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Making a meaningful contribution to theory

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Published in:
International Journal of Operations and Production Management

DOI (link to publication from Publisher):
10.1108/IJOPM-03-2015-0119

Publication date:
2015

Document Version
Publisher's PDF, also known as Version of record

Link to publication from Aalborg University

Citation for published version (APA):
Boer, H., Holweg, M., Kilduff, M., Pagell, M., Schmenner, R., & Voss, C. (2015). Making a meaningful contribution to theory. *International Journal of Operations and Production Management*, *35*(9), 1231-1252. https://doi.org/10.1108/IJOPM-03-2015-0119

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International Journal of Operations & Production Management

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Article information:

To cite this document:
Harry Boer Matthias Holweg Martin Kilduff Mark Pagell Roger Schmenner Chris Voss , (2015),"Making a meaningful contribution to theory", International Journal of Operations & Production Management, Vol. 35 Iss 9 pp. 1231 - 1252
Permanent link to this document:
http://dx.doi.org/10.1108/IJOPM-03-2015-0119

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Received 16 December 2013
Revised 31 May 2014
5 September 2014
Accepted 17 October 2014

Making a meaningful contribution to theory

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Abstract

Purpose – The need to make a "theoretical contribution" is a presumed mandate that permeates any researcher's career in the Social Sciences, yet all too often this remains a source of confusion and frustration. The purpose of this paper is to reflect on, and further develops, the principal themes discussed in the "OM Theory" workshop in Dublin in 2011 and the special sessions at the 2011 and the 2013 EurOMA Conferences in Cambridge and Dublin.

Design/methodology/approach – This paper presents six short essays that explore the role and use of theory in management research, and specifically ask what is a good or meaningful contribution to theory. The authors comment on the current state of theory in Operations Management (OM) (Harry Boer), the type of theories the authors have in OM (Chris Voss), the role of theory in increasing the general understanding of OM problems (Roger Schmenner), whether the authors can borrow theories from other fields or actually have theory "of our own" (Matthias Holweg), the different ways in which a contribution to theory can be made (Martin Kilduff), and how to construct a theoretical argument (Mark Pagell).

Findings – The authors argue that theory is fundamental to OM research, but that it is not the inevitable starting point; discovery and observation are equally important and often neglected avenues to contributing to theory. Also, there is no one right way to making a contribution, yet consistency between ontology, epistemology, and claimed contribution is what matters. The authors further argue that the choice of theory is critical, as a common mistake is trying to contribute to high-level theories borrowed from other fields. Finally, the authors recommend using theory parsimoniously, yet with confidence.

Originality/value – The paper presents a collection of viewpoints of senior scholars on the need for, and use of, theory in OM research.

Keywords Agility, Advanced manufacturing technology (AMT)

Paper type Viewpoint

1. Introduction

The need to make a "theoretical contribution" is a presumed mandate that permeates any researcher's career in the social sciences. It starts with doctoral studies, where great emphasis is being placed on making a general contribution to theory in the field, and continues with publishing afterwards, where one of the most frequent

International Journal of Operations & Production Management
Vol. 35 No. 9, 2015
pp. 1231-1252
© Emerald Group Publishing Limited
0144-3577
DOI 10.1108/IJOPM-03-2015-0119

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(and frustrating) rejection letters states that "[…] while your findings are empirically interesting, there seems to be an insufficient theoretical contribution to warrant publication". Inadequate theory remains a primary reason for journals to reject a paper (Sutton and Staw, 1995).

The resulting question that often arises, in operations management (OM) as much as in any other field, is: "I have done rigorous work on a problem relevant to practice, so what contribution to theory am I missing? Why is it not enough to have 'empirically interesting' findings? And if I find a solution to the problem that I have observed in the real world, why is this contribution to theory still necessary?" These questions are at the heart of what this paper discusses. This issue is particularly relevant due to the "applied" nature of OM, which stems from its dual upbringing in both industrial engineering and the social sciences. The engineering tradition stipulates that if one solves a given problem and is able to demonstrate a viable solution, the "job" is indeed done. Social sciences are more complex. Here, we seek a theory or set of theories that offer predictive value (i.e. that provide the ability to predict the relationship (causation) between two variables).

So what is this elusive "theoretical contribution"? Here several guidelines have been proposed: Whetten (1989) and Wacker (1998), for example, each define criteria for what constitutes a "valid" or "good" theoretical contribution, yet "ticking all boxes" is necessary but not sufficient for making a meaningful contribution to theory, in our view. Scholars often remain confused by the literature on theory building (Whetten, 1989; Sutton and Staw, 1995), as there is a lack of agreement as to whether a model or typology constitute theory, whether falsifiability is a prerequisite, and whether the strength of the theoretical contribution "[…] depends on how interesting it is" (Sutton and Staw, 1995, p. 371).

In this paper we present six different, complementary and not necessarily fully congruent views on the role of theory in management research. We explore the role and use of theory in management research, and specifically ask what is a good or meaningful theory. In Section 2 Harry Boer explores the current state of theory in OM; in Section 3 Chris Voss reviews the type of theories we have in OM; in Section 4, Roger Schmenner discusses the role of theory in increasing our general understanding of OM problems; in Section 5 Matthias Holweg explores whether we can borrow theories from other fields or actually have theory "of our own"; in Section 6 Martin Kilduff outlines the different ways in which a contribution to theory can be made; and in Section 7 Mark Pagell explores how to construct a theoretical argument.

2. Theory in OM: the current state of affairs

By Harry Boer

With its origins in Smith, Babbage and Taylor (Buffa, 1980) or even further back (Sprague, 2007; Voss, 2007), OM has become one of the key areas of management, covering topics ranging from "traditional" manufacturing management, via operations strategy and supply chain management, to service management. The table of contents of OM textbooks reflects how OM has changed in the course of its development. The earliest books (e.g. Buffa, 1961) deal with all core OM topics of the day, on the level of tools and techniques. Later books[1] (e.g. Wild, 1971) deal with largely the same topics, still present a wide range of OM tools and techniques, but add a systems and, sometimes also, service dimension. Current OM textbooks[1] (e.g. Slack *et al.*, 1995) are more conceptual, and focus on operations strategy, process and product design, philosophies such as lean manufacturing, and approaches such as continuous improvement. Furthermore, there is a wealth of textbooks addressing specific topics such as operations strategy, supply chain management and service management.

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There is no doubt that OM has had, and continues to have, a demonstrable impact on practice. Virtually all concepts studied by OM researchers and described in OM textbooks are found in companies. Equally, the OM community plays an important role in spreading its philosophies, concepts, systems and techniques to industry, especially through teaching and consultancy. In spite of its richness and managerial importance, OM as a scientific discipline has frequently been criticized, by itself and by others, for the theoretical status of the discipline and its relevance for practice.

OM theory and theory development

In the course of time, many "state-of-the-field" articles have been published (e.g. Buffa, 1980; Chase, 1980; Amoako-Gyampah and Meredith, 1989; Meredith *et al.*, 1989; Filippini, 1997; Pannirselvam *et al.*, 1999; Meredith, 2001; Pilkington and Fitzgerald, 2006), taking stock of and critiquing the state-of-the-theory and, in many cases suggesting visions (Meredith, 2001) or agendas for future research (either or not including criteria to assess the quality of that research) (Buffa, 1980; Chase, 1980; Meredith *et al.*, 1989) and/or aspects that should be borne in mind (Filippini, 1997).

According to Chase (1980, p. 12), "OM research does not draw upon management theory to any noticeable degree". Slack *et al.* (2004, p. 372) observe that, relative to "[…] strategy, marketing or finance", which "[…] are more-or-less directly connected to base theoretical disciplines such as economics, sociology, psychology and mathematics, OM's underpinnings are more fragmented". In 1998, Schmenner and Swink (1998, p. 97) summarize the general feeling in the OM community: "[…] operations management suffers in at least some quarters because there is no recognized theory on which it rests or for which it is famous". Eleven years later, Schmenner *et al.* (2009) debate the status of the field. In his part of the article, Schmenner asks: "[…] what is the status of theory in our field? Has it advanced? Are we using theory in productive ways to advance our understanding? How have our theories changed in response to empirical investigations? Which theories have been abandoned and which ones have been developed in their places? I am afraid that my responses to these questions do not ring with contentment. It is time to reassess the role of theory in operations management" (p. 339).

In order to solve the "lack of theory" problem, some authors use, or propose to do so, theories from adjacent areas to explain OM phenomena. Observing that "[r]esearchers in OM cannot afford to 'reinvent the wheel', and thus must borrow from other disciplines", Amundson (1998) gives three examples of the potential utilization in OM of theories from other fields, namely, transaction cost economics, from economics, the resource-based view of the firm, from strategic management, and organizational learning, from organization theory, and considers the advantages of using external theories along with possible pitfalls. Pilkington and Fitzgerald (2006) observe that the resource-based view has indeed emerged as a major theme in OM. Linderman and Chandrasekaran (2010) propose conducting more cross-disciplinary research with other management disciplines such as (general) management, finance, and marketing, so as to improve the scholarly development of OM. Sousa and Voss (2008) examine and critique the current state of OM practice contingency research and put forward a framework to underpin research on the process of selection of OM best practices by organizations by integrating contingency theory and other theoretical perspectives. Kauppi (2013) proposes to extend the use of institutional theory in OM and SCM research.

Handfield and Melnyk (1998) propose a "road map" for OM researchers to follow in developing theory-driven research (observation, empirical generalization, theory creation, hypothesis generation and testing, logical deduction), and outline a number of key attributes for evaluating this research ("not wrong", falsifiability, utility, parsimony). Meredith (1993) advocates the use of conceptual modelling and methods in theory building. Fisher (2007), looks to "[..] the prospering fields of physics, medicine, and finance" (p. 455) and suggests to strengthen the empirical base of OM through various research strategies. One such strategy involves identifying and verifying important phenomena (case studies), identifying and characterizing important questions on which we can do useful research (principles, hypotheses), validating models and assumptions that we have made (econometrics, statistics), and establishing the relevance of our research by demonstrating how the research outputs apply to practice. In a similar vein, Slack *et al.* (2004, p. 385) propose "[..] two different research processes, consolidation and application, that seek to reconcile the worlds of theory and practice".

Emerging questions

Many authors have sketched a rather bleak picture of the field of OM. However, is the situation really so dire? On the one hand it is normal and helpful for any discipline to worry about its status and shortcomings. Since Buffa's (1980) editorial, several special issues on the "state-of-the-theory", its empirical relevance and/or methodologies have been published, containing useful suggestions to advance the field. Furthermore, in spite of his critique, Swamidass (1991) gives a number of examples of general/grand/unified theories (general systems theory of OM, JIT principles, economic theory of the firm) and mid-range theories (waiting line, product-process matrix, plant focus, production competence). Many of these theories have not been rigorously tested, though. Schmenner and Swink (1998) use the theory of swift, even flow and the theory of performance frontiers "[..] to illustrate […] how such theories and their related laws might be developed" – and dismiss the product-process matrix as a theory in the process, positioning it as "an insightful framework" (p. 105) instead. Gupta and Boyd (2008) suggest that the theory of constraints can serve as a general OM theory. Pilkington and Fitzgerald (2006) recognize the weaknesses of OM – "[..] the failings of theory construction […], the lack of empirical validation […] [and its reluctance] to adopt ideas and methods from other subjects", but also conclude that (*IJOPM*) publications are beginning "[..] to break down the once noticeable isolation of sub-fields […]. They show a greater integration of key concepts, a more subtle appreciation of context, and more rounded evaluation of specific practices" (p. 1268).

On the other hand, the very same concerns are coming back time and again, suggesting that the underlying issues have not been resolved. As recently as 2011, DeHoratius and Rabinovich summarized these concerns. Aiming "[…] to give readers a renewed appreciation for the use of field research in operations and supply chain management", they call for "[…] research that is not only grounded in practice, and thus managerially relevant, but that also strives to make substantial theoretical contributions […]" (DeHoratius and Rabinovich, 2011, p. 371). Heyl, paraphrasing Schmenner in Schmenner *et al.* (2009): "We do have a flawed approach to theory, attempting to build new wings on existing structures that should have been long ago demolished while applying the newest methodologies to address issues of marginal interest or importance" (p. 342). And Amundson (1998, p. 341): "OM lags disciplines such as sociology and economics in the creation of formal, research-oriented theoretical perspectives".

The fundamental questions that seem to emerge from this discussion seems to be: do we need an OM theory or can we borrow from other disciplines? If we need a theory of our own, do we have one and, if not, how do we develop one? The contributors to the remainder of this paper approach these questions from different angles and offer different answers.

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3. What theories do we have in OM?

By Chris Voss

It has often been stated that OM has only a limited number of theories and must rely on those drawn from other disciplines such strategy, economics, and behavioural science. Whilst theories from other disciplines are widely used, this is clearly underselling the OM discipline. It does not take too much examination of the related literature to find a whole range of, often high level, theories. Here we explore a number of these theories and through this illustrate how contribution to existing theory might be made.

OM problems with explicit theoretical foundations

Let us examine some "explicit" theories that can be seen to explain or inform a particular phenomenon. The phenomenon of lean production, an area made up of multiple theories is widely considered to increase productivity. The theory of "swift even flow", was put forward by Schmenner and Swink (1998), in part to give a sound theoretical basis for the relationship between lean production and productivity, but it is put forward as a more general theory. "The theory states that 'the more swift and even the flow of materials through a process, the more productive that process is" (Schmenner and Swink, 1998, p. 102). It argues that productivity rises with the speed of flow of materials through a process, and reduces with increases in the variability associated with the flow.

"Queuing (waiting line) theory" is one of the longest established theories in the field. Originally developed by Erlang in 1909, it provides a mathematically based explanation of queuing phenomena, which occur in virtually all operations contexts. Like many theories, it requires assumptions about behaviour, in this case the variability of both arrival and service, arrivals typically seen as a Poisson process. After its simple origin, scholars have developed it so that it can explain queuing phenomena in a wide variety of contexts from restaurants to contact centres; from single lines to networks.

A theory, which was publicised by an unusual method – a novel, The Goal, is the "theory of constraints" (Goldratt and Cox, 1984). The theory states that every process has a single constraint (bottleneck) that stands in the way of achieving the goal of improving profit. Management should focus on systematically improving that constraint until it is no longer the limiting factor as only improvements to the constraint will further the goal. The theory of constraints defines the steps that managers can use to manage constraints, thereby increasing profits.

The "theory of performance frontiers" was put forward by Schmenner and Swink (1998). A performance frontier is the maximum output that can be produced from any given combination of inputs, given their technical considerations. An organization will have two types of performance frontiers; an asset frontier that represents the maximum performance under optimal asset capability and utilization, and an operations frontier which represents the achievable performance under the current strategies and policies. An organization can improve its performance by improving its current process to approach the operations frontier, or by betterment, moving the frontier forward.

A final example of an explicit theory and our first illustration of how theories can be developed and built upon is "competitive progression theory". One way of contributing to theory is through strengthening the theoretical base. When it was first published the "sand cone model" put forward by Ferdows and De Meyer (1990), became an instant classic. It argued that to get "lasting improvements in manufacturing", capabilities were cumulative and needed to be put in place in a certain sequence, each one building on the other, hence the sand-cone metaphor. However, despite is attractive face validity and eloquent argument, there were many criticisms based on potential conceptual problems and the poor empirical justification (which was recognized by the authors). Given its attractiveness and originality, a number of authors sought to test and develop the theory more strongly and to develop a stronger empirical view of cumulative capabilities. An example is the work of Narasimhan *et al.* (2005), who conducted an empirical study of "capability progression". This was taken further by Rosenzweig and Roth (2009), who developed and tested the concept of "competitive progression theory". The theory contends that sustainable competitive capabilities are built cumulatively, from quality to delivery reliability, to process flexibility, and to price leadership.

OM problems with multiple theoretical foundations

There are many major areas which embody or are made up of multiple theories. Some examples include operations strategy, lean operations, total quality management (TQM), and bullwhip effects. One of the core theories of operations strategy, performance frontiers, has been described above. Much of the theory of operations strategy is concerned with alignment between operations choices with factors such as process, volume, variety, and competitive priorities. These theoretical product-process relationships have been articulated in Hayes and Wheelwright's product-process matrix and its equivalent in services (e.g. by Silvestro *et al.*, 1992, and others).

Lean operations, sometimes called just "Lean", originated as a term used by Womack *et al.* (1990) to describe what was then called "JIT" or "Toyota Production System". Over the years there has been both considerable development and exploration of the theory. The theoretical foundation of how we understand lean operations is clearly made up of numerous individual theories, all of which need to be understood, in order to understand what is a high-level theory. We have already discussed one such theory, "swift even flow". There are numerous others such as pull scheduling, which itself has a theoretical base in factory physics. Closely associated with lean operations is TQM. Whilst TQM itself is a broad and contentious concept with both support and many doubting its concept and effectiveness, it has attracted much attention in exploring its theoretical base (Handfield and Melnyk, 1998).

Our final example is the bullwhip effect. This provides an excellent example of ways of contributing to OM theory. The bullwhip effect has been known to exist in manufacturing for a number of years, being initially described and explained over half a century ago (Forrester, 1958), and receiving its present label in 1997 (Lee *et al.*, 1997). The theory explaining the bullwhip effects states that it occurs when, in a chain of interlinked process stages, the variation in the demand pattern coming out of a process stage is greater than the variation of the demand that came into that process stage. Moreover, this process of amplification is repeated from process stage to process stage. The "bullwhip effect" also provides two interesting examples of further contribution to a well-established theory: Anderson *et al.* (2005) examined the dynamic behaviour of service supply chains in the presence of varying demand and information sharing. Using data from two service supply chains in telecom, Akkermans and Voss (2013) set out first to validate the work of Anderson *et al.* (2005), and then to explore any distinctive root causes of bullwhip effects in service supply chains.

Contributing to theory

Whilst we have illustrated the development of a number of theories, as in any discipline, developing a new theory is difficult and research in any management discipline tends towards contributing to existing theory, or using existing theory to explain phenomena, rather than developing new theory. One of the most common ways of contributing to

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existing theory is through contingency approaches. The theoretical background of contingency approaches in OM is described in Sousa and Voss (2008), in the context of OM practices. The use of contingency approaches is widespread, even when not explicitly acknowledged. Some of the theoretical relationships described above such as the product-process matrix are contingent relationships. Contingency theory can be seen as a theoretical lens used to view organizations, for example, this theory holds that organizations adapt their structures in order to maintain fit with changing contextual factors, so as to attain high performance. Theoretical and practical contributions of this approach are achieved by identifying important contingency variables that distinguish between contexts; grouping different contexts based on these contingency variables; and determining the most effective internal organization designs or responses in each major group" (Sousa and Voss, 2008, p. 698). Typical questions addressed in an OM context include: what processes and practices apply in which contexts, what relationships hold of do not hold in which contexts and where do methods work and do not work or how do they vary in different contexts.

The above is in addition to the most widely used method of contributing to existing theory, especially when survey research is used: to examine and test relationships. At its simplest it testing the predicted relationship that "A leads to B". In reality, most relationships examined are more complex, with tools such as regression and more complex analysis techniques. In many situations it is often more interesting to explore the moderators of these variables; moderating variables that influence the strength of the direct effect between two independent variables. Moderating effects inform our understanding of those core theories and their contingent relationships.

4. Do we actually need theory?

By Roger Schmenner

The OM literature is rife with allusions to theory and purported tests of one theory or another. Does our discipline actually need all of that? The short answer is "no", not if we are truly trying to be scientific about the study of operations issues. There is nothing in science – and, particularly, nothing in the hard sciences that social science so longingly tries to emulate – that insists on theory. Consider the definition of the scientific method, per the Oxford English Dictionary: "A method of procedure that has characterized natural science since the 17th century, consisting in systematic observation, measurement, and experiment, and the formulation, testing, and modification of hypotheses." Nowhere is theory mentioned. Rather, doing science is about "observation, measurement, and experiment" and about the "formulation, testing, and modification of hypotheses".

Hypotheses are different from theories. Again, consider the Oxford English Dictionary definitions for hypothesis:

H1. A proposition made as a basis for reasoning, without the assumption of its truth.

H2. A supposition made as a starting point for further investigation from known facts.

Hypotheses are guesses that guide research, and that research is full of observation, measurement, and experiment. The guess is then revised and the cycle continues.

Theories, on the other hand, explain facts and provide stories as to how phenomena work the way that they do. Theories are invented; they are not built (i.e. forget about theory building; it does not exist). Our understanding can be built up from hypotheses and their tests, but the theories that explain what the hypotheses have shown are inventions that often come years after the facts have been settled. Theories should

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make predictions and they can be disproved by findings that run counter to their predictions. Theories cannot be proved, though; they can only be supported by the evidence.

Discovery, prelude to theory

The implication of this argument is that we researchers can, and should, advance our understanding of OM by doing "discovery" and not simply by trying to advance some theory or another. Discovery is prelude to theory, and just as important. The phenomenally rich history of science at the University of Cambridge is testament to the importance of discovery and shows clearly how theory lags discovery often by years and years. Table I lists some of Cambridge's renowned contributions to science, with the Cambridge scientist indicated in bold. Note how the theory lags the discovery.

Professors J.J. Thomson and Ernest Rutherford of the famous Cavendish Laboratory were pioneers in the exploration of the atom, identifying first the electron and then the nucleus of the atom. They could not explain why those oppositely charged particles could be separated within an atom, but their work laid the foundation for Bohr's revolutionary theory of how the atom is structured.

Faraday and others had investigated electromagnetism and had discovered many of its properties, but it remained for the brilliant Clerk Maxwell to develop a theory of electromagnetism that predicted all sorts of things, including radio. Likewise, Kepler and others had identified how planets moved in our solar system and it was widely known that mass and distance were involved, but it was left to Newton to lay out precisely how gravity worked to explain the particular orbits of the planets.

Charles Darwin was both discoverer and theoretician. His careful collecting and note-taking aboard the Beagle left him scratching his head about the mechanism of evolution until his reading of Malthus in 1838 provided the trigger to a full-blown theory to explain what he had observed. Even the more modern development of the double helix structure of DNA was dependent on the earlier X-ray crystallography of Rosalind Franklin.

Has OM had discoveries?

All this may make one wonder whether OM has seen its own discoveries. Of course it has, but we have been reluctant to recognize them as such. Consider the following:

- Specialization of labour: we generally attribute the observation of labour specialization to Adam Smith in his classic 1776 book, *The Wealth of Nations*. However, Smith's observation was really taken from Diderot's *Encyclopaedia* published in 1772. The pin factory was in Normandy. Smith missed two better pin factories in Britain.

Discovery	Theory
J.J. Thomson (1897) – electron	N. Bohr (1913) – model of the atom
E. Rutherford (1909) – nucleus of the atom	
M. Faraday (1838) – lines of force	J. C. Maxwell (1865) – theory of electromagnetism
J. Kepler (1605) – planetary motion	I. Newton (1687) – theory of gravity
C. Darwin aboard the Beagle (1831-1836)	C. Darwin on reading Malthus (1838), and later
R. Franklin (1952) – DNA X-ray crystallography	F. Crick and J. Watson (1953) – Double helix structure of DNA

Table I.
Discovery vs theory

- The factory: concurrent with Adam Smith was the development of the first true factory (for cotton textiles). Richard Arkwright put together water frames and carding machines in a single building with a central source of power (water), labour, and management oversight. The resulting huge leap of productivity signalled the start of Great Britain's Industrial Revolution.
- The moving assembly line: the development of the moving assembly line at Henry Ford's Highland Park factory in Detroit during 1913-1914 is justly famous. Clarence Avery and others of Ford's engineers completely re-imagined how an automobile could be assembled and what it would take to make it happen.
- Combatting the bullwhip effect: shortly after the development of the moving assembly line, in 1919, Richard Deupree, then the Sales Manager for Procter and Gamble, persuaded his management to remove the wholesalers in P&G's soap supply chain. They, he realized, were the ones causing vast fluctuations in the orders placed on P&G's Ivorydale plant. The fix required that Procter and Gamble create its own distribution system, but that innovation eliminated the bullwhip effect and was soon copied by many other high-volume consumer products companies.
- Just-in-time manufacturing: by now, the story of Taiichi Ohno and Toyota is familiar. Ohno and his team recognized how mass production could be re-envisioned and productivity greatly enhanced.

All of these dramatic advances in operations required insight and lots of experimentation. Were they based on theories then existing? No. No formal theory guided these stunning initiatives. Our understanding of why they work as they do has come much later.

Implications for OM research

The upshot of this discussion about the history of science and the history of operations is that our discipline of OM could perhaps benefit greatly from more observation and discovery and a less slavish attention to theory. There is much still to do. There are operations problems that companies face all the time that need continued observation, measurement, and experimentation. For example, why do companies backslide and fail to continue the progress that they have made? Is there a best set of performance measures that an operation should adopt? Is there a best way to set up a company's supply chain? One could go on.

Where does that leave theory? Perhaps we should have the courage to kill off those theories that are not supported by the evidence we gather, rather than letting them linger to be cited yet again. Perhaps we should take pains to modify them. Perhaps we should do a better job of defining their boundaries and applicability. Leaving theory on a pedestal is not the way forward.

By being less tied to theory, OM might just become more scientific.

5. Should we borrow theory or do we have theory "of our own"?

By Matthias Holweg

It was a passing comment by my colleague Andy Neely that reinvigorated my interest in the way we use and build theory in our field: he had noticed an intriguing dichotomy between two recent books, *Great Minds in Management* by Smith and Hitt (2005), and *Giant Steps in Management* by Mol and Birkinshaw (2009). While the latter in its very first chapter lists no less than nine fundamental innovations in management that anyone would immediately recognize as "OM" concepts (of a total of 40 across all

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management disciplines), the former does not feature a single "great mind" with an OM background amongst its 24 management theorists. While it seem clear from this comparison that OM has a lot to say to practice, the root cause for its apparent underrepresentation in "management theory" is far less clear.

Why not borrow what we need?

What is clear though is that OM has few theories that it can call its own, so OM research frequently borrows management theories borrowed from organizational behaviour, strategy and economics: transaction cost economics, the resource-based view and the notion of dynamic capabilities, as well as information processing, adaptive structuration theory, but also others derived from the sciences, such as evolutionary theory, network theory, systems theory and complex adaptive systems. As Morgan Swink commented at EurOMA 2009: "[…] we have few things that we call our own [theory in OM]." It is astounding that all OM research requires theory, yet that we acknowledge none of our own devising. One could argue that borrowing theories from other fields is both sufficient and simply a necessary response to the predicament the field faces: having to contribute to theory to get published, while not being able to draw on a standard set of theories from the field. Yet there are three specific dangers in this approach.

For one, it can lead to circular arguments. Take, for example, the concept of dynamic capabilities (Eisenhardt and Martin, 2000). Here OM researchers might look at high performing firms, isolate them from the population of other firms to identify their distinguishing features, which are subsequent labelled as "dynamic capabilities". It is then argued that these dynamic capabilities, which are likely to include operational practices such as JIT, Lean, TQM, and Six Sigma, lead to higher performance. Not only is this a potentially circular, possibly even tautological, argument – but of what actual explanatory value is this for OM as a field? We have long known, and empirically proven, that Lean practices, for example, lead to superior performance. So what exactly is the incremental learning (or predictive value) that we in OM derive from re-coding them as "dynamic capabilities"? A cynical outlook might lead one to argue that theories from other fields all-too-often serve largely to assist publication in certain journals, and/or to be recognized by other management scholars. The latter of course hardly take notice of what is being written in OM journals about their theories.

A second real danger in adopting theories from other fields is that while these theories may be valid in their respective domain, by exporting them into a new context (namely, the operational side of the firm) they lose validity. In other words, one cannot assume that a theory that works in one context also works equally well in another. Theories can be borrowed across social contexts, and across levels of analysis, and it is not given that it retains its context validity (Whetten *et al.*, 2009). This is a particular danger when borrowing theories from fields in the social sciences other than management. It is certainly possible to borrow a theory and using it effectively – but one needs to always bear in mind that borrowing bears the danger of compromising its explanatory power. Similar to Mark Pagell's comments later in this paper on the need to understand and state the "boundary conditions" of a theoretical contribution, I see this as one of the main reasons why the theory section in OM papers often seems "bolted on": it may broadly fit with the problem at hand, but overall few (if any) new insights into the actual problem studied are brought forward by adopting it as theoretical foundation of the paper.

The third danger lies in the actual contribution. The underlying purpose of our research is to make a contribution to theory. That is how our field moves forward. Yet all-too-often new theory is proposed, but then never tested or refuted. As a result

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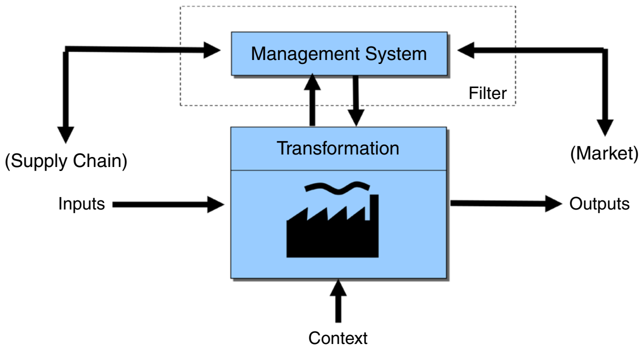
we have journals full of papers that all claim to make novel contributions to theory – most of which stand uncontested, and sadly, unnoticed. As Linderman and Chandrasekaran (2010) show, OM is one of the disciplines that is more open to a scholarly exchange with other disciplines. Many other disciplines however are not. This in turn means that claiming a contribution to a theory outside of the OM context may well be a mirage as the theory-building and-testing cycle remains unclosed.

Why not use what we (implicitly) already have?

It is obvious why OM scholars seek to adopt theoretical foundations from other fields: the dearth of theory of our own and the need to demonstrate this elusive contribution to a theory of relevance to get published are the obvious drivers of behaviour. Yet it is important to note the inherent dangers in this approach. The aforementioned assumption of contextual validity when transferring a theory from one context to an OM one commonly is taken for granted. I would argue that herein lies the root cause that so many literature reviews in OM papers appear disconnected from the core of the research. Instead I would encourage OM researchers to go "back to the roots", and consider their research problem in the context of the very same "input-conversion-output" process model that underpins virtually all we do in OM. Figure 1 shows this simple process model as it would appear at the start of virtually any OM textbook. All operations are composed of processes; a process is the sequence of operations and involved events, taking up time, space, expertise or other resources, which lead/(should lead) to the production of some outcome. As such it provides the raison d'êtrefor OM as a discipline: to derive and implement improvements to this conversion process. It also introduces our very core concepts – efficiency and productivity – as key ratio of outputs over inputs (productivity), and its change over time (efficiency).

This model allows us to make predictions about the performance of any process by linking inputs to outputs expected by the customer. If, for example, the variability in inputs increases, the productivity of the process is likely to decrease, and so on. The model can also be directly applied to ground the very few attempts of generating OM theory, such as Schmenner and Swink's "Swift Even Flow", "Performance Frontiers", Ferdows and De Meyer's "Sand Cone" model, and Hopp and Spearman's "Factory Physics" (Schmenner and Swink, 1998; Ferdows and De Meyer, 1990; Hopp and Spearman, 2000). Indirectly the process model also underpins many of our key



Source: Holweg *et al*. (forthcoming)

Figure 1.
The process model

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concepts and methodologies, such as Lean and Six Sigma, for example, in terms of reducing lead-time and undesired variation.

What is striking is that – despite its implicit prominence – the process model is hardly ever used as explicit theory in OM research. This is despite the fact that it offers great predictive value about the nature and behaviour of processes: we can make predictions about how a process performs in the light of variation, delays and multiple handoffs, and study the behaviour of connected processes under the notion of "supply chains" (Holweg *et al.*, forthcoming). However, instead of claiming it as our theoretical grounding, we seek refuge in theories outside of our field. Maybe the root cause for the predicament that Harry Boer outlined at this start of this paper is that we as a field are lacking the confidence to claim and develop our own theory? In my view it is the very basic process model that we all teach to our students that also provides the true and forgotten theoretical foundation of our field – the theory that is our very own.

6. How to make a theoretical contribution

By Martin Kilduff

What do we mean by a theoretical contribution in OM or in any scientific field? This question is fiercely debated (see the review in Kilduff *et al.*, 2011). The good news is that you can (and should) craft a theoretical contribution according to your stance on fundamental issues of meaning and knowledge in science. First, do you think that scientific theories represent reality or not (the ontological question)? Second, do you think scientific theories get closer and closer to the truth over time (the epistemological question)?

Let us say you answer "yes" to both questions: you think scientific theories represent reality and that these theories move closer and closer to the truth. This defines you as a realist who believes that scientific theories aim to provide true descriptions of the world, including the world that lies beyond observable appearances. Thus, from a realist perspective, scientific terms (such as "utility function") themselves may reference actual things in the world, and scientific theories replace each other by offering better accounts of scientific objects (Putnam, 1975, 1987). Research from this perspective aims to discover fundamental truth about nature. A contribution to theory consists of a better or more inclusive explanation of phenomena in the world, often couched in mathematical language. For an example of this realist, pure research endeavour (and all examples in this section of the paper come from the same source – *Academy of Management Review*), see Makadok and Coff, 2009. A variant on this realist perspective, specifically designed for social science, is *critical realism* (Bhaskar, 1998) with its emphasis on the exposure of hidden realms of reality and the emancipation of people from hegemonic tendencies (e.g. Mumby and Putnam, 1992).

But let us say you answer "no" to both questions concerning ontology and epistemology: you think scientific theories are useful instruments in helping predict events and solve problems, but you accept no claims concerning theory's correspondence with reality or its approach to the truth (cf. Cartwright, 1983). This defines you as an instrumentalist, a.k.a. a pragmatist (Laudan, 1977). Your contribution to theory consists of a better predictive framework, model, or theoretical tool that helps solve an empirical problem even if the framework incorporates wildly inaccurate representations of reality (cf. Friedman, 1953). (For one example of instrumentalist theorizing – that does not involve improbable assumptions – see Mayer *et al.*, 1995.)

But what if you agree that science progresses towards truth while rejecting any claim to reality by unobservable entities? This would put you squarely in the foundationalist camp (e.g. Ayer, 1952; Carnap, 1928) of those who emphasize empirical

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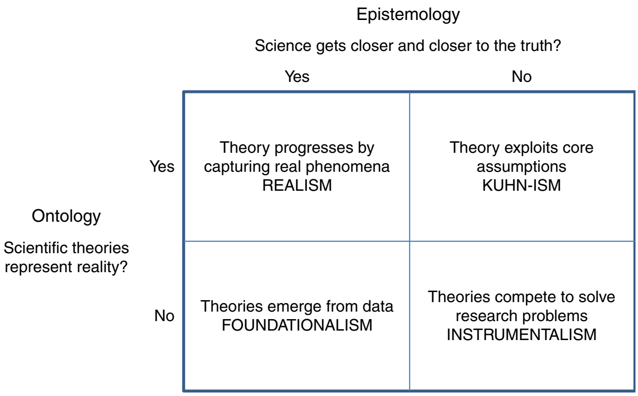
data gathering from which scientific knowledge emerges inductively (see the review in Kleindorfer *et al.*, 1998). This position has also been called the "received view" (Suppe, 1972) given its widespread popularity prior to the consensus-shifting work of Thomas Kuhn (1996/1962). If you are a foundationalist, your contribution to theory consists of a new theoretical pattern or approach that emerges from empirical data gathering (e.g. Van de Ven and Poole, 1995).

But what if you accept the actuality of the physical world, including unobservable aspects, but are sceptical concerning the claim that science progresses to an ever-closer approach to truth? You would find yourself aligned with Thomas Kuhn who had no doubt there was a real world, but who recognized that each successful scientific paradigm defined what aspects of this reality scientists attended to, changed, and adapted. From Kuhn's perspective, you could not step outside of history to evaluate truth claims from a paradigm-free, objective perspective (Kuhn, 1996/1962). A theoretical contribution from this perspective includes the creation of a new paradigm linking new phenomena to new empirical inquiry (e.g. Hambrick and Mason, 1984).

Figure 2 summarizes these four perspectives.

So, you are ready to make your case for a theoretical contribution according to one of these perspectives. But there is another important decision of vital importance to how your claim will be received: whether to cast your contribution as shifting the consensus of the scholarly community away from an accepted position; or whether to cast your contribution as the creation of a new scholarly consensus where one did not exist before (Hollenbeck, 2008). If the paper is purporting to be consensus-shifting, the argument goes something like this: "It has long been thought that […]. But this is false […]. We've seen instead that […]. New investigation is necessary to […]." By contrast, a consensus-creating paper follows this formula: "There is a clear lack of consensus regarding…. The different approaches can be merged into a new understanding […]. Bringing together these different traditions opens up new areas of research."

My impression is that consensus-creating articles accumulate more citations than consensus-shifting articles. This might seem surprising – surely a contribution to theory that fundamentally changes how we view the field should garner more attention than a contribution to theory that involves bringing fragmented approaches together? But scientists do not like being told they are wrong. Consensus-shifting papers,



Source: Kilduff *et al*. (2011)

Figure 2. Matrix of philosophy of science approaches and associated logics of action

I suspect, not only fail to garner many citations, but may prove difficult to publish given that the theory claim is based on criticizing the very people who are likely to review the paper.

Finally, what other approaches to theory contribution are unlikely to succeed? Top of the list are critiques of existing theory without the provision of alternative theory. Second are claims to have found gaps in the literature – such gaps are necessary characteristics of theory contribution but are insufficient absent a compelling and outstanding problem. Third, are models of specific problems without generalizability – for example, a solution to factory management specific to a plant in Eastern Scotland.

7. Crafting a good theoretical argument

By Mark Pagell

It is somewhat ironic that in a practice-oriented field with a focus on waste reduction that our typical paper is often thousands of words longer than papers using similar methodologies and addressing overlapping questions that are published in other equally "practical" fields. And this extra girth is almost all in the theoretical development and implications sections of papers; sections that are often of little value.

This is not to say that theory is not important; developing good theory and being able to answer how and why questions are the largest contributions we can make. But to paraphrase others (Schmenner *et al.*, 2009) far too often the elaborate theory development sections in our papers are a cover for doing tests that were never theory driven and need not have been. Hence, I start from the position that frequently the best theoretical argument would be little or none. Our lexicon acknowledges only theory-building and theory-testing research. Yet most of what we do is neither and is instead aimed at identifying important new facts (fact building) or testing the validity of previously identified facts (fact testing). The contribution of these papers, which if we are honest, are the overwhelming majority of what we write, will be a collection of new facts. If the facts are important and have been discovered in a robust manner they should be published.

Medical researchers of course want to know how and why a medication works; they want a theory to explain a medication's efficacy. Yet, it is unimaginable that the fact that penicillin or any other drug works, would remain unpublished until the why was fully understood. Facts alone are important contributions. These facts will over time accumulate and help in the development of new theory or the falsification of existing theory.

My ideal world then starts with an assumption that most research will be building or testing facts, not theory. Such research should not be burdened with bloated and often pointless sections on theory development. The literature review/theoretical development sections of these papers would be a concise explanation of what is (un) known, to justify why there is a need for further evidence of a fact's existence or validity. This vital work forms the core of normal science (Kuhn, 1996) and we could do far more of it if we were not spending so much time writing elaborate theory development sections when they were not needed.

This would leave the theory building and testing to a much smaller group of papers, where the theoretical argument would be critical. This is my ideal world. And I can follow this advice risking little more than frequent rejections with the hope that eventually the field will follow me. But the reality for most academics is a need to publish, now. And the field presently demands all research be theory driven. Therefore in an effort to reduce the waste in many of our papers I build on observations from being a reviewer, associate editor, guest editor and author (who to be clear has violated

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every one of the suggestions that follow) to try and explicate what makes a good theoretical argument, even in those cases where I would prefer none.

The application of what follows is more obvious for theory or fact testing research. Typically we treat qualitative research as both exploratory and theory (fact) building. Yet, qualitative research that truly starts with a small set of conflicting predictions with an aim of determining which is the best fit, is a form of theory testing and should be treated as such. In confirmation or testing research (regardless of method), building the theoretical argument mainly occurs in the paper's front end as part of a literature review.

The review sections in the remaining exploratory research, work that aims to build facts or in rare cases theory, should be short and pointed towards what the research is trying to learn. The goal of the literature review for exploratory papers should be to show:

(1) that the issue is of practical importance;

(2) what we do know about this or similar phenomena; and

(3) that what we presently know cannot explain the phenomena and hence we need new facts/theories.

The detailed exploration of theory and the theoretical argument in exploratory papers should be reserved for the discussion where the authors examine what new facts have been discovered and how (if) they contribute to our understanding of existing theory or in rare cases build new theory.

The key difference between building and testing research is then where the theoretical argument should mainly be developed, not how it should be constructed. Hence the suggestions that follow should be equally useful in developing good theoretical arguments for all papers. When done well, the theoretical argument is tied to the task at hand. A good theory makes precise predictions (e.g. Schmenner *et al.*, 2009) about relationships. Hence the relationships explored must align with the theory that the authors purport to be using. Exploring all possible predictions of a theory in a single study is often not possible, but this does not mean authors can ignore predictions that are germane to their work.

For example, if your theoretical argument is built around a contingency theory, the measures have to capture multiple contingencies, not just one. For instance, the prediction that in highly uncertain environments firms need more flexibility is only half of what theory predicts (i.e. Burns and Stalker, 1961; Lawrence and Lorsch, 1967). The other half of the prediction is that in low-uncertainty environments excess flexibility will be expensive and unnecessary. Yet research in this area often ignores the low-uncertainty/low-flexibility predictions made by the theory the authors claim to test (Pagell and Krause, 2004). Linking theory to the research seems obvious. However, based on my experience a great deal of the review process focuses on making the theoretical arguments in a paper align in a coherent fashion with the analysis and results, because in many papers the path from theory to measures to analysis to discussion and back to theory is not at all clear.

The next common issue is conflating an accumulation of facts with theory. Many authors justify their research using a plethora of only vaguely related citations, which are claimed as theory. Multiple previous findings that suggest a relationship are at best an accumulation of facts, and should be treated as such. If your goal is to confirm that A leads to B, a fact suggested by the literature and important to practice

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that should be your primary argument. If theory truly predicts this relationship as well, that is great and you are actually testing theory. But if not, you either need a different theory or none.

Another common problem is an over-abundance of theory. Some of the best research that truly tests theories starts by noting that 2 or at most 3 theories provide different predictions, and then actually tests which theory best fits the data. But papers of this nature are relatively rare. Rather than determining which theory best fits the data, and by implication which does not, many papers claim to test *H1* based on the predictions of theories A and B. *H2* is based on theories B, C, and D, while *H3* is built on theories A, B and, D. Yet, there is no coherent argument which would tell the reader how the acceptance or rejection of multiple hypotheses would translate into rejecting/rethinking the existing theories. And these papers often seem to be written thinking only of confirmation, with little thought to how the results could be used to falsify. If your theory development section references three or more theories or if different hypotheses/results are supported by different theories odds are that your theoretical argument is weak.

Poor theoretical arguments also tend to do a poor job with boundary conditions. Applying a theory of the firm, like the resource-based view, to a work group, plant, or function, requires careful thought as to what competitive advantage means to these sub-units within the firm. But too often the boundary conditions are missing and a broad theory is applied to a narrow setting or a narrow theory is stretched beyond recognition.

Our existing reality is a wasteful one. It requires that the authors of almost all papers develop a theoretical argument, when in many cases this is not needed. Hopefully as the field evolves we will become better at accepting that identifying and validating robust facts makes an important contribution, saving discussions of theory for where they are appropriate and can make an impact. That change cannot occur fast enough for me, but it would not change my suggestions for creating a good theoretical argument.

A good theoretical argument is linked to the data and builds on a small number of existing theories, preferably one or two, to make a coherent argument. The variables that are measured align with the relationships the theory predicts. Boundary conditions are clearly spelled out and there is a clear path from supporting or rejecting a hypothesis to the theory being used. Finally, a good theoretical argument makes it clear how results could be used to falsify as well as confirm.

8. Synthesis

In this paper we have presented six complementary, even if not fully congruent, views on the status and use of theory in OM research, what constitutes a theoretical contribution, and how to construct a theoretical argument. In the confines of a single paper we could not have hoped to do justice to such fundamental topics; still we hope that this paper will further the debate about the nature and use of theory in OM. In the following we summarize the five main points made in the viewpoints presented below.

8.1 Theory is fundamental to OM research

While we have argued above that the way in which theory is currently being used leaves much room for improvement, there is no doubt that theory is the fundamental engine that drives the creation of knowledge. This unequivocally also applies to an "applied" discipline such as OM. Let us take another applied field, medicine, as an analogy: clearly curing one patient from malaria is an accomplishment, yet the greater contribution to society is to develop a cure for the 200+ million people in the world who

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also suffer from malaria. "Doing empirically interesting work" or solving a "real problem" a particular firm faces is of merit, but it is generalisation that determines the theoretical contribution of the work. Theory is the currency in which researchers trade, so just "doing good work" or "solving a problem" is not enough. In fact many scholars will balk and consider it heretical to even ask whether we need theory, since theory creation and testing is the fundamental way by which our, and any other, field advances. The underlying cycle that tests, expands upon or refutes the aspects and implications of theory is the most fundamental mechanism that social science has to develop knowledge, and move the field forward. Theories explain facts and provide explanations as to how phenomena work the way that they do. They can, and should, be used to make predictions. In turn, theories can be disproved by findings that run counter to their predictions or explanations, to make way for better theories (Hempel, 1965, 1966). Hence hypotheses as such are not theory, they represent concise statements about what is expected to happen. Theory explains why something is likely to happen (Sutton and Staw, 1995); predictions without the underlying causal logic as to why something will happen are not theory (Weick, 1989).

There can be two fundamental ways in which research contributes to theory: exploratory studies that observe and identify interesting, relevant, and potentially counter-intuitive phenomena that cannot be explained well enough by existing theory, and thus propose these for further testing These studies seek to discover new phenomena and develop hypotheses. Second, there is confirmatory work that puts these propositions to the empirical test in a given context to either refute, amend, expand, or confirm these as new theory, and to define its realm of its applicability. These studies seek to test theory and describe its application in practice. Both are equally valid ways of contributing to theory. Contributions can also come in less formal ways, for example, by pointing out flaws in an existing theory when applying it to a new or different context (even without necessarily proposing a better one), or by identifying areas where the literature so far fails to address important and relevant issues.

8.2 Theory is not the inevitable starting point

Theory neither needs to strive to represent reality, nor should it be the default starting point of academic research. Observing interesting phenomena is an equally important starting point. The often-cited "empirically interesting findings" may not constitute theory as such, however, the very reason why they appear interesting is of course that they challenge our existing models and theoretical constructs. Thus, the very notion of what makes it interesting needs to be developed further in the context of the existing literature or understanding that it contradicts or challenges. As Weick (1989) argues: "An assessment of interest represents the terminal stage of a substantial comparison between previous experience summarized into an assumption and a current experience summarized into a conjecture which questions that summary. The reaction that's interesting essentially signifies that an assumption has been falsified." (p. 529). Challenging, critiquing and even refuting existing theory would constitute a valid, and valuable, theoretical contribution.

In this context it is interesting to note that all but one of the OM innovations Mol and Birkinshaw (2009) list were developed by companies, not academics. It is for a good reason that "gemba" (i.e. "the workplace") and "genchi genbutsu" (i.e. "go and see") are fundamental building blocks of the Lean Production logic. "Go and see" should play a more prevalent role in OM research, in our view. As Luk van Wassenhove (in Schmenner *et al.*, 2009, p. 342) points out, "[…] why make up problems when the world around us is full of fascinating and crucially important problems that beg for some elementary insights? [..] We need answers to pressing problems, not more theories or methodological scrutiny. The field is called OM, not mathematics." All too often we encourage our new doctoral students to start with a literature review rather than exposing them to the "messy" problems of the real world.

8.3 There is no one right way to making a contribution – what matters is consistency

The OM research landscape covers a broad set of topics and features a diverse set of epistemological perspectives (e.g. Optimization of a vehicle routing schedule or creating high-involvement work systems). So hoping for "one right way" would be naïve. Instead we should embrace and cherish this epistemological diversity. The key to making a valid contribution is thus not to strictly follow a certain set of criteria (as set out, e.g. by Whetten (1989) and Wacker (1998)) but to be consistent: what are the ontological and epistemological perspectives taken? This will determine what a valid contribution is. From a realist point of view, developing a theory that better explains reality is a valid theoretical contribution. Most empirical OM research takes such a realist perspective, aiming to discover fundamental truths about nature. Here a valid contribution to theory would consist of a better or more inclusive explanation of observed, or observable, phenomena. But this is only one of many equally valid perspectives. Consistency in defining the research and its objectives in relation to the ontological and epistemological assumptions that underpin the research is the key. We have outlined four major philosophical stances above and regard all as equally valid and applicable in the OM context.

8.4 The choice of theory is critical

A common misunderstanding is that, in order to contribute to theory, one has to choose one of the "major" management theories, such as the resource-based view or transaction cost economics. A resulting mistake is attempting to contribute to high-level theories, rather than focusing on the mid-range or focal theories that are more specific, and possibly relevant, to the OM problem at hand. In this context we consider "high-level" theories as those theories that are being used across all fields management (such as Transaction Cost Economics, the resource-based view, Institutional Theory, or the like); "mid-range" theories that make general predictions within a given context (such as the Sand-Cone model, Swift Even Flow, or Performance Frontiers); and "focal" theories as those that make specific predictions within a prescribed context (such as Little's Law and Kingman's formula).

We argue that it seems virtually impossible to make a significant contribution to high-level theories that are so fundamental in explaining how businesses operate that they are almost tautological. OM papers that make such claims not surprisingly often struggle make a convincing case in this regard. Rather than focusing on high-level theories, it is equally valid to contribute to mid-range and focal theories, which are often context-specific to OM. The "sand-cone model" (Ferdows and De Meyer, 1990), for example, is a mid-range theory that explains the improvement mechanisms and performance implications of a manufacturing operation. A contribution of equal value can be made to focal theories as well. Consider the Kingman formula (Kingman, 1962) that estimates waiting times in a single-server queue: it offers the ability to predict fulfilment lead-times in manufacturing and service operations alike. The focus is often wrongly set on high-level theories, which leads to disconnected and redundant literature sections, as well as a plethora of theoretical "contributions" that remain

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largely unheard and unchallenged. Furthermore, as virtually all of these high-level theories are borrowed from other fields, the generally ignored assumption of contextual validity remains a perennial and often ignored concern. What value lies in presenting a contribution to a theory in another field, if those respective scholars will never know about it?

Whether or not the contribution made is actually meaningful of course lies in the eyes of the beholder. Theory testing, theory building, critiques of existing theory, or simply describing a new phenomenon observed in the real world all have a valid place in research, and can all be contributions in their own right. Ultimately, the value of any theoretical contribution will be determined by its utility in informing practice and/or future research.

8.5 Use theory parsimoniously, yet with confidence

While there are no general guidelines on how to make a theoretical contribution, there are some general guidelines on how to construct a good theoretical argument. As most senior editors will agree, in a theory-testing setting the argument(s) should be closely linked to the data and build on a small number of existing theories (preferably one, or at most two) to make a coherent argument. More is not better, and using a barrage of references as a "[…] smoke screen to hide the absence of theory" (Sutton and Staw, 1995, p. 373) is not a viable strategy. Second, the variables measured should align with the relationships the theory predicts. Third, boundary conditions should be clearly stated so that it is clear to what degree the data supports or does not support a given hypothesis. The claim should be made with confidence but not overstate what can be said from the data. All too often authors make sweeping theoretical statements when they are testing a small bit of the space the theory covers. Finally, a good theoretical argument makes it clear how the results could be used to falsify as well as confirm. In other words, there should be no ambiguity as to what a confirming or rejecting result would be.

Note

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