Operations management theory: a four-level framework for next generation operations

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Abstract
A four-level framework developed in philosophy of science is used to review recent operations management research. Findings reveal that operations management research still bypass key observations and taxonomic steps of scientific inquiry. This blocks research that can move past middle range theories and that can engage with more abstract theoretical levels and also stops the development of theories specific to the field of operations management. We recommend adopting the 4-level framework to get rid of such 'bad habits'.

Keywords: operations management, theory, philosophy of science

Introduction

Despite a long-term interest in operations management theory, from Swamidass (1991) through to Stratton *et al*. (2016) and through Schmenner and Swink (1998), it cannot be said that operations management researchers share a strong theoretical identity and research agenda. The fuzziness of the real theoretical scope of operations management as an academic discipline can be linked to rather bland accounts of the discipline (e.g. Meredith, 2001) and questions about the boundaries of operations management and whether or not it is a discipline with a declining core (Slack, 2005).

In this paper, we use a four-level framework originally designed to evaluate claims to scientific explanations made in biology (Griffiths, 1994) to describe different types of operations management research but also different levels at which operations management theories can be investigated and formulated.

Theoretical Framework

It can be very challenging for researchers to critically examine whether or not the research which they produce is useful. Increasingly academics can be torn between two different interpretations of this question: usefulness in terms of theory (i.e. making a original, valid, rigorous contribution to knowledge) and in terms of relevance (i.e. practical impact). This theory/relevance dichotomy is in fact an over-simplification of long-standing debates in philosophy of science about the right way to make a scientific contribution when researchers have to specialise. Research specialisation is required for performance, but it obfuscates the actual impact of a contribution as it can only be useful if it is picked up and utilised appropriately by another specialist researcher performing a differentiated task.

Griffiths (1994) provide such a discussion in the case of biology. His challenge is to assess how different research approaches in biology effectively explain natural phenomena. Griffiths proposes that research inquiry in biology can be explained with a 4-level theoretical framework.

The lowest level, *characterisation*, is about the actual physical workings of biological systems and is concerned with anatomy and physiology. Scientific work is often associated with this level only (and wrongly) as the discovery of mathematical laws derived from experimental observations. It is at this level that we model and understand the components or characteristics of the system that we study.

The second level, *systematisation* (from systematics, the science of classification) is about understanding the distribution of the traits and characteristics from the previous level over specimens from a natural historical perspective. Research here is about defining biological species and their historical relationships on the basis of the traits that they possess. These relationships are the results of the evolution of species.

In the third level, *functional classification and explanation*, the traits from the lower level are again considered but in terms of the function that they serve in general ecological theory (for example the flukes of whales and the tails of fish are functionally identical, to exert muscular force on a fluid medium, but are taxonomically distinct, Griffiths, 1994, p. 215).

The last and most abstract level, ecology and artificial life, is about explaining how the different species from level 2 co-exists and sustain life in an ecosystem through the functional strategies that they adopt to survive. This is the domain of population dynamics (e.g. Hawk-dove population demographic model) and of studies of adaptation explained by fitness differences within an ecosystem. Figure 1 illustrates the nested nature of this 4-level framework.

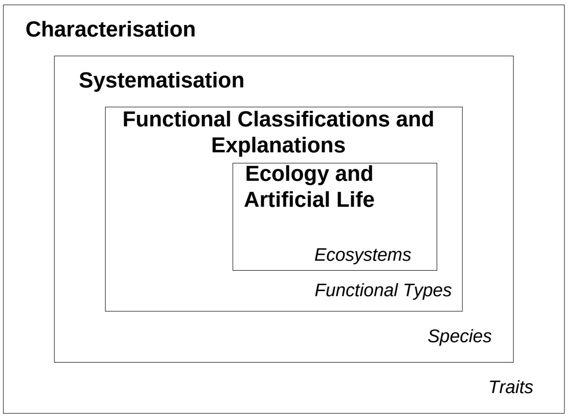

Figure 1. The 4-level framework of Scientific Explanation

From Biology to Operations

It may seem odd to use of a biological framework to investigate research in operations management. Yet Griffiths' framework has a general scope that covers all natural phenomena subject to evolution. Parallels between business/economics and biology are not new and Griffiths' framework can legitimately be used if the conditions of applications of the framework are appropriately customised and defined:

- The characterisation level is about defining the components of systems; this is a legitimate activity both for biological systems and organisational systems.
- The systematisation level is about classifying the different types of families of specimens, or species. McKelvey (1982, 1978) was the first scholar to call for a creation of organisational systematics, a genuine science of classification of business organisations. Operations management scholars actively pursuing this

research agenda exists but are far from the operations management limelight (Leseure, 2015; Rose-Andersen *et al*., 2009; McCarthy and Tsinopoulos, 2003; McCarthy *et al*., 2000; Leseure, 2000). All these publications replace the specifications of biological evolution with general evolutionary theory and explain at length how to classify 'organisational species'. Not all research at the systematisation level needs to be about taxonomies though and research about contingency theory (Sousa and Voss, 2008) and configuration theory (Boyer *et al*., 2000; Boyer, 1999; Meyer *et al*., 1998) are further examples of existing research at this level.

- The functional level is about explaining the impact that the possession of a trait of component confer to a system in terms of function and performance. This is a very standard form of business research.
- The ecological level is also a well established practice in business research and is usually associated with the work of Hannan and Freeman (1989, 1979) and Carroll and Hannan (2000).

In the next section, we use a debate about the relevance of Aggregate Production Planning (APP) to illustrate the application and implications of the 4-level framework.

Model Illustration: Is Aggregate Production Planning useful?

How can an operations planning practice such as APP be described as a chimera (Buxey, 2003) and by other authors as the source of 'renaissance' in operations management (Singhal and Singhal, 2007)?

A detailed historical account of the birth of the domain of APP can be found in Singhal and Singhal (2007), Holt (2002), and Sprague *et al.* (1990). The first APP model was the result of work conducted in the middle 1950s by Charles C. Holt, Franco Modigliani, John F. Muth, and Herbert A. Simon on the "*planning of control of industrial organisations*". The focus of this pioneering research was to develop a method through which demand requirements for a product (or several different products) could be translated into a master schedule. In the 1950s, observation of industrial practice revealed that a particular challenging area was that of tactical planning. Holt and his colleagues developed a quadratic cost model (the HMMS model) and proposed a solution giving the optimal production and workforce levels. The solution allows managers to compute production level as a linear function of past inventory levels, hence the alternative label for the HMMS model as the linear decision rule. Their work and the subsequent improvements of the model were applied to a variety of industrial case studies, and significant cost savings were documented in the majority of cases. When savings failed to materialise, further investigation revealed that this was down to managers not using the linear decision rule (Singhal and Singhal, 2007).

Singhal and Singhal (2007) further argue that HMMS model contributed further to the renaissance of OM as it opened the pathway to:

- Linking strategy, tactics and operational aspects of planning, a stream of research about Hierarchical Production Planning (HPP) which was later taken over by Hax and his colleagues (e.g. Hax and Candea, 1984).
- APP made explicit the study of strategic trade-offs.
- Research on APP models' lack of implementation showed that too often, models could not be used because capabilities were missing (e.g. forecasting) or because other functions (e.g. marketing and distribution; Aucamp, 1986) should be involved in the process. This last stream is

especially important as it led to the extension of APP models to the modern notion of Sales and Operations Planning (SOP) and of Enterprise Resource Planning (ERP). More generally, the HMMS model highlighted integration and co-ordination as fundamental roles of modern operations management.

There has been a long stream of survey research that has noted that the uptake of APP by practitioners has been disappointing. Bowman (1963) was the earliest writer to look at APP "in use" and to start from the proposition that many practicing managers will actually make good APP decisions thanks to experience rather than explicit knowledge of mathematical APP models. In the 1980s and 1990s, there were a few attempts to survey APP practices in industrial firms (e.g., Gilgeous, 1987; Dubois and Oliff, 1991), and all concluded that (i) APP is often done intuitively rather than through the application of the APP models described in textbooks, (ii) constraints such as human resource, marketing and companies policies were more important in practice than in theoretical illustrations, (iii) practitioners often lack the information required to use APP models, and (iv) practitioners are not necessarily trained to use mathematical APP models. Buxey (2005) state that much of the modelling research on APP was "*deemed necessary because there are virtually no reported industrial applications*" and he later concludes that "*the reasons behind the dearth of industrial application for aggregate planning algorithms remain somewhat of a mystery*." His publications (Buxey, 1993, 2003, 2005) are based on manufacturing companies located in South-Eastern Australia. In terms of context, Buxey cases can be described as firms subject to considerable seasonality in a business environment where temporary and seasonal contingent workers are easily found. In his first paper, Buxey (1993) concludes that "*[assumptions] associated with this paragidm [APP] are all at odds with real situations*" and that "*complete integration of planning stages is a chimera as the available information is always imperfect, and there is no such thing as an optimal schedule*". In his second paper, he concludes that "*aggregate planning is a chimera. In practice, planners construct the master productions schedule directly, in line with a preferred solution strategy. A chase plan is the most popular choice*" (Buxey, 2003). Finally, in his last instalment, Buxey (2005) confirms his previous conclusions and declares that "*there is a more pressing problem though with the underlying theory. How does a cost minimisation model, which may actively encourage the stockpiling of finished goods, fit in with the current belief that just-in-time production represents the ultimate goal?".*

These two contrasting views (of AHP as functionally valuable vs. a useless scholarly fiction) is fascinating as it reproduces almost word for word the discussion of Griffiths (1994) in a very different academic discipline. Griffiths' argument is that there is not in biology a purely functional level of explanation. Translated to operations management, it means that it is impossible to research operations at a purely functional level: it is impossible to bypass the systematisation level.

Buxey's research (1993, 2003, 2005) documents firms exposed to a very high seasonality and a very adjustable, mostly labour-based, capacity. In contrast the HMMS model focused on moderate seasonality but uncertain demand with a moderately adjustable capacity. These are very different contexts, i.e. they are looking at every different 'species'. The former can be handled with little or no mathematical modelling, whereas the later can highly benefit from analysis as the solution (e.g. the optimal trade-off between chase and level strategies) is not trivial. Thus, Buxey's conclusions are valid, but only in the context in which they have been researched: that of a superseasonal form of demand that can be addressed by a local context which allows the implementation of a flexible chase strategy.

Research Questions and Methodology

The objective of this paper is to assess the quality of modern operations management research in terms of the validity of the scientific explanation that they provide by using Griffith's 4-level framework as a benchmark. This means answering the following questions:

1. At which level is research performed?
2. When research is performed at one of the upper levels, is scientific explanation a genuine multi-level, natural history, explanation (as in the case of Singhal and Singhal) or not (as in the case of Buxey)?
3. Are aggregate research efforts covering all levels of investigations?

These questions are answered by reviewing all the articles published in the *Journal of Operations Management* in 2015 and 2016. All the papers were downloaded and reviewed by classifying them as being primarily about characterisation, systematisation, functional explanation, or ecology. The criteria used for this classification are:

- Characterisation papers: the focus of these papers is to describe, model, or measure operations phenomena. Describing workarounds used by workers to overcome the shortcomings (such as resource shortage) of systems in which they work is an example of this type of research (Morrison, 2015).
- Systematisation papers focus on classifying different operation systems and exploring the relationship between different spatial and historical contexts with the nature of systems. They focus either on defining archetypes or configurations or providing adaptive historical explanations of practice. The evolutional history of professional services in the Leadership in Energy and Environmental Design by Lawrence *et al.* (2016) is an example.
- Functional explanations papers research the relationship between either the possession of a trait/component or a configuration of traits and the performance of a system. The impact of health information technology bundles on hospital performance (Sharma *et al*., 2016) is an example.
- Finally, papers about 'operations ecology' research ecosystems of operations systems and how contextual conditions drive practice and strategies. For example, Gao *et al.* (2015) describes the conditions in an ecosystem (technology diversity in supplier network, forms of information sharing, network structure, and market forces) that support a buying's firm new product creativity.

In addition to classifying papers according to the above scheme, each paper was scored in terms of how well it complies with the 4-level framework. Papers that do not contain research that is designed to research a uniform phenomenon within an homogenous population were rated as ambiguous in terms of methodological strength. Note that it may not be the case, i.e. different firms in different contexts may be identical units of analysis given a set research question. Take for example the research of Lam *et al.* (2016) on the impact of social media initiatives on operational efficiency and innovativeness. Their data set includes from more than 20 SIC codes. Although industry dummy variable are used to account for industry differences in their panel data model, Lam *et al.* (2016) makes the implicit assumptions that industry differences do not significantly change the relationship between social media initiatives and their dependent variables. This is an example of single-level research, i.e. it bypasses the systematisation level. A counter-assumption would be to posit that firms in different sectors or operating in different contexts will experience different effects of social media initiatives: such an historical adaptive assumption would be unambiguously compatible with the 4 level framework.

Whereas papers were classified as being rooted at a given level of the framework by using abstracts (with only a few papers needed to be read further to fully appreciate the scope of the research), it is impossible to score compatibility with the 4-level framework just from abstracts. Typically, scoring is only possible after reading the method and data sampling sections of each paper. The following scale was used to score the papers:

- A score of 0 was used for research that is designed at a single level of explanation, e.g. research which assumes that a purely functional level of analysis in operations management research exists, or for papers where data sampling remains ambiguous in terms of systematisation. For example, this is the score allocated to Lam *et al*. (2016) and for most cross-sectional research that do not consider historical or taxonomic views of diversity. This score should be taken as an estimation: it may in fact be that efforts to use dummy or other research controls variables are sufficient to account for diversity in a dataset; and whether or not this is the case remains unclear without further reading and research.
- A score of 50 was used for research that is indirectly compatible with the 4-level framework. Papers in this category includes research design that are consistent cross-level because they are based on a single case study or a set of specimens homogenous enough that the assumption of impact or anatomical homogeneity is warranted.
- A score of 75 was used for research where evidence could be found in the methods of the paper that the research design was informed by cross-level considerations. This includes for example the use of contingency variables.
- A score of 100 was provided for exemplar research, i.e. papers where the analytical work performed is multi-level or calls for such research.

Findings

Out of the 90 papers published in 2015 and 2016, 7 were eliminated as addressing methodological issues or viewpoints not directly related to scientific explanation. The classification of the remaining 83 paper is shown in figure 2.

Figure 2 shows that 51% of the papers are performed at the characterisation level and 40% at the functional explanation level. Only 5% of papers are about systematisation and 5% about ecological investigations. Furthermore, 33% of papers are based on single-level approaches and 42% are papers restricted in scope to a single case study/sector. This means that only 23% of papers are compatible with the 4-level framework of scientific explanation proposed by Griffiths (1994). Out of these 23% of the papers, 50% were scored as exemplars. The overall compatibility of all reviewed papers with Griffiths' framework is 42 out of 100.

These findings are interesting as they are reminiscent of Griffitths' concern with the claim that a purely functional level of analysis exists in biology. In the case of biology, Griffiths reject this claim and argue that both ecological life and functional explanation only make sense if they are consistent with an adaptive historical views of the species. Our data suggests that the commonly agreed view of operations management scholars is one where scientific explanation is based on two levels. First characterisation research is required to describe the traits of operations systems and to develop the measurements constructs to research them. Then, research can proceed at a purely functional level of analysis.

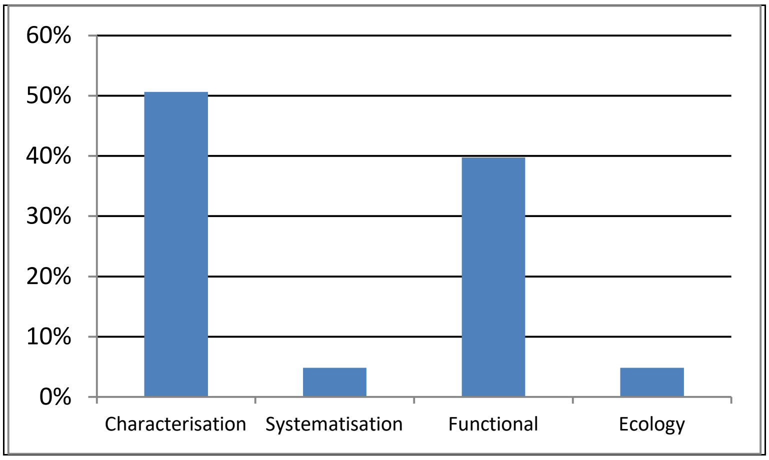

Figure 2. Percentage of papers performing research at each level

Discussion

It is interesting to compare the results shows in figure 2 with the extant literature discussing research in operations management. Swamidass (1991) provides one the first account of operations management as an area of research. He describes operations management research as being very good as a formal science, i.e. as a classical form of deductive science (this is making a reference to work published in management science). Swamidass stresses, at the time of writing his paper, the lack of classical empirical studies using induction, i.e. drawing conclusions from specific observations. His conclusion is that operations management should embrace modern empiricism, i.e. achieving a balance between deduction and induction, and proposes a staged model of building empirical operations management theories, starting from generating empirical generalisations, developing middle range theories, to finalising the construction of general theories.

Swamidass (1991) contribution can be broken-down into three key statements:

1. the need for operations management to embrace modern empiricism;
2. the need to build theories; and
3. the need to 'jump start' empirical research through observations, conjectures, and case studies.

The papers reviewed in figure 2 attest that operations management have come a long way as the vast majority of them are empirical papers. The number of characterisation papers demonstrates a genuine research effort at the empirical generalisation stage and the focus on the functional level confirms a strong interest in middle range and general theory building.

Many authors have reinforced Swamidass's (1991) call for empirical OM research and have focused especially on the second point, the need to make a theoretical contribution (Hitt et al., 2016; Walker *et al.*, 2015; Boer *et al.*, 2015; Hitt, 2011; Choi and Wacker, 2011; Ketchen and Hult, 2011; Craighead and Meredith, 2008; Wacker, 1998; Filipini, 1997). Some papers confirm that operations management research has come a long way and is now a more mature and scientific discipline (Craighead and Meredith, 2008; Helmut *et al.*, 2015).

Other papers are more critical and their findings echo the results shown in figure 2. Walker *et al*. (2015) conclude that "*the majority of studies are atheoretical, empirical, and focused upon theory testing rather than on theory development*". Boer *et al*. (2015) point out that discovery and observation are important parts of the scientific process and are "*often neglected avenues to contributing to theory*". Based on the results of figure 2 and these conclusions, it is worth asking why do operations management researchers almost systematically avoid the systematisation level? This is the level at which empirical generalisations performed at the characterisation level are compared and categorised and where conjecture, through historical adaptive considerations, result in theory building at the functional explanation level. Paying little attention to this level, or skipping it entirely, also explains why ecological-level research is under-represented in figure 2. The conjecture required to formulate theories at the ecological level requires an understanding of traits, species, and functions. Without a clear definition of species, it is unlikely that much research can be done at this level.

This point is reminiscent of Meredith's (1992) concern about theory building in operations management and his promotion of conceptual modelling as an inherent part of the scientific process. Meredith explains the disconnect between operations management research and practice by the lack of observation-based stages of description: "*if the description stage is ignored [...] research findings become more and more disconnected from the real world and irrelevant to the reality of the problems facing managers*" (Meredith, 1992, p. 4) and he quotes Lin as stating: "*plunging into functional modelling without sufficient theoretical and empirical examination usually brings confusion and frustration to the theorist* (Lin, 1976, p.52 quoted in Meredith, 1992, p. 4). The results of the review summarised in figure 2 are symptomatic of this confusion and are 'bad habits' as described by Schmenner *et al*. (2009) statement about "*too much theory, not enough understanding*".

Another related debate in the operations management theory literature is whether or not operations management should utilise theory from other fields, i.e. strategy or economics, or should be based on its own 'home-grown' theories. Experts are very much divided on this issue. One position (Stratton *et al*., 2016) is to limit the scope of operations management to these theories (e.g., swift even flow, theory of constraints, focused factory) and to consider any other theoretical view as non-operations management. It is interesting to note for example that many excellent operations paper are published in management and strategy journals, and that many of the exemplar research reviewed in this paper could have been published in such journals. This position, however, would result in restricting the scope of research to characterisation research akin to 'factory physics' (Hopp and Spearman, 2000). At the other end of the spectrum are scholars that recommend the use of theories from other field (e.g. Hitt *et al*., 2016; Hitt, 2011) and argue that operations can contribute to further elaborate these theories. Holweg in Boer *et al*. (2015) suggests a middle of the road approach and argue that although it is possible, but not without risks, to use theories developed elsewhere, it is nevertheless a worthy endeavour to develop our own stock of theories.

Conclusion

The conclusion of this paper is to support Holweg's viewpoint and to argue that in order for operations management to truly find its place in science it needs to develop its own theories. This is only possible by developing 'better habits' and adopting a less truncated view of the scientific investigation process. This means acknowledging the central role of observations and taxonomies in the development of such an 'operations science'. Like Griffiths (1994) in biology, we reject the idea that a purely functional science of operations management is possible and call for the development of next generations operations management research based on historical adaptive explanations. New research areas such as sustainable operations and supply chain management are particularly promising as they expand the scope of research and make possible the definition of conceptual levels such as operations ecology, provided that we do update the way in which we do research (Pagell and Shevchenko, 2014).

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