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Too Much Theory, Not Enough Understanding

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Ten years ago, Morgan Swink and I published an article in JOM entitled “On Theory in Operations Management”. It was meant to counter the concerns of many at that time that operations management did not have theories of its own that could ground our discipline. Now, ten years on, 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’m afraid that my responses to these questions do not ring with contentment. It is time to reassess the role of theory in operations management.

What is Theory and How Should It Work within a Discipline?

Our paper 10 years ago pointed out a number of important features concerning the use of theory, many of which were attributed to Carl Hempel, a renowned philosopher of science.

1. Theories explain facts and provide stories as to how phenomena work the way that they do. They can, and should, be used to make predictions.
2. Theories are not built; they are invented. That is to say, theories cannot be systematically constructed or deduced from facts. Theories require inspiration and creativity. Facts and the regularities among those facts may exist for generations before an adequate theory is invented to account for them.
3. Theories can be disproved by findings that run counter to their predictions or explanations. On the other hand, theories cannot be proved. They can only be supported by other evidence.
4. The building blocks of understanding are hypotheses and their tests. Hypotheses do not need to be based on any theory; they can be mere guesses. When hypotheses are tested, we gain facts with which we can confront theory.

In a number of disciplines, these features of theory and their use are fundamentally important. Recently, for example, there was much ado about the newly-built, 27 km-in-circumference underground large hadron collider near the Geneva airport. This expenditure of \$8B has been made specifically to provide more “facts” against which to evaluate the so-called “standard model” of sub-atomic physics. The standard model has been able to handle the facts as they are currently known, so the entire physics community has rallied behind this construction of a new and more powerful accelerator that, it is hoped, can provide new facts with which to confront the standard model. It is also hoped that new facts can resolve the usefulness of string

theory as well. String theory is elegant, to its credit, but there are an increasing number of skeptics with regard to its usefulness. The physics community is thus united in the importance of generating new facts and of showing the weaknesses of prevailing theories with those facts. The goal is to topple a deficient theory (and all theories can be deficient) and to erect a new one that does a better job with the facts as they are known.

Unfortunately, we in operations management do not appreciate theory as the physicists do. We do not get excited about tearing down prevailing theories and erecting new ones that can handle the facts better. We, unfortunately, conform to the indictment of behavioral science by Abraham Kaplan, another leading philosopher of science. As he put it,

“It might well be said that the predicament of behavioral science is not the absence of theory but its proliferation. The history of science is undeniably a history of the successive replacement of poor theories by better ones, but advances depend on the way in which each takes account of the achievement of its predecessors. Much of the theorizing in behavioral science is not building on what has already been established so much as laying out new foundations, or even worse, producing only another set of blueprints.”
(Kaplan, 1964, p.304)

And, Kaplan argues, behavioral science often has an unhealthy fixation on methodology:

“Many behavioral scientists, I am afraid, look to methodology as a source of salvation: their expectation is that if only they are willing and obedient, though their sins are like scarlet they shall be as white as snow. Methodology is not turned to only as and when specific methodological difficulties arise in the course of particular inquiries; it is made all encompassing, a faith in which the tormented inquirer can hope to be reborn to a new life. If there are such illusions, it has been my purpose to be disillusioning. In these matters, the performance of the ritual leaves everything unchanged, and methodological precepts are likely to be as ineffective as moral exhortations usually are. There are indeed techniques to be mastered, and their resources and limitations are to be thoroughly explored. But these techniques are specific to their subject-matters, or to distinctive problems, and the norms governing their use derive from the contexts of their application, not from general principles of methodology. There are behavioral scientists who, in their desperate search for scientific status, give the impression that they don't much care what they do if only they do it right: substance gives way to form. And here a vicious circle is engendered; when the outcome is seen to be empty, this is taken as pointing all the more to the need for a better methodology. The work of the

behavioral scientist might well become methodologically sounder if only he did not try so hard to be so scientific!” (Kaplan, 1964, p. 406)

What Have We Done in Operations Management?

Think now about the typical paper published in operations management. At the risk of straining the archetype, allow me to identify some key features of such a paper.

1. The paper is an empirical piece where direct influences on the variables of interest may be posited but where indirect, or moderating, effects are often posited as well.
2. Although the piece is empirical it is typically positioned as useful for building theory, and not simply as a source of new facts that may need to be explained.
3. The hypotheses to be tested allegedly derive from theory. The hypotheses are not merely guesses but predictions from one or another social science theory. A great literature search has been accomplished to isolate these hypotheses and to defend them.
4. Great attention is paid to the reliability of the measures used.
5. Although great attention has been paid to the variables measured, somehow the effects of significance to managers are “latent” ones and thus structural equation modeling is engaged in.
6. The empirical results are often mixed. Some of the hypotheses are supported but others are not. The lack of support for the theory standing behind these hypotheses, and for which so much searching in the literature was done, does not lead to any criticism of the theory nor any claim that it has been overturned by the empirical results.

What is wrong with this archetypal approach?

- a. Theories come, but they never go. The goal, it seems, is never to knock down the theory that suggests an effect, even if the empirical results contradict it completely. The theory is thus never treated as a fully-fledged theory that lives and dies with its ability to explain current facts and to predict new ones. It is a vacuous theory that shamelessly escapes to be cited another day. While the typical claim is that the paper is “grounded” in theory, it really is not. Theory is on the periphery and not at the center of the inquiry.
- b. The literature search is next to meaningless. The literature search is done to provide cover for the researcher and not to isolate an applicable theory and provide a good test of it.
- c. Hypotheses are tied to theory and they need not be. We do ourselves a disservice by asking authors to base their hypotheses on some theory or another. By doing so, we may stifle the inspired guess.

- d. The ambition of the paper is either limited or misdirected. Often, the best claim that the paper thinks it can make is that it is a building block for theory or that it is a way station “towards a theory”. Of course, this loses sight of the fact that theories are invented and not built. In fact, the paper need not develop a theory itself. Lots of good science is done without the need to proliferate theories. In reality, the paper can simply provide us with new facts. Sometimes those facts can provide evidence that contradicts an existing theory and forces us to rethink it. Sometimes it can provide support for a theory. Other times, it simply provides us with more facts to ponder. This is worthy enough.
- e. The research pulls punches. One of the strengths of operations management is that it lives in the real world. We can frequently go out and measure directly lots of the important aspects about processes of all different types. We can expect that a lot of the relationships that govern those processes are direct ones. The need for “moderating” influences or for “latent” variables is much diminished in our field. Labeling something as a “latent” variable should be a last resort when we cannot be clever enough to procure a direct measure. Wherever possible we should avoid convoluted cause-and-effect or having to rely on the second-rate power of structural equation modeling. We would be better off spending more time in the field going after clever, effective, direct variables that can save us from such potentially risky techniques.

What Should Be Done?

Several things that can tie us better and more fully to theory and to the good practice of the philosophy of science come to mind:

1. Good empirical work does not need to be based on theory. It can be independent of theory. Some of the more important touchstones of operations management came into existence without theory; any theory that now stands behind them came later. Think, for example, of just-in-time/lean manufacturing, the bullwhip effect, cellular manufacturing, factory focus, and much of scientific management. The useful early work on these topics was heavy with observation. What we need are well-reported and well-defined facts to explain.
2. We need to go the extra mile to uncover the most telling situations and the most appropriate measures. The Nobel Prize in Physics is often given to the clever experiment that is stunningly simple in concept. Simple, straightforward tests are almost universally preferred to convoluted ones.
3. Have the courage to debunk what our colleagues, revered as they may be, have done. We need to knock down more theories and explanations and invent better ones in their place. We also need to avoid treating vacuous theory as “theory”. If a “theory” is so squishy that it cannot be invalidated straightforwardly, then we need to abandon its use. We will not advance our understanding by using it.

4. We in operations management need to teach our doctoral students differently and we need to review one another's papers with less of an eye to methodology and more of an eye to creativity, insight, and understanding.

The excerpt from Kaplan suggests that we, as a discipline, should not get carried away by the form of our research. Methodology is not knowledge. Instead, we need to focus on what appears to be important to know, what we understand about it, and what we can do to understand it better.

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