings of our research, as well as in our executive MBA and executive education classes. Some of my most interesting experiences have been with providing feedback and company presentations to the organizations that provided data for our research, which also helped us to validate our data collection methodologies.

In order to effectively disseminate our research through such outlets, however, requires that we learn to speak another language. Most supply chain professionals are not interested in our hypotheses or statistical methods — what they are interested in is our results and our ability to be confident that the results are sound and can be trusted. We need to speak about issues and recommendations, not hypotheses and statistical analysis, and use terminology that makes sense to our audience. The bonus for us is that, if we are able to present what we have done in meaningful terms, it will likely result in an interesting and lively discussion which may generate further relevant research ideas.

From a larger perspective, there are structural changes that could stimulate more supply chain management research that is both rigorous and relevant. Research funding that requires inputs from industry would stimulate relevant research. For example, the "Transformations to Quality" initiative by the National Science Foundation in the 1990s required both matching funds from organizations and industry advisory boards for all of the research projects that it funded, leading to a set of high-quality research projects that were both rigorous and relevant. Rynes, Bartunek and Daft (2001) suggest industry sabbaticals for business school faculty as a means of keeping faculty members in touch with real world problems and priorities. Seeking doctoral students who have managerial experience before entering their doctoral program would help train a cadre of young academic researchers who have both an industry perspective and rigorous training in doing high-quality research. In addition, some academic organizations are beginning to do more to develop tracks and programs that will attract practicing managers to attend their conferences and network with academic researchers.

So what is the essence of what we must do, in order to have it all? First, we need to be knowledgeable about theory in a variety of disciplines, so that we can expand our arsenal of theoretical weapons. Second, we should stay open-minded about potential research questions, listening to our executive students, attending industry association meetings and paying attention to the news, constantly thinking about which of our theoretical weapons would provide the best way to attack the most interesting and relevant research questions. Application of a theoretical framework will naturally lead to the generation of testable hypotheses. Third, we should never compromise the rigor of the analytical methods that we apply to testing the hypotheses. Finally, we should strive to become bilingual, presenting our research in the language and conduits of both academic researchers and supply chain professionals. In doing so, we will contribute to the advancement of knowledge on both fronts.

REFERENCE

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