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OM Forum

Three Simple Approaches for Young Scholars to Identify Relevant and Novel Research Topics in Operations Management

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Abstract. The revolution in information technology has provided the research community in operations management (OM) with new areas to explore and many new avenues to develop. In recognition of this, many editors of OM journals strive to publish new ideas. These two forces generate strong motivation, but many young OM scholars find it difficult to find new OM research ideas. To address this challenge, I describe three simple thought processes (or approaches) that I have learned and used to identify new research topics over the last 35 years. These approaches are as follows: (1) observe and learn to develop "problem-based" research, (2) ask "whys" to develop "phenomenon-based" research, and (3) sketch graphs to develop "insight-based" research. Clearly, these simple approaches are neither complete nor optimal; however, I share these personal thought processes with the hope of contributing to discussions as to how the next generation of OM researchers can build upon and expand the remarkable impact that our field has had and will continue to have.

Keywords: research idea generation • creativity • problem identification

A Quest for Finding New Research Ideas in Operations Management

With the advent of information technology that enabled many innovative business models, there is a golden opportunity for the operations management (OM) research community to explore many novel and relevant research ideas. OM research has evolved over the last 30 years. In the 1980s, OM researchers tackled many important quality management, job scheduling, inventory control, and production planning problems with great success. As manufacturing firms changed from "vertically integrated" to "globally decentralized" in the 1990s, many OM researchers examined various planning and contracting issues arising from decentralized supply chains (Tayur et al. 1998, Simchi-Levi et al. 2004, Lariviere 2016). Since 2000, many OM researchers investigated various OM issues arising from service operations (Maglio et al. 2010, Karmarkar 2015), healthcare operations (Brandeau et al. 2004, Green 2012), revenue management (Gallego and van Ryzin 1994, Bitran and Caldentey 2003), innovations (Girotra and Netessine 2014), marketing and operations interfaces (Ho and Tang 2004), finance and operations interfaces (Birge et al. 2007, Babich and Kouvelis 2015), and human resource management and operations interfaces (Boudreau et al. 2003). More recently, there are new research trends in OM research exploring issues arising from emerging markets (Iyer et al. 2013), humanitarian operations (Starr and van Wassenhove 2014), supply chain risk management (Sodhi et al. 2012), sustainability (Tang and Zhou 2012, Drake and Spinler 2013, Girotra and Netessine 2013), and the sharing economy (Cachon et al. 2017, Taylor 2016).

In recognition of many new areas, many editors of OM journals aspire to publish more novel and relevant OM research papers (Gallien et al. 2016). At the same time, based on my interactions with many Ph.D. students and young scholars in various countries over the last 10 years, I have learned from them that they are inspired to work on important, timely, and relevant OM research problems; they all want to work on "good" research topics that address issues that are of practical relevance but not adequately dealt with in the literature (Porteus 2002). Also, they can recognize a good research topic when they "see" one, but many young scholars are not sure about "how" to find good OM research topics. To address this challenge, Jiang (2017) surveyed 80 OM researchers and summarized their common approaches for finding new research ideas that include the following: (a) working closely with doctoral students, (b) reading the literature, (c) networking, (d) contact with the real world, and (e) curiosity about things.

Even with these common approaches suggested by 80 OM scholars, many Ph.D. students and young scholars still find it difficult to identify "good" research topics. It is certainly an important first step to get "exposed" to new ideas by adopting those common approaches suggested by those 80 scholars, but many young scholars still wonder "what the next step ought to be." Specifically, they are not sure about how to "formulate" conceptual understanding of these new ideas and how to "identify" a meaningful research topic in OM. In many instances, Ph.D. students and young scholars think that they are the only ones who face this difficulty. They may be surprised to learn that their challenges are not new; I faced the same challenge when I was a Ph.D. student in the early 1980s, and I still find it difficult to find new research ideas.

As I discussed with my colleagues and searched through the research literature, it appears the question of "how to identify new research idea in OM" remains unanswered. I certainly do not have a definitive answer, but I am eager to help. At the same time, I was unable to find a well-documented process for identifying new research ideas in OM. This observation motivated me to write this "thought" paper with the hope to stimulate discussions and to facilitate mutual learning among OM researchers.

According to psychologists and historians (e.g., Koestler 1964, Wicker 1985, Niemeier 2017, Berkhofer 2017), they suggest the author of a thought paper should (1) "describe his or her personal thoughts" and (2) "assume the role of the teacher." Based on these guidelines and my fear of misinterpreting other OM researchers' thought process, I shall describe my own thought process for identifying new research ideas with concrete examples. As a disclaimer, I wish to declare that these concrete examples (i.e., my projects and research papers) are intended to "illustrate" my thought process, and they are not necessarily unique or novel. Also, I wish to clarify that this thought paper does not contain a review of new OM research articles. Instead, it is intended to share three simple approaches that I have learned and used over the last 35 years to identify new research ideas.

In this paper, I shall use my personal experience to describe three simple approaches: (1) observe and learn to develop "problem-based" research, (2) ask many "whys" to develop "phenomenon-based" research, and (3) sketch graphs to develop "insight-based" research.[1] Obviously, these approaches are neither complete nor optimal; however, I share these approaches with the hope to stimulate discussions among OM researchers to develop a better process. Through mutual learning, our OM community will continue to thrive, and the impact of OM research will continue to be far-reaching (Van Mieghem 2012, Tang 2016, Roth et al. 2016).

Approach 1: Observe and Learn to Develop Problem-Based Research

One cannot identify relevant OM research topics without knowing how particular business operations actually work. Therefore, it is important for OM researchers to observe and learn about real operations. We can certainly learn from reading academic journal articles and popular press (e.g., *Business Week*, the *Wall Street Journal*, and the *Economist*), but I do not think it is sufficient. To supplement these readings, I strongly believe that Ph.D. students and young OM scholars should take up internships or summer jobs in industry to observe and learn about real-world problems. This is because companies tend to focus on urgent issues that hint at basic problems; however, with an up-close examination, we may be able to identify fundamental research questions. I am delighted to see more young scholars are taking up internships at companies such as Airbnb, Amazon, Google, IBM, Microsoft, Uber, etc., so that they can identify new research topics.

Personal Experience

As a typical Ph.D. student without any work experience, I was unable to find a new and relevant research topic in the early 1980s. Encouraged and supported by Professor Eric Denardo of Yale University (my thesis advisor), I was given an opportunity to work as a summer intern at the IBM T. J. Watson Research Center in 1982. By observing how flexible manufacturing systems (FMS) actually worked at different IBM sites and along with my understanding of various FMS research problems introduced by Kathy Stecke (Stecke 1983), I was able to engage meaningful discussions with various IBM plant managers about certain unique production planning issues associated with FMS. As I reviewed my meeting notes based on these discussions, I was able to identify a "common problem" arising from FMS:[2] how to schedule different jobs and the requisite FMS component feeds simultaneously without incurring unnecessary setups.

After several failed attempts to solve different mixed integer programs optimally by using the IBM PIPEX code (that was developed by Ellis Johnson, Harlan Crowder, and Manfred Padberg), various IBM researchers agreed that this problem was intriguing and technically challenging. With the help of my advisor and the support of Dr. Chacko Abraham (my manager at IBM), I was able to solve various subproblems analytically. By combining the optimal solutions of these subproblems, I managed to develop a near-optimal heuristic solution for IBM Vimercate, Italy (Tang and Denardo 1988).

After receiving my Ph.D. in 1985, I continued to work with IBM on various research problems. For example, I spent two summers in 1991 and 1992 working at IBM San Jose that produced "hard disk assembly" for IBM

mainframe computers. From observing the operations on site and from reading integrated circuit (IC) wafer fabrication magazines, I learned that the underlying manufacturing process at IBM San Jose resembled that of IC wafer fabrication: the process entails multiple stages, each stage involves a sequence of photolithographic and chemical etching processes performed on a serial line, and all stages are performed on the same serial line. However, due to the inherent yield uncertainty in each step, the actual final output was highly uncertain. To meet customer demand, I observed lots of work in process (WIP) inventory accumulated throughout the entire plant. While the plant manager was able to meet customer demand, he was very concerned that most of the WIP inventory would become obsolete as IBM introduced new models.

Upon discussing various production planning issues with various operators and the plant manager and reviewing the research literature, I learned that the underlying process is known as "re-entrant flow shop" that is commonly observed in IC wafer fabrication process. In the early 1990s, there were research articles and commercial software that focused on solving various job scheduling problems; however, there was no commercial software and research articles that dealt with production planning in re-entrant flow shops with uncertain yields. This discovery enabled me to identify another "common problem" associated with re-entrant flow shops: how to develop an effective production plan at different stages (with uncertain yield) to meet customer demand with low WIP inventory. With this exciting discovery, I asked Professor Denardo to help me work on this relevant and seemingly novel research topic. We managed to develop a robust production planning process that incorporates the notion of self-adapting (Denardo and Tang 1997). By applying the structure of this robust production plan, I worked with my former Ph.D. student Lieven Demeester (currently a professor at the Singapore Management University) to implement a Just-In-Time system at IBM San Jose in 1993 that helped IBM to meet customer demand with 50% reduction in inventory (Demeester and Tang 1996).

Afterthought

Besides my own personal experience, there are many instances in which we can observe and learn from real practice to develop problem-based research. For example, through direct observations, Fisher et al. (1994) developed the well-known case study of how their "accurate response" idea could help Sport Obermeyer to make supply meet demand. This case study, in turn, motivated Fisher and Raman (1996) to develop a novel analytical model to quantify the benefits of accurate response. In a similar vein, Felipe Caro of UCLA and Jérémie Gallien of London Business School observed and learned about the fast-fashion process developed by Zara, and they developed various relevant and novel research ideas that helped Zara to perform even better by increasing its sales by 3%–4% (Caro and Gallien 2007, Caro and Gallien 2010, Caro et al. 2010). More recently, Ferreira et al. (2016) developed a novel algorithm that can enable an online retailer (such as Ruelala.com) to learn about customer demand on the fly and adjust the selling price dynamically so that the retailer can maximize its revenue by selling a limited amount of inventory over a limited time window without knowing the customer demand in advance. This "learn and earn" algorithm enables the retailer to strike a balance between exploration and exploitation.

The above examples are promising, but these examples can be viewed as consulting projects that may not be publishable in most academic journals. This is a legitimate concern, but it is helpful to mention that, in the mission statement of *Interfaces* as well as the announcements of the *Biennial POMS Applied Research Challenge* and the *Biennial M&SOM Practice-based Research Competition*, novel consulting projects in OM that utilized rigorous research methods can be published in OM journals such as *Interfaces* as well as special sections of *POM* and *M&SOM*. At the same time, it is useful to be cognizant of the difference between a traditional consulting report and a problem-driven research article. In general, a traditional OM consulting project tends to focus on prescribing a specific solution to a specific problem by using off-the-shelf techniques. However, a problem-driven research project tends to address a new class of problems arising from different companies (or in different settings) and to prescribe new solution approaches by exploring the underlying structure of this class of problems. Therefore, it is important for us to examine whether the problem we discover from a company is novel and whether the problem is relevant to other companies (or in other settings). Young scholars can do so by discussing with practitioners and by reading industry-based journals and by discussing with their trusted advisors or senior colleagues.

It is wonderful if we can discover novel and relevant problem-based research projects, but there are other situations when the company we interact with does not have an immediate problem. When facing these situations, how can we develop relevant and novel research ideas?

Approach 2: Ask Many "Whys" to Develop Phenomenon-Based Research

Managers who are not facing an immediate problem are less willing to provide information or data with academics. When this happens, impatient researchers (including myself) tend to define a problem based

on half-baked ideas without a deep understanding of underlying issue. Also, due to our training in research methods (optimization, simulation, statistical analysis, game theory, etc.), we rush to "solve" a problem (that may not need solving) without spending sufficient time to "identify" the "real problem."

When the company I interact with does not have a real problem that needs to be solved immediately, I have learned to first check to see if the company has changed the way they operate recently or has planned to change the way they operate in the future. Knowing the market is dynamic and competitive, companies often change the way they operate. In general, I try to learn from managers regarding how the company operated in the past (e.g., how the company was organized in the past and why), and then ask them many "whys" about the way they plan to organize/operate in the present (or in the future).

More importantly, we should ask the company why they changed or why they plan to change. Was it due to changes in regulations/government policies (e.g., recycling legislation (Atasu and Subramanian 2012)), changes in competition landscape/consumer preferences (e.g., speed and quality competition (Li et al. 2016)), technological disruptions (e.g., innovative business models (Girotra and Netessine 2014)), or man-made/natural disruptions (e.g., disaster relief operations (Starr and van Wassenhove 2014)), and product adulteration (Babich and Tang 2012)? Without asking enough "whys," we undermine the possibility of discovering new research ideas. Many innovative ideas originated from why, not how. Also, by considering the past practice as a "benchmark," it may be possible to generate new research ideas by examining whether and when the present (or future) practice dominates the past practice. We can examine these research ideas by using different research methods including analytical, empirical, and behavioral experiments. In some cases, it may be even possible for us to develop an even better scheme so that we can share our finding with the company. Even when there is no immediate problem, these managers may not mind listening to our ideas.

Personal Experience

In 2007, I was engaged by GKN—a British multinational automotive and aerospace components company—to develop an executive training program about global supply chain operations. With over $8 billion in revenue at that time, GKN did not have an immediate problem that needed to be solved. However, from reading the annual report and the public press, I learned that GKN was a key supplier to the Boeing 787 Dreamliner program, providing a range of structural titanium wing fittings, horizontal stabilizer spars, composite materials, etc. Motivated by this observation, I decided to develop new teaching materials based on the supplier relationship between Boeing and GKN for this training program.

To develop this teaching case, I interviewed various managers and executives at GKN to learn more about the supplier relationship between Boeing and GKN. I discovered a new change: Boeing offered a new supply contract to its tier-1 suppliers for the 787 development program that was drastically different from the old contracts for the 747, 767, and 777 programs. Specifically, under the old contract, a supplier would get paid immediately after it completes its own development task for a specific part. However, under the new contract for the 787 program (also known as the "risk sharing" contract); each supplier would get paid only after all suppliers completed the requisite tasks for the development of the entire aircraft. To understand the dynamics between Boeing and GKN, I interviewed various Boeing managers to learn why they decided to offer this new (i.e., the risk sharing) contract, and asked GKN why they agreed to accept this new contract. Also, I asked Boeing and GKN for their opinions about whether and when the risk sharing contract is beneficial to them and why.

To summarize my findings based on various interviews and public information, I invited my MBA student Joshua Zimmerman (currently at Amgen) to work with me to develop a teaching case by highlighting different supply chain risks arising from the Boeing 787 development program (Tang and Zimmerman 2009). At the same time, I also searched through research literature and discovered that, besides the classic program evaluation review technique (PERT); project management contracts with uncertain project completion time are not well understood. This discovery motivated me to enlist my UCLA colleagues Kevin McCardle, Steven Lippman, and our Ph.D. student Dharma Kwon (currently a professor at the University of Illinois) to work with me to examine whether and when the risk sharing contract is beneficial to Boeing and/or GKN. By incorporating the issues of self-interests, moral hazards, and progress information sharing, we managed to develop an economic model to examine the conditions under which the risk sharing contract is beneficial to each party (Kwon et al. 2010).

Afterthought

With an inquisitive mind, a new phenomenon (or a new practice) can be the trigger to motivate us to ask many "whys": Why did it happen? Why did the company operate this way? Why change? There are many instances that one can find new research ideas by asking why. Consider the bullwhip effect. This phenomenon has been observed by many companies for years, but few asked "why" it occurs in so many supply chains. This interesting phenomenon motivated Hau Lee of Stanford University and his colleagues to

develop a collection of descriptive models and show analytically that the bullwhip effect would still occur even when each supply chain partner acts rationally (Lee et al. 1997a). Based on the insight generated from their descriptive research, Lee et al. (1997b) prescribe innovative solutions to mitigate the bullwhip effect. This novel descriptive/prescriptive research began with the "why," and it has sparked thousands of OM researchers to examine different aspects of the bullwhip effect using analytical, empirical, and experimental research methods. Throughout history, many novel research ideas began with "why," and OM research is no exception.

Asking "why" is a practical approach, but it requires the discovery of a new phenomenon (e.g., a new practice, a new business model, etc.) for us to ask those "why" questions. What can we do if the company we interact with has not changed for some time?[3] When this happens, I have learned to use the next approach.

Approach 3: Sketch Graphs to Develop Insight-Based Research

When a company does not have an immediate problem and has not changed for some time, one may think that there is nothing new to explore with this company. However, I think there is still a chance to "salvage" our effort if we examine the operations of this company in a broader context. For instance, we can develop new research ideas by examining the operations strategy adopted by this company to compete. Over the years, I have learned to sketch graphs and use these graphs to generate new research ideas. Indeed, psychologists believe that one can generate creative ideas by applying metaphors through sketching graphs (e.g., Wicker 1985). In our context, the purpose of sketching graphs is to generate "insights" that may enable us to develop new research questions by examining an operations strategy graphically.

Personal Experience

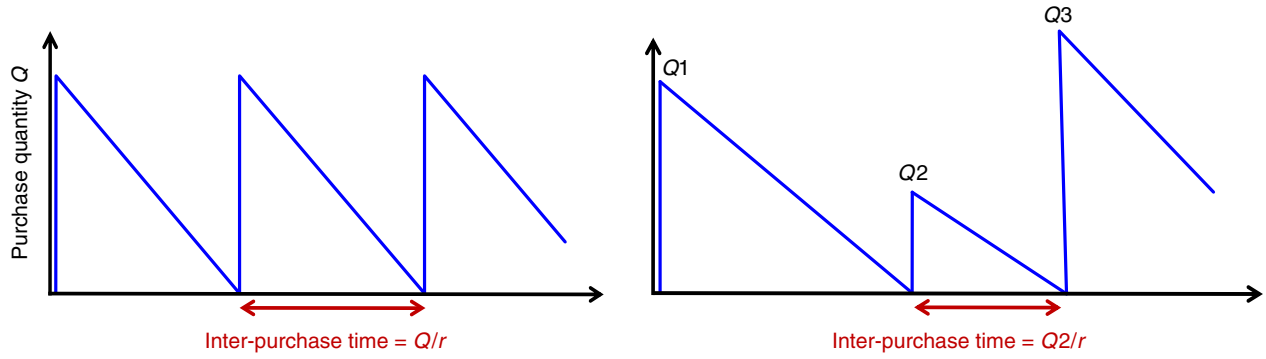
In the late 1990s, I was curious about the phenomenal success of Walmart and wanted to learn more about its retail operations. Unfortunately, through my interactions with various managers at Walmart, I learned that they were focusing on global expansion and did not have any operational problems for me to examine. Back to the drawing board, I thought about Walmart's EDLP (everyday low price) strategy as opposed to other retailers' HILO (i.e., promotional pricing) strategy. Without a clear problem definition in mind, I decided to discuss the impact of price formats (EDLP versus HILO) on shoppers' purchasing behavior with my colleagues David Bell (currently at the Wharton School) and Teck Ho (currently at the National University of Singapore).

To facilitate our discussion, we sketched different graphs to establish the relationship between a store's pricing strategy and a shopper's purchase decision during a shopping trip. After several attempts, we settled on the graphs (Figure 1) that capture the relationship between a store's EDLP (HILO) pricing strategy and a shopper's purchasing behavior. First, the figure on the left captures the rational shopping behavior under the EDLP price format. Because the selling price was constant under EDLP, a rational shopper with constant consumption rate will purchase the same quantity during each shopping trip; and the inter-purchase time (i.e., time until the next shopping trip) is constant. Second, the figure on the right represents the dynamic purchasing behavior of a rational shopper who shops at a HILO store. Because the selling price at the store is random (from the shopper's perspective) under the HILO price format, a rational shopper will purchase less (more) when she observes a high (low) price during a shopping trip.

Consequently, the inter-purchase time (i.e., time until the next shopping trip) will also be random.

We obtained the following insight from these two figures: a rational shopper's purchase pattern at an

Figure 1. (Color online) Sketch Graphs to Develop Insight-based Research



EDLP (a HILO) store is constant (random). This insight motivated us to develop new research questions arising from a situation when an EDLP store and a HILO store engage in price competition. These questions are the following: (1) Will a rational shopper purchase less and shop more frequently (in expectation) at the HILO store in equilibrium? (2) Will the HILO store charge a higher price (in expectation) in equilibrium?

To examine these research questions, we analyzed a generalized version of the economic order quantity (EOQ) model with random pricing. By examining the equilibrium outcomes, we established two analytical results: (1) a rational shopper will purchase less and shop more frequently at a HILO store in equilibrium; (2) the average unit selling price at a HILO store is higher than that of an EDLP store in equilibrium. Out of curiosity, we treated these analytical results as hypotheses for further validation. To test these hypotheses, we collected actual shopping data associated with 513 households from 1 HILO store and 2 EDLP stores located in Chicago over a 2-year period. Overall, we found empirical evidence to support our hypotheses (Ho et al. 1998). We also summarized our findings regarding various managerial implications about store pricing formats (Tang et al. 2001). In addition to the learning experience I shared with David Bell and Teck Ho, I find this research project satisfying because it allowed us to use both mathematical and statistical analyses to gain a better understanding about rational shoppers' purchasing behavior under different price formats.

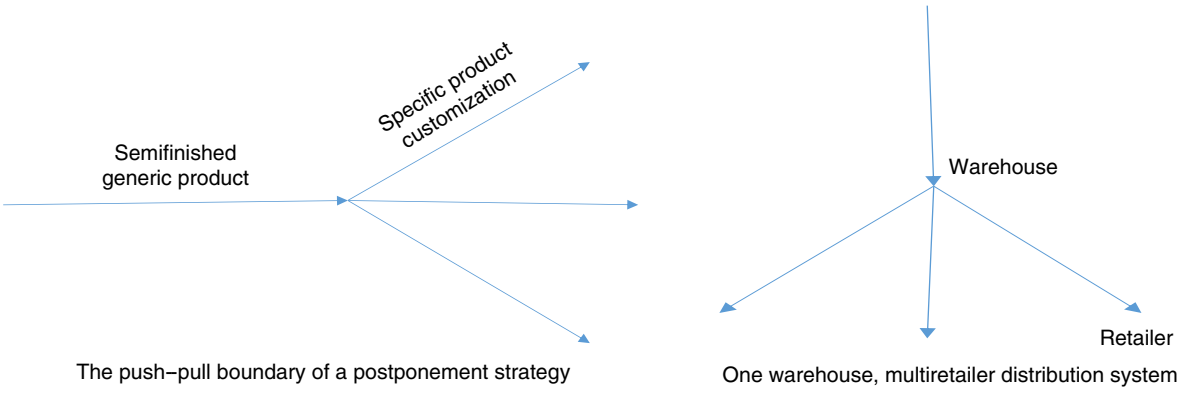
Afterthought

To illustrate another research idea that can be generated through sketching graphs, let us consider the "postponement" (or delayed product differentiation) strategy that was intended to enable a manufacturer to manage product variety without holding too much inventory (Lee 1996). To implement a postponement strategy, the firm will first produce a "generic" semifinished product and store it as work-in-process inventory by using a "push" strategy. When the demand is realized (or when the demand forecast becomes more accurate), the firm can use a "pull" strategy to customize this generic semifinished inventory by adding product-specific components. I was fortunate to work with Hau Lee on various postponement projects at Hewlett Packard (HP) under the supervision of Dr. Corey Billington (our manager at HP) in the late 1990s. Specifically, we were asked to evaluate the total landed cost and lead time implications of different postponement design strategies for different products (e.g., DeskJet printers, LaserJet printers, Workstations, etc.) at HP.

To "visualize" the postponement strategy, we sketched a graph (Figure 2, left) to capture the "push–pull" boundary—the stage at which the semi-generic product is customized into specific end-products. Due to the risk pooling, it is clear that a firm can reduce inventory by postponing the push–pull boundary to a later stage. However, we also realized that there must be some design and operational costs that a firm may incur by postponing the push–pull boundary. These observations generate a key insight regarding the trade-off between early postponement and late postponement. This insight motivated us to develop two new research questions: (1) How would different product design concepts (standardization, modular designs, process restructuring (i.e., change the product design so that certain product-specific components can be inserted later) affect the optimal location of the push–pull boundary? (2) How would different cost factors (e.g., labor cost, inventory cost, and shipping cost) and tax factors (e.g., import duties of the generic semifinished product and/or different product-specific components) affect the optimal location of the push–pull boundary?

To examine these new research questions, we were able to establish the following metaphor by rotating the figure on the left. Suppose we interpret the push–pull boundary as a warehouse that holds in-transit inventory and interpret each end-product as a retailer that faces uncertain demand. Then, by rotating the figure

Figure 2. (Color online) Sketch Graphs to Develop Insight-based Research



on the left clockwise by 90 degrees, the resulting figure on the right can be interpreted as a one-warehouse, multiretailer system. This realization reminded us of the classic inventory model developed by Gary Eppen and Linus Schrage (Eppen and Schrage 1981) that deals with one-warehouse, multiretailer with lead times and random demand. Specifically, the inventory model developed by Eppen and Schrage (1981) assumed that the lead time at each stage is fixed. However, in our context, the lead time at each stage is a decision variable that is dependent on the location of the "push–pull" boundary associated with a particular postponement strategy. By using this metaphor, we managed to apply and adapt Eppen and Schrage's analysis to evaluate different postponement strategies in the context of product/process redesign approaches (standardization, modular design, and process restructuring) pursued by different companies (Lee and Tang 1997).

Concluding Remarks

I have presented three approaches for young scholars to identify problem-based, phenomenon-based, or insight-based research ideas in OM. However, these three approaches are different in two aspects: (a) they generate research with different levels of practical relevance, and (b) they require different types of efforts.[4] First, the problem-based approach is based on real-world problems arising from companies. Therefore, frequent field trips or internships are usually required to facilitate direct and repeated interactions with companies to carry out problem-based research. Second, the phenomenon-based approach is less problem-specific. This approach certainly requires an in-depth understanding of the state of practice across different companies; however, such knowledge can be learned from managers or a third party with first-hand experience. Third, the insight-based approach is more conceptual. This approach requires less direct interactions with real companies. However, this approach requires a broad understanding of different anecdotal events and different models and solution techniques through news articles, research literature, and discussions with practitioners. Through this broad-based knowledge, we are better equipped to generate creative ideas by applying different metaphors through sketching graphs.

Because different approaches are designed for different types of relevant OM research with different requirements, different young scholars may prefer one approach over the other depending on their interests and their research environments. For example, due to various constraints (e.g., time, resources, accessibility), a young scholar may not have the opportunity to establish direct and repeated interactions with companies to identify a research problem. Thus, the problem-based approach may not always be practical. However, many companies (e.g., Amazon, Airbnb, Facebook, Google, Uber, etc.) have summer internship programs for Ph.D. students in OM. At the same time, various societies such INFORMS, MSOM, and POMS have conferences and forums for young scholars to interact or network with practitioners. Therefore, one may expect the problem-based approach to become more applicable for young scholars. Even in the event when the problem-based approach is not suitable, young scholars can certainly explore the phenomenon-based or the insight-based approach to generate more relevant and novel research ideas in OM that can benefit our research community as well as the practice community.

Besides the three approaches described in this paper, there are certainly many other approaches that others can share. I strongly believe that we are entering a golden era for OM scholars to explore different emerging "theme-based" research (e.g., healthcare operations, supply chain risk management, supply chain financing operations, sharing-economy operations, sustainability, humanitarian relief operations, etc.). Also, Netessine (2014) argued that there are plenty of research ideas that OM researchers can investigate by exploring various innovative business models as described in Girotra and Netessine (2014). In addition, Hau Lee and I proposed a new OM research direction in the area of "socially and environmentally" responsible value chains (Lee and Tang 2017).

I fully understand why Ph.D. students and young scholars find it difficult to identify relevant and novel research ideas. However, I do believe there are abundant research opportunities for OM researchers to explore. Even when we discover new research problems that we do not have the requisite technical skills to solve, we can always engage other scholars to work with us so that we can explore new frontiers. Therefore, the key is to identify new research topics. All we need to do is to observe and learn, ask many whys, and sketch graphs.

Acknowledgments

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Endnotes

[1] Besides these three approaches, Simchi-Levi (2014) proposed "data-driven" research by exploring real data.

[2] I would like to emphasize the importance of identifying a "common problem" as a research topic. This is because the value of the knowledge created from a study of a highly specialized problem has limited value to the OM research community as a whole. Therefore, to identify a relevant research topic, it is important to observe, learn, and discover a common problem.

[3] We can certainly ask the company the following: "Why don't you change?" However, based on my experience, this question can be a sensitive one due to the political situation within the company. So, this question is probably the last resort.

[4] The author is grateful to the guest editor for the suggestion to discuss the differences and the required efforts associated with these three approaches.

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