How to Do Research:

Advice from stellar scholars in the POM field

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1. Introduction

What does it take to become a successful scholar in the POM discipline? This question is perhaps the most crucial one confronting faculty members early in their academic careers. As an emerging scholar, I often feel like a beginner in a game of chess: I already know the rules that define some moves as legal and some as illegal (confound variables, for example), but nothing in these rules provides a strategy for winning the game – to become a successful researcher. It is one thing to get doctoral training in research but quite another to know how to apply this knowledge. I have discussed this issue with many junior scholars and found that they share the same feeling as me.

To identify whether commonly-agreed upon principles actually exist on how to do research, I surveyed more than 80 top POM researchers. By summarizing and categorizing their opinions, this paper tries to help junior scholars in the POM fields gain a perspective on doing research.

2. Collecting stellar POM scholars' opinions on research

To identify leading scholars in POM, the h-index for POM-related professors in 225 American business schools in BusinessWeek's list was calculated. This project identified 83 professors whose h-indexes are equal or greater than 10 (see Appendix. The detailed procedure can be required from me).

To make these stellar scholars' personal opinions converge on some commonly agreed upon principles, I used the Delphi method. Focusing on the junior scholars' most frequently asked questions, the first phase involved sending out the open-end questions listed below to the individuals identified in the preceding step:

1. Where do ideas for new research come from?
2. What is the efficient/effective way to build up the scholarship (refer to accumulate knowledge of literature)?
3. Facing the complex real world, what are the key points that your model setting or research design can reflect the reality?
4. What are your rules of collaboration?
5. What are your rules of writing a publishable paper?
6. What is your philosophy of research?

In the initial round of the Delphi, fifty-eight usable responses were received. For each survey question, I summarized respondent's opinions into several most-frequently-mentioned-opinions (MFMOs). Then I resent these six questions with their MFMOs to these stellar scholars, asking them to rank these summarized opinions based on their importance. Using this process, the opinions tended to converge toward several areas of common agreement.

3. Results

After the first round survey, I categorized the responses in the six open-end questions through a frequency analysis, i.e., for each opinion, I counted how many times it was mentioned by respondents. The second and third columns of Table 1 report these MFMOs and their respective frequencies.

Insert Table 1 about here

For Q1 (Where do ideas for new research come from?), the following opinions are the most frequently mentioned:

a. **Teaching:** students' questions; working closely with doctoral students.
b. **Reading the literature:** unsolved theoretical questions that we stumble upon; what can you do to improve upon what has already been done; whether your approach would add substantial value over the approach taken (or vice versa).
c. **Networking:** conference presentations; serving on a variety of panels; talking, discussing, or working with colleagues.
d. **Contact with the real world:** field studies/site visit, working/talking with industrial people, reading industry or practitioner-oriented publications, consulting.
e. **Curiosity about things:** interesting applied problems can come from anywhere, such as intuition, analogies from other fields, hypotheses formed from real-life observations and so on. There was no "the best source", but rather an intersection of sources.

For Q2 (What is the efficient/effective way to build up the scholarship?), most stellar scholars believed that there is far too much research written to master all of it, or even a small percentage. Their opinions varied and sometimes even contradicted each other on how to establish necessary knowledge of the literature.

a. **Snapshot:** It is important not to repeat the past but also not to be led too much by it. So, try to develop something on your own and then look at the literature. You only search for literature that is related to your current projects. With the huge advantages of online search, it is relatively quick to compile citations on a well-defined topic.

b. **Whole picture:** Everyone needs to struggle through the learning curve by reading top-tier journals in the area regularly, attending conferences, and following new research trends and directions. It is better to stay focused in your research rather than being opportunistic and moving from topic to topic. Since you read the literature as it happening, you would develop a strong sense of the historical development of your subject.

For Q3 (Facing the complex real world, what are the key points that your model setting or research design can reflect the reality?), stellar scholars provided the following opinions about modeling:

a. **It is an art:** A model need not (indeed cannot) exactly match reality. In POM we tradeoff robustness with accuracy. The key point is that a model must improve people's understanding of a real situation. Stay flexible about everything and experiment, and don't get hung up on any one set of assumptions.

b. **It is a trial and error process:** Facing a real-world problem, the effective method of reaching a correct solution or satisfactory result is to try out various means or theories until error is sufficiently reduced or eliminated.

c. **Remember Occam's razor:** Always build parsimonious models, test and demonstrate a model's robustness. Try to filter out the local specifics, ask general questions, and answer them with general principles.

d. **To ensure reality, know and understand reality:** Acceptable approximations to reality for research publications must have some concrete "hooks" to what is really happening. Have a deep understanding of what you are investigating.

For Q4 (What are your rules of collaboration?), to pick up the right people to make the collaboration enjoyable, successful, and worthwhile, the stellar scholars suggested focusing on the following criteria:

a. **Personality:** pick coauthors with whom you are comfortable or like personally, with whom you have congruent working habits and writing styles.

b. **Consistency:** Because research requires stamina, you need coauthors that are smart and ready for a marathon.
c. **Expertise:** seek out collaborators who can help to forge new frontiers and methodologies; work with people who have expertise in the area with which you are struggling.
d. **Research topic:** work with people whose ideas excite you; work with people you can find with similar interests.

For Q5 (What are your rules of writing a publishable paper?), most stellar scholars warned junior ones that always write with the reviewers in mind. Reviewers are very busy, so try to make their job easier. Neglecting reviewers risks getting them angry. Once that happens, their axe comes out and giving the author a whack. Every author's goal should be to keep the reviewer's axe in its sheath. The following aspects are the most frequently mentioned by stellar scholars:

a. **Motivation:** Not every reviewer who picks up your paper will be directly interested. If you don't motivate reviewers well, they may purposefully pick holes in your paper.
b. **Contribution:** A paper is publishable if and only if it has something new to say. If your paper does not make your contribution to the literature very clear, reviewers will go through your paper with a negative attitude, rather than digging for the contributions.
c. **Expression:** The expression of an idea can be as important as the idea itself. A good idea badly expressed has no impact; an average idea superbly expressed can have great impact. And impact is the key measure of success.

For Q6 (What is your philosophy of research?), although most stellar scholars pointed out that their research philosophies may be just well suited for their own interests and talents, their philosophies reflect that their research strategies cluster in the following areas:

a. **Focus on the "big problem"** which lasts and gets cited years after it is published. You should go for the big winner and be sure that you will be proud of your article when you look at it years later.
b. **Follow your interests and work on problems you enjoy**. Doing quality research requires a passion for the topic, because it drives innovation, originality, and impact.
c. **Start with simple ideas and extend them** as far as possible to increase applicability and understanding.

d. **Doing derivative work is okay**, as long as it is not too trivial to have an impact. You may develop improved theoretical or methodological results over what other people have done (i.e., applying new methods to old problems), or work on problems that seem intriguing and interesting, and where you have a chance of applying your familiarized tools to solve them (i.e., applying old methods to new problems).

In the second round of the Delphi, the stellar scholars ranked the importance of MFMOs for each questions. I received 52 useful responses. The results are listed in the fourth and fifth columns of Table 1.

4. Discussion

After the second round, the opinions converged toward some common principles that may provide valuable insights to junior scholars.

Where do ideas for new research come from?

Marshall Fisher (Wharton) says: "As I argued in the paper (Strengthening the Empirical Base of Operations Management. *Manufacturing & Service Operations Management* 9(4) Fall 2007, pp. 368-382), I've always felt most researchers rely too much on academic papers and the business press as a source of research ideas, which tends to result in less innovative research. I've found working with a company on a real problem to be a great source of new research topics." John McClain (Cornell) believes that "practices in the real world lead the academic community. JIT is an example. Academic investigation of those practices can lead to improvements in the way they are applied (e.g. optimization), or at the very least, introducing your colleagues to a great 'new' idea." Hau Lee (Stanford) lists several common approaches to connect with the real world: "I mostly get ideas on research from real world problems – through my interactions with industry executives, in attending industry conferences, executive programs, and industry publications." Aleda Roth (Clemson) also gets many of her ideas this way: "I tend to work on problems that emerge from real problems faced by managers--either through field work or through networking with faculty and executives. I also get good ideas from teaching cases in areas in which I'm interested. Oftentimes, such cases bring out issues that have not yet been thoroughly researched. Others ideas come from operations issues raised in the press."

However, several stellar scholars warn junior researchers that they should sparingly undertake consulting. They believe networking is a more effective and thought provoking approach for junior faculty. Keith Ord (Georgetown) suggests: "… staying current with the literature and networking with other researchers in the areas are most important." Kenneth Boyer (Ohio State) also emphasizes the importance of networking: "Social capital is immensely valuable in academia. By regularly conversing with colleagues – about anything – good ideas and themes are bound to develop."

Reading literature sounds like a cliché, but many stellar scholars reveal new insights of it. Wallace Hopp (Michigan) says: "while I don't think that one usually finds highly promising research areas by reading the OM literature, I do think that important research ideas can come from reading other literatures." Richard Chase (Southern Cal.) shares his experience of reading literature with junior scholars: "My work on service design started out when in the process of revising the 2nd edition of my book with Aquilano. We realized that there was little theory on service operations and I decided to develop an operational structure for services. This resulted in the customer contact model for service encounters. My basic source for structuring service interactions was an organization theory book, 'Organizations in Action' by James D. Thompson. He proposed several propositions about organizational rationality that could be applied nicely to services… I also read Academy of Management, Journal of Marketing, and more recently various journals in behavioral decision theory."

While teaching generally requires a separate effort from a professor's research, students can sometimes inspire professors with unanticipated questions. In addition, professors have to invest a significant effort in preparing materials for their courses. This teaching activity can deepen a one's understanding of potential research topics. Many times new ideas come from working closely with doctoral students (Anna Nagurney at UMASS). Michael Crum (Iowa State) says: "I think it is very important for researchers, particularly young researchers, to create synergies among their research, teaching, and outreach. The more you can tie these together, the more time efficient and productive you can be."

Even though stellar scholars agreed that "real world", "networking", "teaching" and "reading literature" as possible sources of new ideas, they also emphasized that without a curious mind, you still cannot discover new ideas through those sources. Urban Wemmerlov (Wisconsin) says: "I do research out of curiosity. I try to learn (a) what is going on in organizations, (b) how people or organizations make certain decisions (c) why they make those decisions, and (d) how they should make those decisions for better outcomes."A successful researcher should have "a general curiosity about things combined with exposure to new situations and problems" (Garrett van Ryzin at Columbia). Paul Zipkin (Duke) says: "I picked E (curiosity), because it sort of means 'all of the above'." This may be the reason why "curiosity about things" was ranked the most important source of new ideas.

What is the efficient/effective way to build up the scholarship?

Stellar scholars acknowledged that it is very difficult to rank the importance of "Snapshot" and "Whole picture". After running a paired t-test, I found that there is no significant difference between the two schools of thought.

For choice B (whole picture), many stellar professors pointed out its benefits to junior scholars. James Orlin (MIT) believes that young researchers should "have an intimate knowledge of research that is very closely connected to the research they are doing." By doing so, they can "have a working knowledge of lots of research ideas and methodologies from a wide range of areas so that they can try out lots of different ideas on whatever problem that they are addressing…, and know where to look when they want to pursue an idea." Zhi-Long Chen (Maryland) suggests: "it is important to see the whole picture of the problem/methodology you study and even look at problems and methods in other areas. This can give you complete information as to where you stand. .. There is a time-consuming setup if you want to dive into a new area. So, do not move from topic to topic. Popular topics come and go quickly. You need to focus on a few topics that you can research for a long time."

In contrast, some stellar professors believed that becoming familiar with the literature is a second priority compared with the newness of research idea. As a result, they ranked A over B. John Birge (Chicago) says: "I often follow a process of trying to develop something on my own and then look at the literature." Wallace Hopp (Michigan) also holds the same opinion: "I strongly believe that the most important research ideas come from studying the world, not the research of others. However, after one has discovered a research opportunity by looking at the world, it is important to study the literature in order to understand what has been done and what tools might be relevant to the problem."

David Pyke (Dartmouth) describes his thoughts when he ranked choices A and B: "I tend to stay focused on an area of research or two (choice B), but I often begin a new area by finding a problem from industry or curiosity, and then searching the literature to find out if and how it has been solved (choice A). The reason I put 'B' above is because I think it is very important for young researchers to avoid jumping topics frequently." Similarly, Sridhar Tayur (Carnegie

Mellon) believes A and B are two parallel paths: “Pick a long term ‘canonical’ research direction, such as multi-echelon inventory theory; do many interesting real world contemporary projects, such as time-shared jet aircraft, dynamic in-game (video games) ads scheduling, designing rapid response supply chains.” This may be the reason why stellar scholars’ opinions evenly distribute between the two choices.

What are the key points that your model setting or research design can reflect the reality?

Several leading scholars mentioned that in trying to solve a problem, one is always tempted to use tools with which one is familiar. This is very natural. Our doctoral training for the most part deals with tools. Exercises associated with such learning are always designed to fit these tools. As a result, when junior scholars tackle a problem outside the textbook, their first instinct is to reach for these tools. Too often, they bend the problem to fit the solution. This may trigger one of the greatest dangers in OM research: Type III error – solving the wrong problem (Wallace Hopp at Michigan). Hau Lee (Stanford) urges junior scholars should always be problem rather than methodology driven. He says: “Maybe not the methodologies that we use, but the problem itself, the insights and inferences from the analysis, and the implications and lessons of the completed work, should be of interest to practitioners. With this in mind, I think our research, regardless of what methodologies were used, would make a difference and be of value to industry.” To maintain managerial relevance and academic rigorousness simultaneously, only after the former seed planting and germination can the latter grow and flower. “Understanding the reality” is not only important before modeling but also after modeling, since many researchers “live in a ‘model-oriented’ world, not checking that their work is not applicable” (Sridhar Tayur at Carnegie Mellon). Christopher Tang (UCLA) and Gary Pisano (Harvard) suggest that the model should always be tested against whether it helps people understand reality. “One way of tapping into the real world is to engage in discussions of the problem with knowledgeable practitioners. I call it an OM sense-making exercise,” says Aleda Roth (Clemson).

Compared to “understanding reality” that is treated as a strategy by stellar scholars, the other three MFMOs (a trial and error process, an art, and Occam’s razor) focus on tactical issues. However, they are also important to the development of innovative research.

Egon Balas (Carnegie Mellon) shares his experience of how to go through the trial and error process: “Facing a real-world problem, my first approach is to try to capture its essential features into a model that is manageable, even if the answer is far from an accurate representation of it. In other words, to get going, I settle for an imperfect representation. Then I set out to refine it by adding those features which can be accommodated without making the problem unmanageable."

However, "the real world is always going to be more complex than your model; that is, your model is *always* wrong" (Keith Ord at Georgetown). As a result, a model's acceptable approximations become an art that takes time to develop (Abraham Seidmann at Rochester). To ply this art, Garrett van Ryzin (Columbia) tells junior scholars: "The best advice I can give is to stay flexible about everything and experiment and do not get hung up on any one set of assumptions." Roger Schroeder (Minnesota) suggests: "You must start with a solid theory, (because) you test the theory itself, not the hypotheses. The hypotheses are only a reflection of the theory that is worth testing; the conclusions are only as good as the assumptions that are part of the theory."

Many well-recognized scholars emphasize the importance of building parsimonious models. Gregory Dobson (Rochester) urges junior scholars to "remember Occam's razor", because "too often modeling becomes an exercise in itself" (Gary Pisano at Harvard). A good model should take the problem as it presents itself and not form any pre-conceived idea on how to solve it. Confronting a problem on its terms promotes looking at it in the simplest terms since we have nothing but common sense to guide us. Many authors forget this simple fact; rather, they view the modeling process as an opportunity to bolster their own egos and impress the reader, even discomfit the reader somewhat with too much material.

What are your rules of collaboration?

A stellar scholar (who desires anonymity) provides the analogy that business schools follow the zoo model to build up the faculty group. In a zoo, you have one animal of each kind. Academic staff is chosen to teach their particular curriculums, so the thinking is that you need to have a pretty deep expertise in the subject. However, in today's cross-discipline dominated atmosphere in academic research, a researcher should follow another model in doing research: the safari park model where you have a small number of packs of similar animals. One of the reasons that research groups may be more productive than individuals is that there is a good deal of shared knowledge (Uday Karmarkar at UCLA). Lee Krajewski (Notre Dame) says: "sometimes the coauthor has expertise in a particular methodology necessary for the project or the coauthor comes from a different field of study and provides a needed perspective in a project aimed at bridging disciplines; sometimes the coauthor is a source of energy and excitement and is a great colleague to work with." These stellar scholars' criteria of collaboration almost evenly distribute among the four MFMOs.

Many stellar scholars directly mention personality as a key factor and only work with friends and people that they like (Christian Terwiesch at Wharton). Garrett van Ryzin (Columbia) says: "I like collaborating with many people and working with people I like personally and enjoying working with. If it's a fun project with people I like, the result is more productive and creative." Abraham Seidmann (Rochester) states: "I like to work with coauthors that share my personal joy of research, have bright mind, and a sense of humor."

Some top scholars believe that the research topic is the key driver of collaboration (Jeannette Song at Duke). Charles Corbett (UCLA) believes that the selection of coauthor is opportunistic: "whoever is interested in similar problems." Nicholas Hall (Ohio State) emphasizes: "Work with the best people you can find with similar interests." In addition, working with people have same research interest with you does not waste a lot of time and effort establishing background knowledge when you want to discuss issues.

Since it is hard fully to grasp cross-discipline knowledge, complementary expertise is a reasonable criterion to select coauthors. The anonymous stellar scholar makes a strong statement: "The best way is to work with people who already know how to do it!"

Abraham Seidmann (Rochester) points out: "Research requires stamina; you need coauthors who are ready for a marathon." Nicholas Hall (Ohio State) warns: "Try not to work with people who are obviously overburdened with other tasks, or even other research." Stefan Thomke (Harvard) emphasizes that good coauthors should "delivers high quality contributions on time." To keep all coauthors on the same pace, Herbert Moskowitz (Purdue) suggest: "Meet once per week (regularly) to discuss/evaluate research status and progress, and to set goals for following week."

What are your rules of writing a publishable paper?

The anonymous stellar scholar joked that a paper published in an academic journal is read on the average by five persons – the author, the editor, and the three reviewers of the paper. This is not far from the truth because most academic papers are difficult to understand and follow even for the professionals. As a result, it is naturally that most stellar scholars mentioned the importance of keeping reviewers in mind during the paper writing.

Many stellar scholars ranked “Contribution” as the No. 1 necessity of a publishable paper. They believe that a publishable paper must “be very clear what your contribution is to the literature” (Christian Terwiesch at Wharton), whether its advancing theory, developing a new methodology, empirically examining an important issue, etc. Egon Balas (Carnegie Mellon) says: “My rule is simple: a paper is (or should be) publishable if it has something new to say. How significant the new thing is will decide whether the paper should go to a top level or to a less exigent publication. But, again, the ruling criterion is to have something new to say.” Reviewers are not here for the money. The real benefits for them are that they stay current in their own fields and improve their own reputation by being associated with a good-quality journal. As a result, reviewers often think in terms of “value of the paper per unit time spent reviewing the paper” (James Orlin at MIT). You have to “make sure the contribution is evident right up front” (Wallace Hopp at Michigan).

Reviewers are “forced” to read your paper, so that they often will not be directly interested in your paper (Nicholas Hall at OSU). In addition, because the reviewers are often those who work on different topics, it is important to help them find your paper interesting (James Orlin at MIT). The paper should grab the reader's attention and interest early (Michael Crum at Iowa State). The author should facilitate the review of the paper by not placing unreasonable demands upon the readers. Wallace Hopp (Michigan) reminds junior researchers that they should be very careful to cite the literature properly: “An author who fails to cite a previous paper that is related to the current paper can be perceived as trying to deceive the referees into thinking the contribution of the current paper is larger than it is. Nothing turns off referees faster than a perception of deception.”

Many stellar scholars emphasize the importance of expression. They advise junior researchers to “strive for excellence in writing” and “strive to make explanations and reasoning as simple, clear and direct as possible” ((Urban Wemmerlov at Wisconsin; Garrett van Ryzin at Columbia). Michael Crum (Iowa State) points out that “nothing frustrates reviewers more than a paper that is poorly organized, full of typos, formatted improperly, etc.” The quality of reading flow stems naturally from a well-organized outline. Charles Corbett (UCLA) says: “write VERY VERY carefully and thoroughly; define the outline of the paper, then the outline of the sections, then the outline of the subsections, etc. Don't just start writing, it will be impossible to read and painful to edit." An anonymous stellar scholar mentions: "In many ways writing a research paper is like writing a good novel, which gets readers absorbed in the plot and eager to read the next chapter."

What is your philosophy of research?

Facing the four MFMOs, almost all stellar scholars ranked "follow your interest" as the top issue, i.e., "find a research topic/area about which you are passionate" (Bruce Golden at Maryland). John Current (Ohio State) says: "In my opinion, B (follow your interest) is the most important advice in the entire list!" Wallace Hopp (Michigan) points out that "too many authors seem to do research on problems because 'they can'." In fact, "doing quality research requires a passion for the topic" (John Birge at Chicago).

The second most important attitude of research was ranked as "focus on the big problem", because the big problem related research "will be published forever" (Nicholas Hall at Ohio State). Gary Pisano (Harvard) gives more detailed reasons of why junior scholars should do this: "Too often, younger scholars fall into a trap of thinking they have be 'safe' by doing incremental work well. That's NOT a safe approach. In fact, I would say it is a sure fire way to get nowhere. When you start working on a project, ask yourself: what is the upside? Can I have big impact? If not, don't do it. It is not going to help your career to publish one or two incremental papers. You might as well strike out completely. Go for the big winners." Aleda Roth (Clemson) concurs: "Research should address problems and issues that can potentially have a high impact on the profession and society – either in substance or in research methods, and oftentimes in both areas. Importantly, senior scholars can often contribute to the development of junior colleagues as collaborators, mentors and readers."

While many stellar professors praise the attitude of "focusing on the big problem", they also remind junior scholars that "no one can write a 'big idea' paper every time out" (Wallace Hopp at Michigan) and "to focus on big problems requires enormous foresight, patience, and permission to spend time on research that may take long time to come to fruition" (Urban Wemmerlov at Wisconsin). Wallace Hopp (Michigan) suggests to junior researchers: "it's better to think in terms of a portfolio. With the goal of having an impact on an area you really care about, write some papers that address small issues and some that address large ones. While you might not change the world with a single paper, you might with your cumulative portfolio." John

Current (Ohio State) compares the creation of a research portfolio to landscaping: “You need some tall oak trees but you also need some lesser trees and shrubs as they also enhance the overall impact. Over time, some ‘shrubs’ end up being a highlight of the landscape (portfolio).” To build up such a rich portfolio, beside “focusing on the big problem”, stellar scholars’ advice converges on “starting with simple ideas and extending them as far as possible to increase applicability and understanding” and “doing derivative work is okay, as long as it is not trivial.”

5. Closing thoughts

The POM field is a relatively “hard” discipline in management. Gregory Dobson (Rochester) describes: “Our field is a strange one. What we do is not quite science since we don't just *explain* the world but work on finding ways to *improve* it. We don't just want to make a *particular* situation better because we want to *generalize* what we learned. We struggle to generalize by making our model *mathematical*, but occasionally fail to make *realistic* in the simplifying assumptions.” Junior scholars must struggle with these conflicts to develop their research “rigorous enough to defend to academia while relevant to interest business people” (Seungjin Whang at Stanford).

While junior scholars may possess the latest analytical/statistical techniques, these research methods tell them almost nothing about how to do research. “Vision” and “sense” in research are unlikely to be learned through examinations. Junior scholars need guidance and experience to build up their vision and sense in research. Consequently, interaction and feedback with established scholars is a necessary part of this process.

The beginning of this paper started out by posing issues that were important to emerging scholars. As we progressed through the paper, the observations, advice and even philosophical commentary about research were provided by leading POM scholars. Some common wisdom that was culled from these scholars for developing a research agenda are listed as the following principles:

Principle 1: You must be curious

While new ideas can come from everywhere, whether from media, colleagues’ papers or presentations, students’ questions, or consulting projects, the underlying principle is that you must have a basic curiosity to synthesize unconnected pieces of information and relate your knowledge to the real world; otherwise, you are not able to explore new insights beyond known.

Principle 2: The dichotomy of literature is not a matter

No matter which school of thought you belong to, it is important for emerging scholars to recall the purpose of academic research: to advance the collective knowledge of the discipline. So it is not a matter of either the snapshot or the whole picture. “Be driven by the world around you, not the academics next door” (Christian Terwiesch at Wharton).

Principle 3: You must know and understand reality

While all of the following statements are true – models should be parsimonious, models are appropriately developed by trial and error, and modeling is something of an art – the real key to a good model is that it captures the true essence of the system under study. “No amount of modeling skill can serve as a substitute for a deep understanding of the problem” (Wallace Hopp at Michigan).

Principle 4: Working with the right people

“The collaboration has to be based on mutual respect and support” (Hau Lee at Stanford). As a result, your coauthors’ personality, complementary expertise, research interests, and working styles and habits are all critical to a successful collaboration.

Principle 5: A publishable paper is a trio

Contribution, motivation, and expression are all important to a publishable research, and very hard to pick one as more important than another, i.e., “these factors are multiplicative, not additive” (Charles Corbett at UCLA), so if a paper completely fails on any of the three aforementioned, it's not publishable.

Principle 6: Follow your interest rather than your tools

“If you aren’t interested in your topic – no one else will be either. Your goal is to convey your enthusiasm” (Kenneth Boyer at Ohio State). If you are engaged in problems that you enjoy, whether they are big problems or small ones, “you are creating, not working; you have a passion, not a job” (John Current at Ohio State).

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Table 1. Two-Round Delphi Survey Results and Analysis

Questions	Most Frequently Mentioned Opinions (MFMO)	Frequency Counting (1st round)	Importance Rating* (2nd round)	Analysis of Importance Rating
Where do ideas for new research come from?	Reading the literature	32	2.92	ANOVA results:
	Contact with the real world	28	3.04	F = 19.25; F-critical = 2.41; p-value = 0.00
	Curiosity about things	27	2.15	Reject H_0 (All means are same)
	Networking	19	2.37	
	Teaching	9	4.06	
What is the efficient/effective way to build up the scholarship?	Whole picture	36	1.29	Paired t-test results: t = 0.82; t-critical (two-tail) = 2.12; p-value = 0.42
	Snapshot	31	1.47	Fail to reject H_0 (Two means are equal)
Facing the complex real world, what are the key points that your model setting or research design can reflect the reality?	To ensure reality, know and understand reality	29	1.61	ANOVA results: F = 24.18; F-critical = 2.65; p-value = 0.00
	It is a trial and error process	21	2.12	Reject H_0 (All means are same)
	It is an art	13	2.82	
	Remember Occam's razor	8	3.06	
What are your rules of collaboration?)	Personality	30	2.37	ANOVA results:
	Research topic	27	2.58	F = 1.16; F-critical = 2.65; p-value = 0.33
	Expertise	25	2.31	Fail to reject H_0 (All means are same)
	Consistency	12	2.65	
What are your rules of writing a publishable paper?	Contribution	43	1.53	ANOVA results:
	Motivation	39	1.67	F = 1.54; F-critical = 3.06; p-value = 0.22
	Expression	34	1.78	Fail to reject H_0 (All means are same)
What is your philosophy of research?	Follow your interests.	34	1.11	ANOVA results:
	Focus on the "big problem"	26	2.43	F = 95.99; F-critical = 2.65; p-value = 0.00
	Doing derivative work	22	3.43	Reject H_0 (All means are same)
	Start with simple ideas and extend them	18	2.78	

* For the important rating, the less the number is, the more important the item is.

APPENDIX

Stellar scholars in the field of POM (based on publications since 1985**)**

Name	University	H index (by 04/15/08)
Hau L. Lee	Stanford University	30
Awi Federgruen	Columbia University	22
Egon Balas	Carnegie Mellon (Tepper)	19
Paul H. Zipkin	Duke University (Fuqua)	18
Sunder Kekre	Carnegie Mellon (Tepper)	18
Dimitris J. Bertsimas	MIT (Sloan)	17
Uday S. Karmarkar	UCLA (Anderson)	17
Roger G. Schroeder	University of Minnesota (Carlson)	17
Morris A. Cohen	University of Pennsylvania (Wharton)	16
Wallace J. Hopp	University of Michigan (Ross)	16
Herbert Moskowitz	Purdue University (Krannert)	16
Suresh P. Sethi	University of Texas at Dallas	16
John Sterman	MIT (Sloan)	16
John R. Current	Ohio State University (Fisher)	15
Alan R. Dennis	Indiana University (Kelley)	15
Marshall L. Fisher	University of Pennsylvania (Wharton)	15
Nicholas G. Hall	Ohio State University (Fisher)	15
Panos Kouvelis	Washington University (Olin)	15
James B. Orlin	MIT (Sloan)	15
Lawrence M. Wein	Stanford University	15
Yu-Sheng Zheng	University of Pennsylvania (Wharton)	15
Gabriel R. Bitran	MIT (Sloan)	14
Paul Glasserman	Columbia University	14
Thomas L. Magnanti	MIT (Sloan)	14
David F. Pyke	Dartmouth (Tuck)	14
Nallan C. Suresh	Buffalo University	14
Christopher S. Tang	UCLA (Anderson)	14
Sridhar R. Tayur	Carnegie Mellon (Tepper)	14

W.C. Benton, Jr.	Ohio State University (Fisher)	13
Bruce L. Golden	University of Maryland (Smith)	13
Sushil Gupta	Florida International University	13
Haim Mendelson	Stanford University	13
Anna Nagurney	University of Massachusetts - Amherst	13
Abraham Seidmann	Rochester (Simon)	13
Glen L. Urban	MIT (Sloan)	13
Kenneth R. Baker	Dartmouth (Tuck)	12
John R. Birge	University of Chicago	12
Gérard P. Cachon	University of Pennsylvania (Wharton)	12
Zhi-Long Chen	University of Maryland (Smith)	12
Gérard P. Cornuéjols	Carnegie Mellon (Tepper)	12
Gregory Dobson	Rochester (Simon)	12
Izak Duenyas	University of Michigan (Ross)	12
Howard C.Kunreuther	University of Pennsylvania (Wharton)	12
Evan L. Porteus	Stanford University	12
David A. Schilling	Ohio State University (Fisher)	12
Shawnee K. Vickery	Michigan State University (Broad)	12
Peter T. Ward	Ohio State University (Fisher)	12
Urban Wemmerlov	Wisconsin (Madison)	12
Seungjin Whang	Stanford University	12
Kenneth K. Boyer	Ohio State University (Fisher)	11
Suresh Chand	Purdue University (Krannert)	11
Charles J. Corbett	UCLA (Anderson)	11
Stephen Eppinger	MIT (Sloan)	11
Barbara Flynn	Indiana University (Kelley)	11
Soumen Ghosh	Georgia Tech	11
J Michael Harrison	Stanford University	11
Paul Kleindorfer	University of Pennsylvania (Wharton)	11
Ram Narasimhan	Michigan State University (Broad)	11
Gary L. Ragatz	Michigan State University (Broad)	11
R. Ravi	Carnegie Mellon (Tepper)	11

Aleda Roth	Clemson University	11
Rakesh K. Sarin	UCLA (Anderson)	11
Jeannette Song	Duke University (Fuqua)	11
Morgan Swink	Michigan State University (Broad)	11
Christian Terwiesch	University of Pennsylvania (Wharton)	11
Richard B. Chase	USC (Marshall)	10
Clayton M. Christensen	Harvard Business School	10
Michael R. Crum	Iowa State University	10
Charles H. Fine	MIT (Sloan)	10
Robert M. Freund	MIT (Sloan)	10
Genaro J. Gutierrez	Texas – Austin (McCombs)	10
Lee J. Krajewski	University of Notre Dame (Mendoza)	10
Vincent A. Mabert	Indiana University (Kelley)	10
John O. McClain	Cornell University (Johnson)	10
Steven A. Melnyk	Michigan State University (Broad)	10
Praveen R. Nayyar	NYU (Stern)	10
J Keith Ord	Georgetown University (McDonough)	10
Michael L. Pinedo	NYU (Stern)	10
Gary P. Pisano	Harvard Business School	10
Srinivas (Sri) Talluri	Michigan State University (Broad)	10
Kwei Tang	Purdue University (Krannert)	10
Stefan H. Thomke	Harvard Business School	10
Garrett J. van Ryzin	Columbia University	10