DISCIPLINE BOUNDARIES IN INNOVATION STUDIES: OPERATIONS MANAGEMENT AND ALLIED FIELDS

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In this essay we summarize the state-of-the art of innovation studies shadowed by the daunting challenges this eclectic subject domain incurs as a result of discipline silo structures in most universities. Indeed, developments in technology and other external forces like competition and regulatory changes have pushed organizations towards development and adoption of new approaches particularly salient in Operations Management (OM). We show, however, that in the academic community, these discipline-based efforts have created an impediment to development of a general theory of innovation processes. We conclude with an upbeat and optimistic appraisal of possible ways forward and suggest a process to accelerate theory-building that might deal with the growing feeling among many scholars that many academic journals are becoming irrelevant. Examples from the current literature and reflections on the past 40 years of research in the field are liberally sampled. Although our essay views innovation through a broad bi-disciplinary lens, we place OM and technology as the centerpiece of discussion and use it as an exemplar for the proposed way forward.

INTRODUCTION

Interdisciplinary science continues to increase in pursuit of new answers to complex problems (Van Noorden, 2015) and has unique challenges according to many reports, including publication potential in otherwise discipline-bound journals. In a study covering an 18-month period and five interdisciplinary research centers, it was found that young scientists attracted to this approach as graduate students are "deterred by professional risks as early career, tenure-track scientists" (Rhoten & Parker, 2004, p. 2046). Further, problems of management of interdisciplinary research might be overcome by taking a fresh look at knowledge integration issues, which are often at the core of the underlying process of this approach to science (Siedlok & Hibbert, 2013).

One field of study that has great appeal for interdisciplinary science is the innovation process as organized activity in R&D laboratories and start-up business ventures. We have come a long way in the 40 years since Downs and Mohr (1976) argued for two types of innovation attributes, and the struggles that followed to arrive at testable theory in this very eclectic field of study: the i management of the innovation process. We have seen economic models like those of David Teece, sociological theory documenting diffusion of innovation like Ev Rogers,psychological hypotheses like Michael Kirton, marketing frameworks like Gerry Tellis, , strategic management frameworks like Michael Tushman and Phil Anderson, and many more, with apologies for not including everyone here. The challenge continues to be that these discipline-based views have not lead to a general theory of the innovation process. First, we present examples of how this discipline-myopic tendency continues unabated. Second, we cite emerging examples showing there is an alternative to the much-abused edict of multidisciplinary approaches to the field which have not materialized and have not provided much substantial headway in overcoming these discipline-based approaches. This emerging trend is the discipline cross-over and potential capture of the language barriers between (two and perhaps three) disciplines (e.g., supply chain innovation vis-à-vis open innovation). However, we argue that choice of discipline collaboration should be guided by domain centroid distance. Finally, we include examples of these early signs, and suggestions for low-cost ways forward are presented.

THEORIES OF INNOVATION

One of our esteemed colleagues remarked recently that he hoped someday we'd refer to "open innovation" as simply "innovation." If one examines the historic record of contributions to this highly eclectic field, it could easily be argued that this will never happen—or at least it won't happen during the lifetimes of those writing in the field today. The siloes in structures of academic disciplines simply won't allow it. There is no incentive to become a maverick thinker and stray outside the prescribed boundaries, especially for newcomers attempting to establish a reputation.

Having said that, there is little doubt that we have made great progress in understanding the various aspects of the innovation process at different levels of aggregation: from individual creativity all the way up to nation-state comparisons that have appeared in the literature. What often passes for a strategic theory of innovation, like Christensen's disruptive technology model, comes under sharp attack for well-founded and well-meaning reasons.

Many of these more strategic or broader theories begin with observations of empirical trends like the failure of firms to continue to lead their industries once they attain the first position in a sector. Christensen explains this with the notion that leaders are toppled by firms that enter the market with offerings too insignificant to be bothered with by leaders, but eventually these simpler solutions to problems improve and overtake the leaders, as was the case in the calculator industry, the copier industry and the steel industry. Gerald Tellis (2013), starting with essentially the same data, has a corporate cultural explanation for the toppling of leaders so over-invested in the status-quo, they can never change their culture fast enough to meet emergent challengers. The challenge is to maintain two perspectives, long-range and short-range, simultaneously. O'Reilly and Tushman's (2013) answer to these same data is essentially to follow one of three paths towards ambidexterity. There is another argument that goes much deeper in explaining why we don't see agreement on an even somewhat broader theory of innovation, and that is the very nature of the way new theory and empirical research come about in this eclectic field. This topic is taken up next.

Where Theory Begins and Ends

The first barrier to innovation is siloes and parallel theory developments that do not consider relevant developments in parallel areas, resulting in incomplete and possibly irrelevant innovation. As a result we observe a lack of complete topical knowledge or permeation across disciplines. For example, a meta-analysis of SCM research documents the problem of researchers using different interpretations of the same concepts and different concepts with the same meaning (Fabbe-Costes & Jahre, 2008).

Consider the following example. McKinley, Latham, and Bruan (2013) write:

In contrast [to flexible innovation], an inflexible innovation is one in which the possible range of post-introduction configurations is relatively small and the speed of transition between these configurations is slow. A narrow range of post-introduction product, service, or product process configurations and a slow transition between distinct configurations make an inflexible innovation less malleable after it has been put on the market. An example of an inflexible innovation in a product market…would be a new software program for managing a supply chain. Because the operation of supply chain must conform to many criteria imposed by supply chain managers, such software will have fewer possible post-introduction configurations, and will be less open to speedy transition between configurations. In the category of production processes, the automated mass and process production systems that replaced unit and small-batch production technologies in factories around the turn of the twentieth century…represent inflexible innovations. Compared to the unit and small-batch systems that dominated earlier, mass production and process production systems had a narrower range of possible machine-human configurations (retooling) was slow(p. 91).

However, the authors fail to cite one of the most important current and central themes of production and operations management. The shift in production technologies to automated mass production at the turn of the 20th century was actually reversed by the introduction of flexible automation and computer-integrated manufacturing (McDermott & Stock, 1999; Zammuto & O'Connor, 1992; Lei, et al., 1996; Boyer, et al., 1997; Ettlie & Reza, 1992) that allowed flexible lot sizes and part family migration (Ettlie & Penner-Hahn, 1994) at the same unit cost. That is, manufacturing "scope" has replaced manufacturing "scale" as a dominant form of production configuration, including mass customization during the last 40 years.

Further, the substitution of scope for scale in manufacturing and the inextricable connection between product and process flexibility continue to challenge the best of companies. This cross-over of economics into operations is illustrated well by the following example.

The need for flexibility in manufacturing has not diminished; indeed, it is escalating. Take a most recent example of Honda's new automated engine plant in Alabama (Chappel, 2015; http://www.autonews.com/article/20150817/OEM01/308179970/hondas-factory-fix:-robots). Increasing federal regulatory pressure for improved fuel economy (54.5 average mpg by 2025) lead Honda to plan for future engine changes and introduce more flexibility into engine production. Honda overhauled its 15-year-old Lincoln, Alabama engine plant with 92 robots and increased automation to 75% (versus 30% previously) of the total conversion process of aluminum ingots into V-6 engines. The plant now runs two shifts with a reduction in workforce from 340 to 240 people, with further reductions planned as the plant ramps up to full capacity.

Therefore, flexible innovation is not a new concept, but rather one applied to decline and rebound in a product innovation type that emulates this production flexibility enabled by new computer-based and now digital production technology. Is this the way forward in theory development in a field of study? That conclusion seems unlikely.

Further issues arise when McKinley, et al. (2013) give examples of flexible innovations. They say, "…a flexible innovation…[allows] a wide range of possible post-introduction configurations that the innovation can assume and the speed of transition between those configurations is rapid. An example is an 'app' …to tap the market of game users…[like] *Grand Theft Auto* or *Mortal Kombat*…."(p. 91). Setting aside the ethical concerns of computer games being played by minors, consider the following counter-example from this industry. The company is Valve and the method of computer and video game production is crowd sourcing on their Steam application. The innovative business model is based on production cost reduction, similar to the application of the "scope" model mentioned earlier but applied to this industry, quite differently than the examples cited above like *Grand Theft Auto*. Valve has unique organizing principles as well, including no titles and no bosses (Mavidou and Sloan, 2013), , similar to how Quad Graphics began their breakaway organization in the printing industry with no business cards and co-optation, that is, supplying and competing simultaneously in the printing and direct marketing services industry (Ettlie & Rosenthal, 2011).

A poster child for the challenges we face in this eclectic field is the journey Clayton Christensen has been on all these years since 1997 when his first book on disruptive technology appeared. We expand on this "perfect" example next.

Disruptive Technology: Poster Child for Innovation Theory Challenges

How do we reconcile inconsistencies and limitations of current theories? How do we merge research streams to create new theories of Innovation? We start the process of reintegrating and merging existing theory and research streams in this field by starting with the current dilemmas posed by the disruptive technology construct and then considering how the limitations of this model can be ameliorated by open innovation and dynamic capabilities.

A special issue of the *Journal of Product Innovation Management* edited by Erwin Danneels (2004, "Disruptive technology reconsidered: A critique and research agenda," *Journal of Product Innovation Management*, 21 (4): 246-258, voted best article of 2004) was the last omnibus attempt to sort out the pros and cons of the Christensen arguments concerning disruptive technology (DT) that new entrants eventually displace incumbents because the latter focus on sustaining technology and on their best customers and overshoot the much larger, over-served markets.

As of this writing, the most recent published re-evaluation of Christensen's ideas appeared in the Wieners (2012) article in *Bloomberg-Business week*, May 7-13 issue. Although the purpose of the article was to review Christensen's new book, *How Will You Measure Your Life*, at the end of the piece, the innovator's dilemma is revisited and critiqued. Wieners writes, "If there has been one knock against Christensen's theories, it's that they have been better as analysis than as a course of action" (2012, p. 68). He further goes on to quote consultants saying the theory is an "incomplete idea." There is rebuttal in the article, citing solid returns to an investment firm and venture capital enterprise based on the DT model, but the data are incomplete, with a very short track record to date and with the questionable investment returns in earlier ventures (Danneels, 2004). Further to the point, Wieners (2012, p. 68) quotes Larry Keeley, another consultant, as saying "the theory is more descriptive than prescriptive," and "[t]here are very few robust intellectuals working on innovation." We'll come back to this latter point below, and in the end, readers and reviewers will be tasked with either agreeing or disagreeing with this statement. However, it is clear to many in the field that scholars like David Teece, Frank Rothaermel, Andrew King, Chris Tucci, and many, many others do not lack intellectual capacity. Among others, Gerald Tellis (2006; 2013) has been very active in empirical testing relevant to the DT model and concludes the original theory was developed using a flawed sampling methodology. Further, the general confusion in the trade press and even business cases between radical innovation and disruptive technology continues 15 years after the original article by Christensen and Bowers (1995).

Certainly the original book, *The Innovator's Dilemma* generated considerable interest in the field of innovation among practitioners and academics. Christensen received the distinguished scholar award bestowed by the Technology & Innovation Division of the Academy of Management at the 2011 annual meeting. So at least a significant minority of the academy seems to think his contributions have been noteworthy. There have been a number of important articles published by good researchers on DT, but very few have challenged the original model or original data. For example, with the insight of a demand condition that explains the model (Adner, 2002) using the example of desktop computers, Adner argues that lower unit price, not price/performance comparisons, caused disruption. This type of research clarifies the DT model but does not really offer an alternative comprehensive theory or even a satisfying theoretical fragment. It merely extends, quite nicely it can be said, the original DT argument.

More substantial commentary has been forthcoming in articles like that published by Utterback, et al., (2005) which give numerous counterexamples to refute the DT model by showing how they don't conform to the original DT pattern. These examples include the compact disc, electronic calculator, components for fuel injection, wafer board and oriented strand board construction panels. The authors argue that digital cameras when first introduced had superior performance in some dimensions compared with film, which refutes the DT model. Further, they argue that disruptive technology products actually enlarge and broaden markets and provide new functionality rather than displace established products (p. 15). They also say the DT model does not apply to services as Christensen contends but do not address that issue. The authors offer the final suggestion that some combination of the theory of discontinuous innovation and theory of disruptive technology may ultimately be the answer to these inconsistencies.

Other authors have suggested similar resolutions to the “dilemma.” For example, Yu and Hang (2010) suggest integration of the DT theory with other research domains such as open innovation. In this paper an alternative approach is taken to explore and re-evaluate where we stand on the “dilemma” and what to do about it. In the current view, we go back to the roots of the theory of disruption itself for clues at the scene of the crime. First we exam the original data reported by Christensen (1997) and a secondary and extended analysis of these data (King & Tucci, 2002) and then suggest a theoretical way forward in light of these findings.

Christensen originally reported that there was no relationship between radical technology and whether it sustained or disrupted an industry leadership pattern (1997, p. 13). When the technology did disrupt industry leaders, “innovations were technologically straightforward, consisting of off-the-shelf components put together in product architecture that was often simpler…” (p. 15). However, later, in a secondary analysis of these data, he shows that in fact the greatest revenues and profit accrue to firms that enter new markets with new technologies, which is a direct contradiction of the original theory. Further, he says the way to avoid disruption by new entrants is to engage in careful self-disruption by establishing a separate organizational unit far away and removed (physically) from the incumbent’s goals, metrics and organizational routines. This is the way HP introduced the disruptive ink-jet printer. However, HP used an existing organizational unit to launch and successfully commercialize inkjet printers, eventually off-shoring the production to Singapore. And there appear to be industry differences (King, and Baatartogtokh, 2015).

Reanalysis of the Christensen data by King and Tucci (2002) demonstrated that even though leaders were toppled in the disk-drive industry by new entrants, these same firms went on to become even stronger competitors in other industry sectors. Further, that new entrants topple incumbents is not a novel observation—this finding has been in the literature since the 1980s and introduced as a key point in the Utterback, et al. (2005) article as well as by von Hippel in his early work on user innovation. In other words, there are alternative explanations for new entrant success, as advanced by Tushman and Anderson (1986, 1990) and others, involving theories of organizational inertia and other explanations.

In this paper, a new approach to resolving the “dilemma” of the innovator’s dilemma is suggested using an extension of the dynamic capabilities framework (e.g., Teece, 2006). We can draw on data of the economic downturn of 2008 in the auto industry to see how this approach can be extended (Rothenberg & Ettlie, 2011). Integrating both the dynamic capabilities and open innovation (von Hippel & Oliveria, 2010) perspectives including user innovation moves us ever closer to a general theory of the innovation process.

First Things First: Disruptive Technology It’s Not

Christensen’s notion of integrating markets with technology is an original and significant contribution to the literature. But it is not independent of the work that preceded it and has followed since. His data on disk drives supposedly did not sort out or make sense of, in light of the current models at the time, radical and incremental technology. However, using Christensen’s own recasting of his disk drive data (see Table 1), we can easily see that radical (new) technology versus proven (incremental) technology does make a difference in the established versus emerging market success categories of disk drives.

TABLE 1

WHAT STRATEGIES GENERATE GROWTH

	ESTABLISHED MARKET	EMERGING MARKET
New Technology	**0% Successful***	**38% Successful**
	Total Accumulated Revenues $236.7B	Total Accumulated Revenues $16,379.3B
	Average per Company $15.8B	Average per Company $2,047.4B
Proven Technology	**3% Successful**	**33% Successful**
	Total Accumulated Revenues $3,056.2B	Total Accumulated Revenues $35,734.7B
	Average per Company $86.2B	Average per Company $1,906B

Market Entry Strategy

*Success=generated at least $100m per year revenue

SOURCE: The Opportunity & Threat of Disruptive Technologies Clayton Christensen, © 2001-2002 President and Fellows of the Harvard College (from a CD Rom of the same name)

Integrating Dynamic Capabilities with Disruptive Technology

In their seminal article, Teece, et al. (1997) use the economic theory of appropriation of rents to build a model of dynamic capabilities, which relies on learning, integration and coordination to leverage innovation for competitive success. Since then, Teece (2006) has added to this theory by emphasizing the notion of sensing, seizing and reconfiguring this paradigm. However, what is perhaps most relevant to the integration of this dynamic capabilities framework with the disruptive technology model and open innovation is the type of capabilities that are most distinguishing for the ultimate transformation of the firm: intermediate appropriation conditions. Strong and weak conditions refer most generally to the degree to which ideas can be protected and captured for economic leverage. But in the original article, Teece, et al. (1997) suggest that there is an intermediate condition as well. What might this intermediate condition look like? We take that up next.

Why do firms engage differently in co-development of new products (or services)? Partial answers to this question do emerge in the literature. Two have been documented in widely different literature streams. First, there appear to be industry differences based on technology intensity (high-tech versus low-tech) in how dynamic capabilities impact the outcomes of joint development alliances in virtual engineering teams (Ettlie & Pavlou, 2006). Taking technology intensity into account is quite important when one examines Figure 1 which shows that markets AND technology both matter to the success of disruptive strategies, contrary to the DT theory.

Second, there appear to be industry differences in how firms use external access to dynamic capabilities. On the one hand, in discrete parts manufacturing there tends to be intensive, serial, but shorter-term engagements versus longer-term relationships in non-discrete parts manufacturing like chemicals.

The importance of this type of empirical evidence is that it points to the way in which resource issues are resolved in overcoming the innovator's dilemma. In fact, if there were no resource issues, in theory, there should be no dilemma. If resource issues were resolvable, general managers might spread their bets (real-option theory would also apply here) and prepare to self-disrupt on many promising and emerging dominant design options. This does ignore the inertial factor in incumbent firms, of course (Tushman & O'Reiley, 2013), but it is the first step down this path of self-disruption. If the trajectory of dynamic capability development is towards resolving intermediate appropriation issues in a path-dependency framework, this would have very important theoretical implications. What are we left with so far? There is hope that while, in light of the disk drive data originally cast, Christensen says the "existing models and paradigms didn't make sense," they do in fact make sense if one understands how they can be recast (Table 1) taking success of innovation into account and the type of technology market into account systematically. Previous as well as subsequent research on the type of technology (radical vs. incremental) and product versus process technology does matter. And there is potential to integrate these two paradigms.

ONE WAY FORWARD IN THEORY DEVELOPMENT OF INNOVATION

Disruptive technology, dynamic capabilities, and of course, organizational ambidexterity (O'Reilly & Tushman, 2013), are three legitimate contenders for a strategic theory of the innovation process. All three frameworks have shortcomings, some of which have already been pointed out, including a more recent meta-analysis of ambidexterity (Junni, Sarala, Taras, & Tarba (2013), which found that industry context is a significant moderator of the impact of ambidexterity on performance, methodological issues notwithstanding. Given the widespread challenges of these theories on the innovation process in the face of the apparent increasing importance placed on innovation among practitioners, it appears that the reconsideration of the way forward in theory-building for this (and perhaps other related fields like entrepreneurial orientation) would be timely.

We believe that the revitalization of the interdisciplinary approach to science and problem-solving is, in its current, vague form, at best, misguided and at worst, harmful to rethinking the theory-building process because of its indifference to the checkered history of interdisciplinary efforts. Although it may be true that the disciplinary structure of academe discourages interdisciplinary work, it is equally true that a "general" approach to any problem or science which vaguely advocates crossing discipline boundaries is also problematical. We offer an alternative to serious researchers and theory-builders here which responds to the legitimate critics of discipline-based science and at the same time avoids the trap of vague and wasteful admonitions for more "inter-" or "cross-" or "multi-" discipline approaches to improving our understanding of the innovation process.

Discipline Boundaries in Innovation Studies

We make a modest, but we hope very useful, suggestion here to advance productive thinking about innovation studies and the role different disciplines might play in this process. ***This suggestion is to carefully select the boundary conditions between two (or perhaps three) academic disciplines in pursuit of new and useful theory-based research on the innovation process.*** We have taken this approach based upon our own experience in cross-disciplinary projects (many cited later), and with full knowledge of the literature that has pointed to the shortcomings of cross-disciplinary research. For example Rasssch, Lee, Spaeth, and Herstatt (2011) observe a pattern in the evolution of "high-involvement" interdisciplinary research in open source innovation after a few years becoming more like parallel forms of collaboration, and more easily fitting the definition of "low-involvement" or multidisciplinary research. Projects are often difficult to modularize and there are often changes in task uncertainty over the life of projects. Some disciplines are more amendable to crossing disciplines than others (Van Norden, 2015).

We begin with two simple examples that shine a light on the path forward and then review historical precedents in this same spirit. First, we admire the article by Steward and Aldrich (2015) which documents the impact that anthropology has had, and could have on the theory and practice of management. We endorse the proposal of cross-campus initiatives between the ***two*** [our emphasis] disciplines and the idea of a consortia of institutes (Stewart & Aldrich, 2015, p. 184). We would further add that a virtual collaboration of bi-discipline programs focused on the innovation process would exploit state-of-the-art virtual teaming technology so common in companies and in use, in some form or another, for decades.

The second example is the first-ever conference of scholars from many disciplines, including marketing, operations, management information systems and corporate strategy to explore the potential of finding ways to improve our understanding of supply chain innovation. This conference, hosted by Professor Morgan Swink and held at the Neeley School of Business and Texas Christian University, was titled “Driving Innovation in the Supply Chain” (June 3-5, 2015). The focus of applied research on the supply chain has traditionally been on cost reduction, not innovation, and this was a sharp departure from this precedent and is evidence of the movement taking place toward inventing a new process relevant to the innovation process. Although some of material presented at this conference was not easily accessible because corporate presentations of innovation efforts are considered proprietary, the sessions clearly demonstrated a willingness to consider a new process for this important field in the operations management discipline (some of the keynote presentations are available at http://neeley.tcu.edu/innovation/).

As a result of participation in this conference, we have been motivated to offer this modest proposal for refined bi-disciplinary development and process management to accelerate theory development and administrative experiments among practitioners. We think this might be just one approach to avoiding the criticism of irrelevance of some management research (Bartunek & Rynes, 2014; Bell, et al., 2006; Bettis, 1991).

Rethinking Interdisciplinary Innovation Studies

We suggest in the modest process proposal below that there is a way forward to nurture innovation theory-building and experimental practice of innovation management heretofore neglected in this eclectic field. We suggest that boundary management between two (and rarely more) academic disciplines presents an emerging opportunity that has been masked by the traditional approach to management science that has been confined to single or philosophically adjacent academic disciplines. For example, Organizational Behavior and Psychology have centroid discipline foci so closely aligned as to be adjacent in our terms and have less potential for generating new theory for the innovation process. Similar examples of close centroids could be listed, like Industrial Engineering and Operations Management, or Marketing and Psychology. Conversely, disciplines that have widely differing centroids, such as Organizational Behavior and Electrical Engineering, are also unlikely to generate new theory at their intersection.

We offer two alternative examples with more optimal centroid discipline foci from Organizational Behavior and Operations Management, and Marketing and Operations Management. These two sets of academic disciplines exemplify optimality for multiple reasons. First, their foci are not so closely aligned to result in a high degree of overlap or knowledge eclipse, while they are neither so far apart where they may share little overlap and lack a common language or similar theoretical foundations

Second, these disciplines share common methodologies, in the form of both empirical methods such as survey research, experimental studies and case studies, as well as quantitatively based methodologies, such as mathematical and econometric modeling. In fact, the importance of both topical and methodological commonality in bi-disciplinary research is highlighted in a series of studies focused on interdisciplinary research in supply chain management (SCM) (Sanders, Zacharia, & Fugate, 2013; Zacharia, Sanders, & Fugate, 2014). Functional areas of study sometimes lock into a particular paradigm, theory or research method ignoring complementary developments in other functions that could provide additional insights (Merchant, Van der Stede, & Zheng, 2003). Topical and methodological commonality serves to mitigate this by providing “common ground” as the foundation for bi-disciplinary research (Zacharia, Sanders, & Fugate, 2014).

Further, these discipline intersections often_mirror what is naturally observed in the business environment. The importance of this was highlighted by Marshall Fisher’s plenary talk at the 2005 POMS Annual Conference in Chicago, which he appropriately titled "What can we learn about research style from physics, medicine and finance?" Marshall compared the development of innovative and “big ideas” in OM versus those of other disciplines. He argued that unlike academic physics, medicine and finance, which have created "big ideas" that are intellectually deep and highly impactful, the big ideas in OM have rarely come from academe. He gave examples of TPS, TQM and SPC, which have all come from industry. The reason for this, Marshall argued, lies in the integration of theory and practice. He went on to provide examples from medicine that use clinical trials for drugs and conduct epidemiological studies, and where the premier academic journal, *New England Journal of Medicine*, also publishes descriptions of interesting cases. Consumers increasingly co-create products and services with firms (Prahalad & Ramasawamy, 2004).

By contrast, in OM, we continue to make the mistake of not looking to practice for truly relevant problems that result in “big” innovations. Further, in the business community relevant challenges are by definition functionally integrated and cannot be addressed through one siloed lens. Therefore, naturally occurring business functions – OM, marketing, logistics, purchasing, HR, finance, MIS – offer the best start in disciplinary coupling.

Next we look at how two bi-discipline exemplars achieve this.

Organizational Behavior & OM

The convergence of Organizational Behavior and Operations Management, dubbed by some as Behavioral Operations, exemplifies optimal centroid disciplines and showcases the opportunities for innovations in both theory-building and addressing relevant challenges. The nexus of these disciplines involves studying the impact of human decision making on system design and the role judgment plays in impacting operations management. One example can be seen in the area of forecasting, which is at the heart of operations management decisions.

Virtually all operations management decisions are based on a forecast of the future. This includes everything from production and inventory planning, scheduling, capacity planning and location analysis, to logistics and transportation. Improving forecasting performance has been shown to lead to significant benefits throughout the supply chain (Oliva & Watson, 2009). At issue, however, is that although sophisticated quantitative forecasting methods are available, the majority of forecasting processes in practice still rely on human judgment (Lawrence, et al., 2006). Forecasts are essential to most innovation and related processes in the firm.

In an era of unprecedented technological capability, analytics, and big data, this underscores a reality of the practice of operations management. Forecasts in practice continue to be produced as a combination of a statistical forecast and judgment (Fildes & Goodwin, 2007). This practice illustrates that ultimately organizational decisions are made by the interface of technology and the human decision maker. A recent study of a multi-national pharmaceutical firm, for example, finds that only 50% of the forecasting experts rely on model-based forecasts (Boulaksil & Franses 2009). Similarly, in a survey of firms actively employing quantitative methods, 78% report moderate or high degrees of managerial adjustment to statistically generated forecasts (Sanders & Manrodt 2003).

The well-documented industry practice for forecast development begins with a statistical forecast, which serves as a baseline and is then managerially adjusted (Seifert, et al., 2015; Davydenko & Fildes, 2013; Fildes & Goodwin, 2007). The judgmentally adjusted forecasts are then adopted as final forecasts for operation and supply chain management (Moon, et al., 2003; Oliva & Watson 2011). This adjustment may serve to include information received from other functional areas such as marketing and other information not contained in the data. Although ample empirical evidence suggests that such a practice may deteriorate forecast accuracy as human judgment is subject to various types of cognitive biases (Tversky & Kahneman, 1974), this practice has not abated.

Adjustments serve a variety of purposes such as providing a sense of ownership to the forecaster, illustrating the behavioral complexity of this process. Oliva and Watson (2011) argue that careful design of this forecasting process can actually lead to improved forecasts. This, however, requires integrating Organizational Behavior and Operations Management in order to create innovations in understanding of forecasting practice.

Other aspects of forecasting further demonstrate the importance of behavioral research in operations management. Mortiz, Siemens, and Kremer (2014) analyzed how differences in the abilities of individual decision makers, as measured by a cognitive reflection test, affect performance in judgmental time-series forecasting. The authors find that decision makers with the ability to balance intuitive judgment with cognitive deliberation tended to have lower forecast errors.

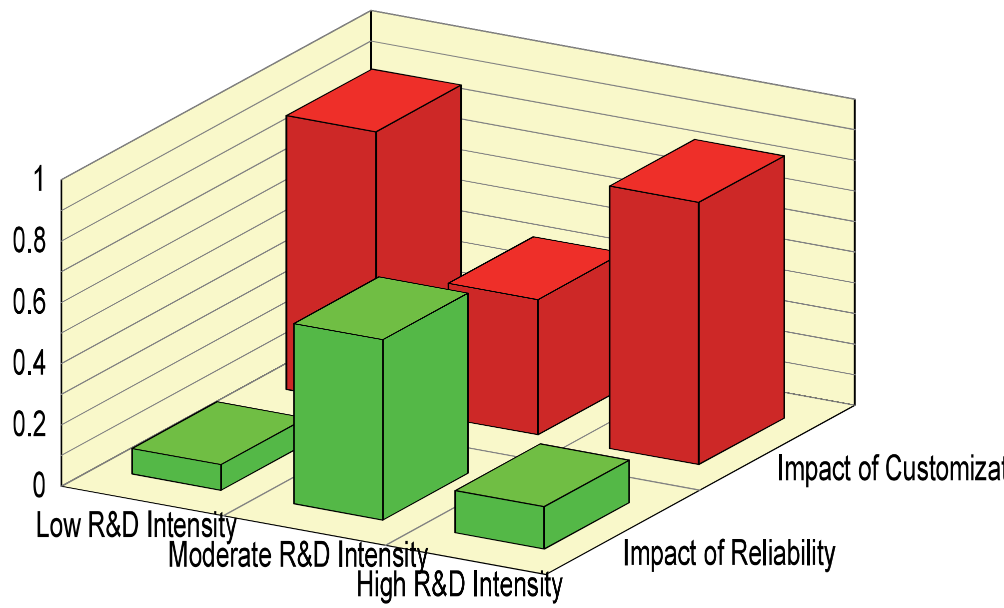
The linkage between Organizational Behavior and Operations Management has been further expanded upon in a more recent study looking at the role of individual decision makers in supply chain performance (Narayanan & Moritz, 2015). While prior research has investigated structural and environmental factors that contribute to the bullwhip effect, Narayanan and Moritz's study looks at the cognitive profile of decision makers and its impact on the bullwhip effect. Within the operations management context the authors use organizational behavior theory to create cognitive profiles of decision makers. Results show that the specific decision tendency to underweight the supply line is linked to the individual's level of cognitive reflection. Simply put, results show that the cognitive profile of decision makers contributes to the bullwhip effect. In fact, supply chains managed by individuals with higher cognitive reflection are found to have lower costs, exhibit less order variance, and have lower demand amplification. This research and findings move beyond the siloed lens and present a more complete composite of the operations problem and develop innovative solutions not possible from a single disciplinary view.

Marketing & OM

The second exemplar is the collaboration of research traditions in Operations Management and Marketing. In practice, marketing and OM are naturally intertwined, and a business cannot exist with neither. At the simplest level the former understands what the customer wants while the latter makes it happen. One example of this bi-disciplinary interface is an article published by Johnson and Ettlie (2001). The Utterback-Abernathy model of evolution of the productive segment through stages of innovation in product and process was the operations management contribution, and the marketing contribution was the theory of quality based on things-gone-wrong (reliability) and things-gone-right (customization). Predictions made by the UA model were confirmed in the secondary data analysis of the ACSI (American Customer Satisfaction Index). These results are presented in Figure 1 below.

FIGURE 1

THE RELATIVE IMPACT OF R&D FOR CUSTOMIZATION VERSUS RELIABILITY ON CUSTOMER SATISFACTION



CATEGORIES OF R&D INTENSITY

This article and research were the culmination of a bi-discipline collaboration, which began with joint MBA cross-section teaching, which led to a preliminary research project (Ettlie & Johnson, 1994) and ultimately the joint work (Johnson and Ettlie, 2001) which was also based on a previous POMS article (Ettlie, 1997).

Yet another example of the bi-disciplinary collaboration between OM and Marketing, and the failure of academia to innovate, is showcased through S&OP. Sales and Operations Planning (S&OP) is a monthly organizational business planning process that creates functional synchronization, most notably between the sales and the production plan (Moon, et. al., 2003; Fildes & Goodwin, 2007). S&OP is one of the top priorities for corporate leaders as evidenced by the proliferation of the topic at practitioner conferences and in practitioner journals (Kato, 2015). S&OP represents the development of a joint plan between the traditional sales forecast and the production or aggregate plan of OM. In contrast to the academic literature, the practitioner literature is awash with descriptions and recommendations as to how “Sales and Operations Planning (S&OP)” processes can be used to effectively integrate cross-functional information to generate agreement on final forecasts and production plans (Lapide, 2007; Stahl, 2010). Yet, there is virtually no academic writing or research on S&OP. Recalling Marshal Fisher’s 2005 POMS plenary talk, the “big idea” has yet again come from industry while academia continues to debate its siloed theoretical development.

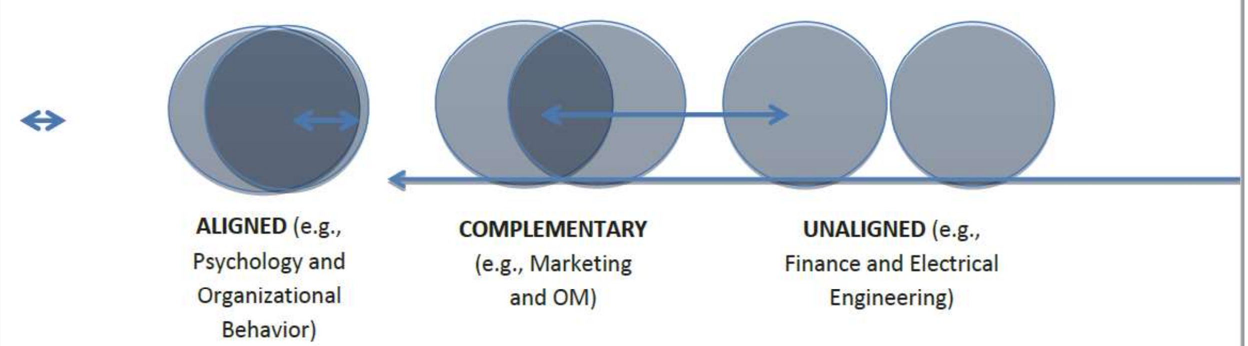
This example presents a tremendous opportunity for academia, indeed a blue ocean in innovation research. Here, small efforts can potentially result in large gains given the dearth of formal scholarly work. Bi-disciplinary research between marketing and operations management in S&OP, for example, could develop theoretical breakthroughs on innovation using rigorous research where currently we only have anecdotal evidence. In fact, marketing and OM represent a complementary locus of centroids of disciplines, in contrast to nearly completely aligned or unaligned domain centroids. As such they represent a fruitful opportunity for the creation of new and useful theory. S&OP is just one such example of where bi-disciplinary research has the potential to result in tremendous gains.

CONCLUSION AND RECOMMENDATIONS

We now summarize our conclusions and recommend a modest proposal for a bi-disciplinary approach to accelerated innovation theory building, theory testing and administrative experimentation. *First*, we recommend that local dictates rule which disciplines are most promising for funding of integrated work. These disciplines should have a reasonable distance between their centroid philosophies in theory and method. In particular, we suggest that adjacent disciplines are probably not likely to yield accelerated outcomes. We suggest that too much topical and methodological overlap is duplicative, lowering the opportunity for generating new theory for the innovation process. It is too much “common ground” with little opportunity to innovate. By contrast, lack of topical and methodological overlap does not provide enough “common ground” or discipline intersection to provide the needed opportunity for generating new theory. It is difficult to even begin a dialogue let alone engage in innovation (Sanders, Zacharia, & Fugate, 2013). Figure 2 shows a representation of these alternative states.

FIGURE 2

MAPPING BI-DISCIPLINARY DOMAIN CENTROIDS



Second, we recommend a virtual community of bi-disciplines be established with rotating coordinating responsibility. The more bi-discipline efforts join this virtual consortium and periodically meet in person, the more rapid the potential for accelerated development of useful theory.

Third, we suggest that both local as well as national and/or international funding of these efforts be shared accordingly. The funding model would be local (e.g., university-based or state-based) for any given bi-disciplinary pair, and national or international for the virtual consortium. The recent announcement of the Cornell Institute for Heathy Futures (http://ihf.cornell.edu/),which celebrates the intersection of hospitality, health and design is a promising sign that the authors are not alone in calling for action on the bi-discipline investigation front working to calibrate research questions with practitioners.

Fourth, we might make modest beginnings on bi-discipline exploration as in the case of Johnson and Ettlie (2001) which began with joint MBA section cross-teaching of units on product development. As mentioned earlier, this lead to a first article and then another representing true joint, bi-discipline research.

Fifth and finally, we need to periodically merge our professional association meeting tracks with the intention of linking potential bi-discipline researchers. A bold experiment on the part of POMS and Academy of Management leadership would be to merge the TIM division track allowance at an annual meeting (not before or after but during the regular meeting), either as preconference PDW work or regular symposium sessions. Further, we need to define disciplines involved by the research problems and questions we confront.

Again, we believe these are relatively modest proposals that represent a start at making true progress in accelerating theory development and testing as well as administrative experiments. For the first time in all of our professional disciplines we can exploit virtual collaboration and seek meaningful integration of the thought process and problem solving.

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