Manufacturing & Service Operations Management

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Practice-Based Research in Operations Management: What It Is, Why Do It, Related Challenges, and How to Overcome Them

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Practice-based research—research performed with the intent of improving the operation of a collaborating practitioner—is an important endeavor for our field: such work may reveal new problems, interesting phenomena, and may also generate data, educational material, and solutions to important practical problems. We argue that the practical relevance of any operations management (OM) research is driven by the two dimensions of generalizability and validity, which together offer a framework for contrasting the potential strengths and weaknesses of theory-based and practice-based research. We review challenges and strategies for successfully engaging in practice-based research, including: choosing a good problem; establishing and managing a relationship with a practitioner; validation; and impact estimation. Finally, we discuss possible ways to encourage more practice-based research in OM. In particular, we argue that our field should, in general, put more emphasis on research validity.

1. Introduction

The *M&SOM Special Issue on Practice-Focused Research* was initiated to motivate, highlight, and consolidate high-quality scholarly work on the practice of operations management, with the hope of contributing to the greater recognition of such work as a rigorous and important academic endeavor within our field (Gallien and Scheller-Wolf 2013). As this special issue is now being published, we reflect upon the current status of practice-based research within our academic discipline. More specifically, the goals of this article include the following:

• Providing our perspectives on what practice-based research entails, why we might do it, and why it is of value to our discipline;

• Highlighting some challenges in the conduct and dissemination of practice-based research;

• Sharing some lessons and guidelines on how to address these challenges; and

• Suggesting opportunities—both for researchers and our discipline as a whole—for increasing the amount and impact of practice–based research.

Others have also written about practice-based research; these include Corbett and van Wassenhove (1993), Fisher (2007), Guide and van Wassenhove (2007), Sodhi and Tang (2008), van Mieghem (2013), and Simchi-Levi (2014). Although our thoughts necessarily have some overlap with theirs, our emphasis on the methodology of practice-based research, and the notion of validity, distinguish our work.

2. What Is Practice-Based Research?

Practice-based research entails engagement with a real operation; as such it requires a collaborative partner (or partners) who provides the contextual background and motivation related to a problem or phenomena relevant to their practice. This typically includes access to decision makers, decision systems, and data. An essential aspect of practice-based research, arguably differentiating it from research that is only *informed* by practice, is the opportunity it presents to directly test or validate the research work, be it through implementation or other means.

Practice-based research is not new. Corbett and van Wassenhove (1993) and van Mieghem (2013) highlight the historical importance of practice-based research within our field. Van Mieghem 2013 goes on to explain how our research has evolved from solving problems to developing theory, generating insights from models, and finally to extracting knowledge from data. The coexistence of these varying types of research is a source of strength and opportunities for our profession. Yet one consequence is that there is less practice-based research, which is regrettable in our view.

One intent of practice-based research is to help the collaborative partner improve their operations in some way. Depending upon the nature of the problem under study, this may require a significant implementation effort. Such implementation is primarily (or often entirely) directed by the collaborative partner, who possesses the required resources and expertise. This being said, several OM researchers have made significant contributions by extending their practice-based research efforts to include implementation, often by means of a commercial venture (see, e.g., Tayur 2013). The costs and benefits of such commercialization are a worthy topic of discussion, but beyond the scope of this article.

From this description, it is not clear how to differentiate practice-based research from consulting. We see at least three key differentiating features:

• First, the problem under study is new or not well-solved, and thus innovative ideas are required. This may make it difficult, or impossible, for a consultant to efficiently solve the problem, at least using known techniques.

• Second, there is often a distinctly educational component to the work. Graduate students, both doctoral and Master's, are typically involved in the work. And even when they are not, the insights from the research will often make their way into our classroom.

• Third, the collaborator understands both the need for rigor in the conduct of the work as well as our desire to eventually disseminate our findings in scholarly publications.

Corbett and van Wassenhove (1993) discuss how our field's initially dominant practice-based research ("Management Engineering") split into two streams: more formal/abstract theory ("Management Science"), and systematic commercial application of previously developed knowledge ("Management Consulting"). They also discuss a perceived crisis in our profession, which they attribute to too little practice-based research in relation to theoretical research and consulting activities, entailing a damaging disconnect between theory and practice.

In the next section we posit the contrapositive of this point: how *connecting* theory and practice—through practice-based research—offers benefits both to individual researchers and our profession.

3. Why Do Practice-Based Research?

We would highlight at least the following five benefits for the researcher and for our community.

You might discover an important research opportunity. By engaging with firms, one can learn about and appreciate the nature and nuances of a real problem, which may translate into a distinctive research opportunity. Sometimes the fundamental question is completely novel; other times it is not new, but it has been newly framed in a way that makes its importance to practice apparent. Such industry collaboration lends authenticity to our research, by providing motivation for the problem setup and a context in which to validate the research.

As an example, a project engagement with Kodak uncovered that they relied on ad-hoc methods for setting the safety stock levels within their material requirements planning (MRP) systems. Existing solutions found in the research literature were not applicable, demonstrating a clear need for a better way to determine these input parameters. This led to the research reported in Graves and Willems (2000). Another example arose from project work with Zara, which revealed ad-hoc manual approaches for determining shipments of different sizes of the same articles to stores, and for simultaneously determining markdown prices of related products during clearance sales. The existing literature was found insufficient to address these practical challenges, which led to Caro and Gallien (2010, 2012), and to Gallien et al. (2015). One further instance arose when Caterpillar contacted researchers to aid in the design of the supply chain for a new product line. During this process the need for a simple yet effective way to source from alternate suppliers became apparent. Solutions to this problem were an integral part of Rao et al. (2000) and were extended in Veeraraghavan and Scheller-Wolf (2008).

You might discover new phenomena which warrant new research. By engaging with practice, one might encounter new or understudied problems that arise because of new technological processes, business models, or operating practices. In the late 1970s, one of the coauthors had a project on scheduling semiconductor fabrication with Western Electric. At first, it was thought that the fab operated like a job shop, but upon inspection, it was seen to effectively be a flow shop but with *reentrant flow*. Such systems with reentrant flow had not been studied before (Graves et al. 1983), which created an opportunity for new research.

Consider also that practice-based research on improving operational planning processes effectively spawned the phenomenon of forecast evolution in Hausman (1969), Graves et al. (1986), and Heath and Jackson (1994); this has led to the Martingale Model of Forecast Evolution used by many other researchers. Yet another example is the role of demand learning uncovered by Raman and Fisher (1996) as part of a project with Sport Obermeyer, which has motivated much follow-up work and contributed to the development of an operational strategy known as Quick Response.

You might get access to useful data. Practice-based research might provide access to data useful for validating previous research: testing a theoretical model with real data can help evaluate the reasonableness and validity of various assumptions as well as the model's utility. This also may identify research gaps or potential extensions. One might also use data to develop empirical models of an operational or decision-making process, providing insights into the underlying dynamics; a classic example is Bowman (1963). In addition, practice-based research might uncover data sets that can be shared with the research community. Some examples are Bodea et al. (2009), Willems (2008), and Mumbower and Garrow (2014). Since many companies are reluctant to share their data, any initiatives that increase our access to such data could greatly benefit our community.

You might get distinctive educational dividends. Projects with companies can be very valuable learning experiences for our students, exposing them to real-world operations. They provide an opportunity to learn about the challenges of conducting rigorous and relevant research in settings with real-world ambiguity and organizational dynamics. Such projects can also yield pedagogical material, like teaching cases, bringing our research to the classroom. Indeed, many widely used cases originate from research-based projects, such as the HP Deskjet Printer case by Lee and Kopcak (1994), the Sport Obermeyer case by Hammond and Raman (1994), and the Zara case by Caro (2012).

Finally, one can view such projects as part of our own personal development—participating in these projects we inevitably learn something new about operations that can be useful both in teaching and in future research. These projects also allow us to develop new relationships and to extend our professional networks; this can lead to continued opportunities for research and learning.

You might solve a problem and have an impact. Operations management is an applied field of study. As such, a primary motivation for our research should be to have an impact on society. There are many paths by which our research can result in impact: as pedagogical material taught to students who might then apply these ideas over the course of their careers, or as software packages, as has happened with inventory optimization and revenue management, for example. Project-based research is another—very direct—way for our research to have an impact. By design, project-based research attempts to address a real problem with the intent of developing a better solution; in this domain, ultimate success often is in terms of achieving measurable benefits in practice.

More generally, practice-based research constitutes one avenue for our field to achieve practical relevance. There are other avenues however, which leads us to discuss the drivers of practical relevance for any research in OM.

4. Practical Relevance and Relationship with Theory

Although our thesis is that practice-based research is extremely valuable, we do not mean to disparage "purely theoretical" OM research, whatever that means, nor are we saying that OM as a discipline can only justify itself through appeals to practical relevance. Rather, in the spirit of Fisher (2007) and Corbett and van Wassenhove (1993), we highlight the synergy between OM theory and OM practice: Practice has historically been an inspiration for many important theoretical contributions within the OM literature, as well as a crucible to test many of our theories. Furthermore, we argue that both theory-based and practice-based research can be practically relevant.

So what makes OM research practically relevant? We see two drivers: how much the research question matters to society, and how useful the answer from the research is. Based on these elements we propose the following qualitative equation for the practical relevance for OM research:

$$\text{Practical Relevance} = \text{Generalizability} \times \text{Validity}$$

We define validity as *the extent to which research results and prescriptions (or predictions) are well-founded and apply effectively to real-world operations.* We define generalizability as *the extent to which the research question considered is of interest to a large number of practitioners.* This equation is reminiscent of, and we hope complements, that in Cachon (2012) (Impact = Interesting × Important).

Our equation provides a framework for contrasting potential strengths and weaknesses of theory-based and practice-based research. Theory-based research, focusing on fundamental research questions and formal abstract constructs, has the potential to achieve great generalizability. But, if too general, it may have

limited relevance to specific, practical settings. In contrast, the deep empirical grounding typically associated with practice-based research may confer substantial validity, but may also present a risk in terms of generalizability.

To illustrate this discussion, consider the following two hypothetical research questions:

• How should a manufacturer with unit production cost c and a buyer with fixed unit retail price p contract with each other?; and

• How should Flextronics negotiate the purchase price of high-resolution screens from Hitachi in 2014?

In the first example, one might question the extent to which the insights generated will apply in the real/practical context motivating the research question. For example, good contracting approaches could depend on uncertainty surrounding the retail price and/or manufacturer's production cost, which might be ignored by the model. Thus, although many manufacturers and retailers would find the question of how best to contract with each other relevant to them (so that this research question is of general interest), the conclusions from this work may not be valid (applicable) in practice.

In the second example, a typical concern is not its validity for the team buying screens for Flextronics in 2014 (although that dimension should certainly be evaluated), but rather whether the conclusions from this work would apply to other contexts, i.e., whether this research is generalizable.

Again, we do not mean to suggest that theory-based OM research is intrinsically less valid than practice-based work: There are many theory-based OM papers reporting seminal results with compelling validity. To cite just two examples, consider the studies by Lee et al. (1997) on the bullwhip effect, or by Cachon and Lariviere (2005) on revenue sharing contracts. In fact, the previous simple contracting model example could yield results that in fact have some predictive accuracy or prescriptive value in a number of situations.

Likewise, we are not saying that practice-based research is intrinsically less generalizable than theory-based work—we have already cited many practice-based papers suggesting exactly the opposite. But, because generalizability does constitute a risk factor, we next elaborate on this (and other) challenges of practice-based research, as a prelude to offering related guidance.

5. Challenges and Successful Strategies

5.1. Choosing a Good Problem

Like any research endeavor, one must start practice-based research with a good question—one that is
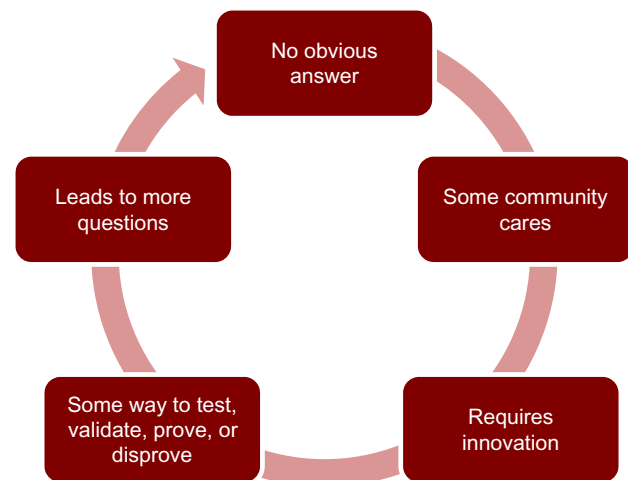
not only relevant to a specific practical context, but that will also yield findings of interest to a broader community. A good question has the characteristics shown in Figure 1, some of which harken back to §2. Note that these characteristics suggest a reinforcing cycle.

For practice-based research we would highlight two of the elements:

Some community cares: The problem needs to be important to the collaborator, but this is not enough. One wants to find a problem that will generate research of broader interest and value: Will the research be of value to other researchers or practitioners? If so, why? One should have good answers to these questions before embarking on a practice-based research project.

One positive answer to this question is that the problem is pervasive and your research will inform other related instances, possibly through direct application or adaptation. Or the research might consider an innovative business practice, like fast fashion at Zara or resource sharing at Uber, satisfying general curiosity as to the nature and characteristics of the new business model. Thus the research will generate broad interest by providing a deeper understanding of the innovation.

Alternately, practice-based research that builds upon an established research literature will find a natural audience. For instance, research might test new and existing methods to solve a practical problem and/or create a benchmark for an already identified important problem. Examples include Sherbrooke (1968) and Muckstadt (1973), who report the development and implementation of inventory models for managing military spare parts.

Finally, research might attempt to inform a controversy among practitioners (e.g., pull versus push

control). As one example, Gallien and Weber (2010) examine different strategies for order picking (wave-less versus wave-based) in warehouses with automated sortation systems.

Some way to test, validate, prove, or disprove: One should try to anticipate how the research might be tested or validated. This applies to any research of course, but for practice-based research, there usually is a natural test-bed. Thus one should confer with the collaborator as to how exactly the research might be tested, and what data and other resources can be leveraged to do this.

One important challenge is that industry moves more quickly than academia, so there may not be the patience for a test that meets the standards of academic research. Another complication is that a collaborator, who has funded a project, might have a vested interest in the outcome of such a test to "prove" the project's value. Finally, depending on the industry, the operational context may itself be a moving target. One needs to recognize these realities when designing and executing the tests to assure scientific validity; we provide related methodological guidance in §§5.3 and 5.4. And, as a community, we need to recognize the constraints practical research places on such tests.

5.2. Establishing and Managing a Successful Relationship with a Collaborating Practitioner

Much of the advice we would give on forming and managing a successful relationship with a collaborating practitioner is very similar to advice guiding any relationship:

(1) Find a compatible partner you can trust;

(2) Be clear about what you both want out of the relationship;

(3) Build your relationship through communication, honesty, and compromise when necessary; and

(4) Carefully manage the areas which are most likely to give rise to conflict.

First, know what you are looking for: A partner to whom you can offer something—your research expertise, and who can offer you something—an interesting, practical problem you can work on *and potentially publish*. Failure in either of these dimensions will likely doom the "match."

Next, you have to put yourself in positions where you can meet good potential matches. Within the academic environment these include the following: Inviting industry speakers to your class or seminar; meeting visitors to your school, including recruiters or members of advisory boards; former students; and site visits with student groups. Internships for your students are an excellent way to foster a relationship with, and learn about, a company (Guide and van Wassenhove 2007 discuss this as well).

After you meet a potential partner, offer to go visit his or her industry site, or invite him or her to your school. Sometimes your potential partner may be slow to respond because of business cycles; in this case it pays to be persistent. But if a partner is regularly unreliable or unresponsive, it may indicate that "he or she is not that into you," and you had best look for a more mutual relationship.

At the end of your "courtship" you need to have identified a project or projects that are of compelling interest to the company and of academic interest to you. You also need to agree on:

• What tangible deliverables does your partner expect? A white paper? Software? A full implementation? What is the prospective timeline?

• Who has ownership of any intellectual property created, and what guidelines/restrictions will there be on its sharing?

• What data is your partner willing to share, and in what form?

• Will there be any restrictions on publication (timing, data, etc.)? Will your partner expect to vet any preliminary drafts? If so how long will your partner have to review the document prior to submission? How will disagreements be resolved?

• What will the financial terms of the relationship be?

This last point deserves discussion. Although it might seem as if *not* charging any fees would make partners more cooperative, we have found that the opposite is often true: Many business people believe that they get what they pay for. Imposing a financial commitment on them to engage your participation can help ensure that they remain attentive to your project needs.

Assuming you get this far, you now have to manage the relationship to bring the project to fruition. The key here is communication. You will need to sustain the interest and engagement of your partner—even when the project moves at academic speed rather than corporate speed—by managing expectations and regularly updating with progress reports. And, when possible, you should design the project so that it spins off shorter-term results that benefit your collaborator.

Within the partner firm one should expect a natural resistance to change, which can be considerably heightened when some stakeholders fail to understand a solution or to see benefits for themselves. It is imperative that your partner be willing—and powerful enough—to champion your project. Part of this responsibility is identifying others in the company who can benefit from your project and/or can support it.

Even in the best relationships, conflicts will invariably arise. It is useful to know where common flashpoints are, so they can be managed proactively. The first of these is the mechanics and understandings around the sharing of data: Without data most

projects will go nowhere. Yet most companies view their data as an extremely valuable *private* asset. Thus it is crucial to think carefully about what data is really needed, and get assurances that there will be access to this data.

Even when you are granted access, data often requires significant effort and resources to obtain, clean, and manage. Such effort should not be underestimated. Ideally, someone from the research team will embed themselves within the organization and, in collaboration with a designated organizational counterpart, obtain the necessary data in the form needed.

The second potential flashpoint involves publication, particularly at the sensitive time when a paper is ready for submission, or a thesis for defence. Industry partners will want to vet the publication, to assure that sensitive or proprietary information is not being revealed. But there should be a mutually understood timetable for such reviews. In particular, if their legal team is to provide input, explicit expectations will need to be set regarding completion times and the scope of their authority.

On the topic of lawyers, we have found it more difficult to launch and manage projects of late because of increased involvement of legal teams (from firms and universities). It is important to understand that this process needs active management from both sides: Pressure from you and your industry counterpart is often the best way to facilitate agreements. When to get lawyers involved, if this happens at all, may vary by setting and project, but certainly no later than when expectations are formalized by each party.

5.3. Validation

As discussed in §3, a key appeal of practice-based research is the potential to validate the research results. Demonstrating this validity is therefore important, in part because respected publication venues for practice-based research (e.g., *M&SOM*, the Practice Sections of *Operations Research* and *POM*, *Interfaces*) require authors to establish realized or potential applicability to practice. Some common approaches for establishing the validity of practice-based research include the following:

• Discussion of the realism of model assumptions, preferably with supporting data. This seems to be the most common approach for arguing validity in OM research papers today;

• Observation that some salient recommendation or prediction of the model coincides with observed practice. A systematic analysis of this sort can be found in Wan et al. (2012);

• Quantitative study of the model's predictive accuracy: evaluating the model based on the proximity between model predictions and actual observations in practice. This evaluation must be performed

out-of-sample—the part of the data set used for fitting the model (fitting sample) must not be used for assessing predictive accuracy (validation sample). An example of this approach can be found in Gallien and Weber (2010);

• Report of an implementation by the collaborating partner of the research results in some form, or description of steps taken to implement them. This information is relevant to validity because it may show that some of the assumptions underlying the research, such as input data requirements, are compatible with the actual motivating environment. This may also suggest that the collaborating practitioner, who is typically both well-informed and strongly incentivized for any intervention in its own environment to succeed, determined that an implementation of the research results was likely to have a positive impact justifying any time and resources required for the implementation. Because of its editorial policy, the journal *Interfaces* contains many examples of such implementation descriptions; and

• Report of positive impact from an implementation. Any such impact will typically be prominently highlighted, as indicative of the practical value of the research (see §5.4). Realized positive impact also provides some *validation* (which is distinct from *impact*)—evidence of directional alignment between research predictions and results. A good example of this is Misra and Nair (2011). We note that this validation opportunity is not always fully leveraged: Many studies fail to evaluate any *discrepancies* between predicted and realized impact. Consider, for example, an implementation arising from a collaborative research project that is estimated to have increased annual sales by 10%. If this annual sales increase were very different from the corresponding model-based prediction, this would provide an opportunity to evaluate the relationship between model and reality. Thus the notions of impact and validation are distinct: Validity is an absolute concept relating research predictions to reality, impact is relative defined in relation to an arbitrary starting point.

Generally speaking, quantitative evidence of validity is preferable to qualitative evidence, and direct evidence of predictive accuracy is preferable to more indirect validation of model assumptions or input parameters. When assessing validity however, it is also important to consider the context for the research work in addition to other factors such as the amount of validation data available, its statistical significance, and any additional related evidence provided in the existing literature.

5.4. Impact Estimation

Accurately assessing the potential or realized impact of practice-based research is extremely important. To

the collaborating practitioner it provides an indication of the benefits, realized or potential, from the deployment or adoption of the research output. From an academic standpoint, an estimate of impact may be an important factor in a journal's decision to publish research work, in part because theoretical analysis may have been compromised to enable practical applicability. In addition, impact estimates may serve as a benchmark or motivation for other researchers (and practitioners) in this area, and realized impact can provide some indication of validity, as discussed in §5.3.

In addition, assessing and communicating impact may confer some legitimacy, visibility, and/or prestige to the researchers, the collaborating practitioners, and our professional discipline as a whole. The Franz Edelman competition is widely communicated and highlighted to the public by INFORMS, where its impact is arguably used as a proxy for the value of our entire profession. Relevant target audiences for these communications include industry practitioners, government and private research funding agencies, colleagues from other disciplines that we interact with within our academic institutions, and young people whom we would like to attract to our profession.

This context obviously generates potential biases affecting the estimation of impact, creating a need for awareness of estimation methodology. We would first broadly characterize the impact of practice-based research based on whether it is *qualitative* or *quantitative*; and *potential* or *realized*. It is usually established by one or several of the following methods:

• *Statement from collaborating practitioner*: Such a letter may be public or only shared confidentially with reviewers. Important features when soliciting or evaluating such letters include the title or responsibility of the letter writer, how specific and quantitative the letter of support is, whether the company is public (in which case its communications may have legal ramifications), and what information can be made public in the accompanying paper;

• *Model-based evaluation of potential impact*: This is typically performed with a simulation model, or an optimization model that estimates the potential impact from the difference in objective values between the legacy and recommended practices, respectively. The central methodological question here is the evidence establishing the validity and predictive accuracy of these models relative to the motivating practical environment (see §5.3);

• *Field implementation*: Any field implementation in itself is a form of impact, since it is a change in practice brought about by research. Relevant questions here include the scope of the implementation in duration, geography, number of affected products, or customers, etc. Field implementations are also often used as experiments to estimate the quantitative impact of the proposed changes. Central methodological questions for these field-based estimations include the statistical significance of the results presented, and more generally the extent to which the estimated field impact could be explained by confounding factors (macroeconomic conditions, seasonal changes in demand, changes in employee behavior, etc.) having nothing to do with the research work being evaluated.

The simplest method is "before versus after" estimation. For instance, the difference in average monthly sales before and after a new pricing model implementation is used to estimate the impact of that model; yet, many other factors might have contributed to any observed difference. A more accurate method is "difference in differences" estimation, which specifies a control group not affected by the intervention considered. It then computes the difference in "before versus after" estimations between the intervention and control groups. Since the intent of this method is to eliminate the effect of confounding factors, a key issue is whether any such confounding factors would affect the control and intervention groups' performance metrics in exactly the same way.

A preferred form of field impact estimation is a randomized controlled trial, whereby a large number of test units (customers, products, stores) are randomly allocated between a control and an intervention group. The intervention's effect is estimated from differences in some performance metric across groups; the randomization helps ensure that confounding factors do not bias the estimation. (See Cox and Reid 2000, for a discussion of such methods.) The increasing number of controlled field experiments in OM is an exciting development, as they have transformed other fields such as development economics (Duflo et al. 2008) and are also impacting business practices through "A/B testing." Properly controlled experiments with high statistical significance can be very challenging to organize however: The collaborating practitioner may be unwilling to risk an experiment with sufficient sample size; there may be financial, legal, ethical, or resource constraints; practical issues such as IT systems or organizational structure may limit design options; and so forth. For examples of controlled field experiments in OM and discussions of practical challenges, see Caro and Gallien (2010, 2012), and Gallien et al. (2015).

When assessing the impact from an implementation or test, one ideally would examine not only the performance metrics, but also the impact on the operational decisions related to the performance improvements (e.g., Foreman et al. 2010). This can increase our understanding of how the improvements were achieved. And providing an explanation of performance improvements may facilitate implementation,

by increasing the understanding and ownership of the research work by collaborating practitioners. This enables them to promote a solution that could be otherwise seen as a black box.

Although it is desirable for impact to be evaluated using objective quantitative metrics, qualitative impact can be critically important in some environments, for example, when research results change the way in which many individuals think about an issue (e.g., Raman and Fisher 1996). Likewise, measured realized impact is superior to potential impact predicted through numerical analysis. Indeed, documented implementation is required by some practice-based publication venues, such as *Interfaces* and the Practice Section of *Operations Research*.

We believe that our field would benefit from a more nuanced perspective: getting a partner to implement a solution can be very challenging, for reasons that may be completely outside the control of the researchers and have little to do with the potential impact of their work. Even when work is implemented, organizing the implementation so that it yields rigorous related knowledge can prove equally difficult. In contrast, computer-based numerical analyses enable one to carefully design experiments in a controlled manner. Here again, however, a critical issue is validity: How do we know that predictions calculated by computer, assuming a simplified representation of reality, will hold true in practice? Thus, although demanding a reported implementation may be too coarse a response and hamper the progress of our field, there remains the risk associated with widely publicizing impact estimated with less than rigorous methods.

These intertwined concerns highlight an *opportunity*: to further develop and recognize rigorous approaches for validating predictive models, for example, through multiple out-of-sample estimations of predictive accuracy against historical data for key outputs. We can draw inspiration from other fields in this pursuit: public health simulations of polio eradication strategies have saved numerous lives (Thompson et al. 2015), and the rich interactions between numerical analysis and theory in fields such as physics and engineering (Zhou et al. 2001, Mouhot and Villani 2011) are a source of both validation and problem generation. In a similar fashion, we see the potential for OM to significantly increase the practical knowledge it generates from numerical methods such as simulation.

6.   Concluding Remarks
We hope we have made a convincing case for the value of practice-based research—personally, to our profession, and the larger community. But this raises a question: If this research is so valuable, why do so

many writers—including us—bemoan the scarcity of high-quality practice-based research within our field? Shouldn't the research market work to incentivize such highly valuable research?

Unfortunately, we would argue that this is not the case, or at least not fully. OM researchers have a choice about what they will work on:

• They can posit and analyze a stylized model, extrapolate insights from these findings and write a paper; or

• They can engage with a real firm, build and manage this relationship, identify a specific and often messy problem, secure and clean data, form and analyze a model, produce recommendations, validate results, write a paper, vet it with the firm, and *then* submit it.

Strategically, it seems much wiser for faculty—particularly junior faculty—to choose the first path. After all, we are operations folks—we should understand that fewer and simpler steps make a more efficient process. So just from the perspective of throughput, practice-based work loses out.

Well, if the risks (and effort) of such practice-based research are higher, maybe the rewards would at least partially compensate for this? Again, we would argue that this is not always the case. Several top schools either discount publications in practice-based outlets like *Interfaces*, or may not count them at all. Although weighing the relative values of different publication outlets is not our intention, to place zero value on a peer-reviewed publication from an INFORMS journal seems a bit extreme.

So what do we propose? We provide some concrete suggestions as to how we as a community can foster more high-quality practice-based research (which, yes, includes simply valuing it more):

Make it easier for researchers to find and initiate projects by facilitating relationships between industry and academia (e.g., by engaging practitioners and industry researchers into MSOM);

Make it easier for researchers to bring these projects to fruition by disseminating relevant research experience and best practices (in a piece such as this one, and others such as Guide and van Wassenhove 2007, Fisher 2007, Roth 2007, and the INFORMS professional colloquium), as well as providing direct mentorship to junior faculty entering into this process;

Reduce publication risks by increasing the number of outlets for high-quality practice-based research and identifying experienced, sympathetic journal editors and reviewers;

Increase incentives for high-quality practice-based research through prizes and awards (especially for junior faculty; a recent initiative is the POMS Applied Research Challenge, see Caro and Tang 2015), emphasizing articles reporting practice-based research in

journals, and highlighting this work prominently in promotion-review letters; and

Increase appreciation among the community for high-quality practice-based research by publicizing its value to our community and society as a whole through introducing problems, providing access to data sets, and simply improving operational practice (the recent Science-to-Practice initiative launched by *Marketing Science* is worth mentioning, see Desai et al. 2012).

Some of these suggestions are akin to initiatives recently undertaken by POMS (Singhal et al. 2014).

At an even higher level, we would argue that maybe we, as a field, need to rethink what it means to do highly valuable OM research.

In §4 we posited that the practical relevance of OM research depends on generality and validity. We want to emphasize that this is a standard we endorse for *any* OM research: Maybe we should evaluate research not just on "correctness" and "novelty," but also on "validity"—how well it applies to the real world. This latter standard is applied to some extent now, but we as a field could do more of it, and certainly more consciously.

The comparison initiated by Fisher (2007) between the academic disciplines of OM and Medicine is useful here, because in medical research validity (e.g., on the impact of a drug or medical procedure) can literally be a matter of life and death. As a result, medical research has a longer and deeper history assessing validity. Many aspects of medical research are dictated by this concern. For example, public registries of research data (such as clinicaltrials.gov) and attempts to reproduce research results by others are more common, research methods and article formats are more standardized, and model results presented without validation are more systematically questioned (Garnett et al. 2011). Perhaps most importantly, authors and reviewers of medical research tend to systematically evaluate and discuss the *standard of evidence* associated with any finding having clinical or policy implications (Sackett et al. 1996).

Why has the field of OM not focused as much on the validity of its results? In contrast to the strong empirical training of doctors and medical researchers, many OM scholars train in fields of applied mathematics, where the dominant knowledge creation paradigm involves self-contained formal results (e.g., lemmas, propositions, theorems). For these results, the question of validation does not arise naturally, as the notion of truth essentially comes down to internal consistency. We should certainly celebrate the substantial and distinctive progress made within our field using mathematics to solve important and challenging problems, and continue to nurture these endeavors and methodological developments. But, lacking a

tradition, tools or incentives to evaluate the validity of our results, we run the risk of focusing too much on the mathematical complexity or novelty of our methods and too little on validity. As a result, we believe that our discipline could benefit from a wider recognition that theory does not have to be complex to be valid or important, and from raising and communicating the standards of evidence associated with all of our research results.

Practice-based research provides a unique closed-loop environment, inspiring research problems, *and* providing a laboratory to test and validate solutions. As such, it can be a valuable platform for establishing what it means to do *valid* OM research. And although we should recognize that not all research can be readily validated, we can, as a field, consciously recognize that validation is something to which all of our work should aspire.

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