

Five challenges for the operations management research community

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Introduction

When Jack Meredith invited me to write this short piece on research in operations management, I took the opportunity to reflect on what I have seen in my nearly 25 years as a professor of operations management and as the co-editor of the *JOM* from 1993 to 1995. My goal in writing this is to be provocative without being offensive, and hopefully add some value to our profession. I came up with five principles that I hope will challenge the new editorial board and the community of researchers in operations management as a whole to continue to pursue excellence in our research. I hereby nail my five principles to the “door” of this journal.

Principle 1: Avoid selling snake oil

An acquaintance of mine works with a pyramid-selling organization to sell high-priced vitamins and “natural” food supplements with outrageous claims. One of the food supplements is St. John’s Wort, which is claimed to have health benefits such as relief from anxiety and mild-depression. One website listed the following internal uses for St. John’s Wort: AIDS, anxiety, cough, depression, diarrhea, dysmenorrhea, fatigue, flu, gout, grief, herpes, HIV, hydrocephalus, insomnia, irritability, jaundice, menopause, neuralgia, rheumatism, ulcers, viral infections, arthritis, backache, bruises, burns, electric shock, hemorrhoids, hysteria, nerve pain, neuralgia, paralysis, rheumatism, sciatica, sunburn, tumors, varicose veins, and wounds. However, recent research suggests that St. John’s Wort has potentially serious interactions with other drugs and is not considered effective (Health Central, 2001). Evidently, some people promoting St. John’s Wort were motivated by financial incentives, which led them to hyperbole, self-deception, and deceptive advertising. They did not examine their assumptions and were uncritical of the “facts.” Borrowing a term from the Federal Reserve Chair, they were guilty of “irrational exuberance.” While some might question the hyperbole surrounding some operations topics such as quality circles, OPT, and Advanced Planning and Scheduling systems, for the most part, the operations management research community (and *JOM* in particular) has avoided this snake-oil sales syndrome. Do we have the right precautions in place for us to avoid selling snake oil (or St. John’s Wort) in our journals? Do we have the right review process?

Principle 2: Don't miss the penicillin

In contrast to above point, some might suggest that we have been too cautious. It took medical researchers a long time to figure out that penicillin mold could fight infection. Would operations management researchers find the operations "penicillin" -- or would we reject it because it is outside of our narrow research methods and paradigms? Where are the tiny spores of ideas that need to be cultured and allowed to grow into full-fledged theories?

Principle 3: Continue to reinvent the profession

Every discipline has a life cycle. Clearly, 80% of the contributions are made in the first 20% of the life of the discipline. In the early life of the discipline, many new ideas are generated, the vocabulary is defined, and people position themselves for influence and reputation. Later in the life of the discipline, the molecules become solidified, few really new ideas are proposed, and those in power ensure that they stay in power, only to be succeeded by those in their own institutions, their graduates, and close friends. How can the operations management discipline continue to revive itself? What new problem domains and research tools should we add to our standard fare? How can we avoid the death cycle that other disciplines are now experiencing?

Principle 4: Seek to be leaders, not just reporters

When I read scientific journals in other disciplines, I am fascinated to learn about academic researchers making fundamental contributions to their fields in medicine, engineering, and basic science. However, when I consider the seminal developments in the operations management field in the last 25 years, I cannot think of a single one that came out of academia. Academics have often accelerated the development of these concepts by serving as "reporters" and by fine-tuning some of the concepts. However, it seems clear to me that few, if any, operations management academics have had the vision and leadership skills to identify and develop fundamentally new concepts that have added significant value to our society. Why aren't we having more of an impact? What is missing from our approach to research? Do we need a larger scale of operation (million dollar projects), new partnerships (with consulting firms, industrial firms, or government), or a simply new self-image (leaders rather than reporters)?

Principle 5: Be careful to have your ladder against the right wall

An old story talks about a man who worked hard to climb a ladder to get to the top only to find that his ladder was up against the wrong wall and that all of his work was wasted. Similarly, operations management researchers need to be sure that we are addressing the "right" problems – problems that will make a difference in the longer run. After hundreds of articles were published on lotsizing heuristics for time-varying demand, Professor Jim Evans at Cincinnati noted that the problem can be formulated as min-cost network problem and solved very quickly, even for very large problems. All of the hype about the Wagner-Whitin algorithm requiring too much computation time was just flat wrong. But even more importantly, we were studying lotsizing algorithms when the real issues of the day were much more fundamental. Firms in North America were struggling with major strategic operations issues such overseas competition, appropriate use of automation, MRP system implementation, and quality. Firms around the

world were struggling with fundamental quality, process design, and systems issues. We were rearranging the deck chairs while the ship was sinking. What are the “ship sinking” issues of the 21-st century? Are the operations management researchers today still asleep at the helm?

Conclusions

In conclusion, if we are going to have real impact on the world, we need to (1) use strong research methodologies (to avoid the snake oil), (2) nurture new ideas (to avoid missing the penicillin), (3) continue to rejuvenate the profession with expansive thinking about problem domains and research methodologies (to continue to reinvent the profession), (4) be innovators and leaders (to avoid just being reporters), and (5) address relevant and important problems (to make sure we have our ladder on the right wall). The *JOM* should continue to demand that the research published in its hallowed pages pass the dual litmus tests of relevance and rigor. We need problem-driven research relevant to managers -- and we need appropriate research methodologies so that we can have confidence that what we say is true and authoritative.

Jack Meredith has done an outstanding job of maintaining these five principles for the *JOM* and for the operations management research community -- and we owe him a loud “thank you Jack” for his service over the last six years. I am very confident that Rob Handfield and his new editorial board will continue to do the same in the future.

References

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