

IN SEARCH OF AMBIDEXTROUS PROFESSORS

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A social system left to itself gravitates toward equilibrium—maximum entropy so to speak. The position of maximum entropy for a professional school is the one in which the faculty trained in the profession is absorbed in the culture of the profession, whereas the faculty trained in an underlying discipline is absorbed in the culture of that discipline, leaving a deep gulf between them. . . . [This is] an equilibrium that means mediocrity for the professional school and inability to fulfill its special functions. All efforts to avoid this state of death must aim at lowering the barriers that impede communication between the discipline-oriented and the profession-oriented wings of the faculty.

-Herbert Simon, 1967

There is a growing concern within the Academy that a not insignificant gap exists between management research and practice. For example, in a recent survey of AOM members, Shapiro, Kirkman, and Courtney (2007) reported the existence and operation of not one but two types of gaps—the “lost in translation” gap (when managerially relevant research fails to reach practitioners) and the “lost before translation” gap (when managerially relevant research is not undertaken by academics). Similarly, in an article that generated quite a bit of controversy, Bennis and O’Toole (2005) claimed that business schools have lost their way in the last 30 years primarily because they have focused too much on doing “scientific” research and ended up hiring professors with limited real-world experience who produce research and teaching that is not relevant to managers. Other studies have shown not only that practicing managers do not turn to aca-

demical research for guidance¹ but also that academic researchers themselves rarely turn to managers for developing their research questions or research agenda (e.g., Abrahamson, 1996; Mowday, 1997). In fact, as Oviatt and Miller (1989) pointed out, cooperation between academics and managers is so rare that when it happens, it makes national newspaper headlines.²

As a result of such uneasiness, several remedies—some more radical than others—have been proposed to close the perceived gap. For example, Rynes, Bartunek, and Daft (2001) built upon the work of Nonaka and Takeuchi (1995) to argue that all four forms of knowledge creation (that is, socialization, externalization, combination, and internalization) should be actively pursued through tactics such as joint symposia between academics and practitioners, consulting relationships, sabbaticals in industry, action research, joint academic-practitioner research teams, and training with distributed practice. Similarly, a recent report by a task force of the Association to Advance Collegiate Schools of Business International (AACSB, 2007) proposed seven action steps (such as developing an awards program to recognize high-impact faculty research and strengthening interactions between faculty and managers) as ways forward. Vermeulen (2005) suggested that the best way to bridge the gap is to ask research questions that are important to managers but to pursue the answer in an academically rigorous way. But he also lamented the fact that this will not happen unless the underlying incentives and culture of the academic system are first changed to encourage this type of research. Other ideas and proposals have come from a variety of academics and practitioners (e.g., Aldag, 1997; Garland, 1999;

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¹ For example, when asked to comment on how “customers” of managerial research view this kind of research, Raymond Price, a senior executive at Hewlett-Packard with a Ph.D. in organizational behavior from Stanford, declared: “In the past I had not been a satisfied customer and now I could hardly be considered a customer” (Price, 1995: 102).

² E. Bean: “By Practicing What It Preaches, Fuqua Wins a Place Among Top Business Schools” (*Wall Street Journal*, May 13, 1986: 35).

Hambrick, 1994; Oviatt & Miller, 1989; Shapiro et al., 2007).

Although I fully subscribe to the goal of making management research (more) managerially relevant, I have two concerns with the way the debate on how to achieve this goal has evolved in the AOM. First, management scholars have jumped into recommendations without first defining clearly what exactly managerially relevant research is or how serious the gap between research and practice really is. And second, I am worried that some of the remedies proposed are worse than the disease. If we don't stem their growth, they will lead business schools to what Herbert Simon (1997) called "a state of death." In what follows, I first comment on two of the remedies proposed and then argue that the problem can be resolved in a satisfactory way only if we take a more holistic view of what managerially relevant research really is.

REASONS FOR THE PERSISTENCE OF THE GAP

The perceived persistence of the gap between research and managerial practice is truly remarkable, if not mysterious! The ideas and advice that have been proposed over the past 30 years to close it are numerous, sensible, practical, and not particularly resource-expensive. The people arguing for such change are senior figures in the Academy who do not lack credibility. The willingness and desire to make the Academy more relevant is real and well intentioned. Yet, the gap persists—or so it seems. What can explain such an outcome?

Blame the System

Several culprits have been identified in the literature. The first and most obvious one is that the underlying incentives as well as the cultures and values of the academic system do not encourage managerially relevant research (e.g., Vermeulen, 2005). For example, Bennis and O'Toole claimed this: "The dirty little secret at most of today's best business schools is that they chiefly serve the faculty's research interests and career goals, with too little regard for the needs of other stakeholders" (2005: 103). This explanation also featured prominently in the responses given in the survey of AOM members undertaken by Shapiro et al. (2007). It is a sensible point that builds upon the work of Senge, who argued that the underlying structure of a system creates the behaviors we see in that system (1990). If one defines the "underlying structure" to include an organization's cultures, structures, val-

ues, and incentives, then the implication is clear: the underlying structure of the academic system does not encourage managerially relevant research, and only a *systemic change* can bring about the desired results.

Although the argument is well taken, it still does not help one understand why we in the field of management keep such a system in place. If it really produces research that lacks relevance, why don't we simply change it? Even worse, if, as many believe, the gap between theory and practice is getting bigger and bigger, then our system is getting worse and worse. What explains our inability to change such a sick system—especially in the face of increasing pressure from our capital providers (such as the National Science Foundation) to do so? Other than arrogance and complacency, could there be a perfectly good reason why we refuse to change our underlying cultures and incentives? Could it be that we *need* the current system and fear that any attempts to alter it will backfire on us?

Oviatt and Miller (1989) articulated a possible explanation for such inaction. Their basic argument is that the lack of pressure to change a system that serves only academics well (rather than our "customers") can be traced back to the economics of the business education industry. Because of a growing market that creates excess demand for business school professors as well as an industry structure (i.e., the five forces of the academic industry) that gives academics the bargaining power, most academics will remain intransigent in the face of calls for more managerially relevant research. Only a drop in the demand for business professors could lead to changes in the system.

My own position is that part of our inaction on this front stems from fear—the fear that in our attempt to alter our current system, we might destroy the very thing that made us (i.e., the senior faculty) so successful. Framed in Porter's (1980) language, the fear is that by attempting to develop incentives and systems that encourage both academic and managerially relevant research, we may get ourselves "stuck in the middle." As I explain below, this is a legitimate concern *only* if we attempt to change the system in a radical way. I believe that doing so would be a mistake: the system does not need radical change—minor "adjustments" are all that are needed. I will propose a few such adjustments to the system at the end of my essay.

What I am advocating is for us to be careful not to throw the baby out with the bath water. This is the same message that Bill Gates delivered at his Harvard commencement speech in June 2007, when he declared that we need to rewrite the rules of the

capitalist system to solve inequity in the world. He asked for not a total overhaul of the system but some reengineering of what exists today. He called that “creative capitalism.” As reported by Grossman (2007: 42), Gates said, “Capitalism has really triumphed in this incredible way and certainly for at least a billion people, it’s done a spectacular job and alternative systems have not. Yet there’s this strong feeling that getting that system to direct itself to the right problem—there’s more that can be done.” He then asked for adjustments to the system so as to get it directed in the right way. The academic system in the field of management needs similar adjustments, *not* major surgery.

It’s Difficult to Do Both

Yet another reason given for the perceived persistence of the gap rests on the rather obvious point that it is hard to be both rigorous and relevant (Staw, 1995). This dilemma surfaces because the set of skills, values, mind-sets, and attitudes that are needed to conduct rigorous academic research are fundamentally *different* from the set of skills, values, and attitudes needed to conduct managerial research.

For example, academics value a carefully crafted argument that builds upon the existing literature, is supported empirically through careful data collection and rigorous analysis, and adds incrementally to the body of knowledge. Managerially relevant research, on the other hand, tends to emphasize a “big idea” and is not particularly concerned with rigorous analysis or the conventional requirements of normal, positivist science. Similarly, academic researchers tend to see other academics as their reference group—they aim to communicate (primarily) with fellow academics and derive their senses of worth and identity from this reference group. Researchers engaged in managerially relevant research, on the other hand, see managers as their audience and aim to develop insights that help managers understand themselves and their organizations better.

According to this argument, not only are the types of skills and mind-sets needed for academic research *different* from the skills needed for managerially relevant research; the two skill sets also *conflict*. Porter (1996) talked about the problem of trade-offs and conflicts in strategy. In his language, what the argument here says is that the activities required to be an academic researcher are incompatible with the activities required to compete in the managerially relevant market because of various trade-offs that exist between the two ways of doing research. By trying to do both, an academic

researcher runs the risk of degrading the value of his or her existing research and may end up paying a huge straddling cost (Porter, 1996).

Porter (1996) identified three main reasons that give rise to trade-offs in strategy. Applying them to academia, one first has trade-offs that arise from inconsistencies in an academic’s *image or reputation*. A researcher who tries to simultaneously offer two different, inconsistent kinds of value runs the risk of damaging her or his existing image and reputation. Second, trade-offs occur because the values and aspirations of an academic researcher are incompatible with the *values and aspirations* of a managerial researcher. Finally, trade-offs arise owing to the limits a person faces in single-handedly trying to *coordinate and control incompatible sets* of activities.

A DANGEROUS REMEDY

According to the above argument, business schools keep hiring freshly minted Ph.D.’s who are trained and socialized to do rigorous academic research. Since they do not possess the skills and mind-sets needed for managerially relevant research, nor are they able to adopt such conflicting mind-sets, it is no wonder that the gap between theory and practice keeps growing.

This formulation had led some people to propose that a solution to this problem was to recognize that the challenge of doing both good academic research and good managerially relevant research is too formidable for academic researchers. They should, therefore, focus their energy on what they do best and “subcontract” the production and dissemination of managerially relevant research to “others” who are better suited for this task. These others could be specially hired professors of management practice who are entrusted with producing managerially relevant research and rewarded for its production; or they could be schools focused exclusively on managerial relevance (like IMD in Switzerland or Ashridge in the United Kingdom); or they could be consulting firms, a few of which—such as Monitor—have already started moving into the business education market.

Proponents of this “solution” find support for it in the literature on *amdidexterity*, where structural separation with limited integrative mechanisms is offered as the way to do two contradictory things at the same time. This idea is also made more palatable by the realization that this business model exists and is widely accepted in what Caves (2002) called the “creative industries”—movies, theatre, art galleries, book publishing, and music publishing. Caves (2002) reported that in such creative

industries, one type of firm (usually small-scale “pickers”) concentrates on the selection and development of new creative talents, while another type of firm (usually large-scale “promoters”) undertakes the packaging and distribution of the products. Similarly, Birkinshaw (2007) found that the people who deliver commercial outcomes tend to be rather different from those who are accustomed to producing academic outcomes.

Elegant as this argument might seem, it could lead business schools down a slippery road if adopted in academia. First, allowing academics to get on with their careers without worrying about the relevance of their work (or without interacting with managers) is like isolating the R&D department of a company from the customer and the market: the end result will be products that nobody wants. That is exactly the kind of result that Simon (1967) warned about. Second, if these professors of management practice are not academics themselves, the inevitable result would be the creation of second-class citizens within business schools, which would be bad for “them” and bad for “our” culture. And third, if these professors of management practice had not been trained in carrying out rigorous academic research (as many retired CEOs who become such professors are not), the field of management could very well end up with findings that are simply wrong (Rosenzweig, 2007). How many popular beliefs or ideas have gained managerial attention in the last 20 years, only to be debunked as half-truths or even total errors (Pfeffer & Sutton, 2006)? Relevant they may be, but that doesn’t make them correct. How could we academics expect people who have not been trained in the art and science of rigorous academic research to produce rigorous research findings? Such a strategy could spell disaster for us and for our schools.

WHERE IS THE GAP?

Before embarking on radical surgery, such as changing the academic incentive system or subcontracting managerially relevant research to specialists—both of which are, in my opinion, remedies that are worse than the disease—it may be worthwhile to step back and take a fresh look at the problem.

The aspiration is to produce and disseminate managerially relevant research. The question is, What exactly is managerially relevant research? Let us take a noncontroversial definition as our basis: it is research that develops insights that help managers understand themselves and their organizations better. This research may be disseminated to managers either through publications or through other

means, such as classroom teaching, speeches, consulting, and so on. Now, let us consider another simple question: How many of you reading this essay have actually used your research findings in your classroom teaching?

I have not done a scientific survey on this question, but I would find it surprising if most of us did not answer this question in the affirmative. Certainly in my own small network of AOM colleagues, I cannot think of a single person who has not communicated her or his research findings to students. I do not mean that all of these colleagues’ teaching is about their research findings, nor do I mean that everything they have researched has found its way into the classroom. Rather, they utilize their research findings whenever they teach things related to what they are researching. Let me, therefore, propose the following: if you have communicated some of your research findings to your students, then by definition you *are* doing managerially relevant research.

If that is the case, why do we still feel that there is a big gap between theory and practice? I believe that one reason lies in an overly narrow definition of managerial relevance. Scholars have somehow come to believe that this is only research that gets published in journals like the *Harvard Business Review (HBR)* or the *Sloan Management Review (SMR)* or in books. Yet we all know that these are only a small subset of the available outlets for the dissemination of our research. Plus we all know that young academics, who are under severe time pressure to produce enough academic publications to gain tenure, cannot afford the time commitment needed to publish their work in *HBR* or as a book. Yet the same academics who couldn’t care less about publishing in *HBR* continue to do managerially relevant research and continue to disseminate their research findings through their teaching.

Another reason why we researchers perceive a gap is because we associate it with other concerns, some legitimate, some not, about the academic system. For example, several academic studies bemoaning the existence of a gap have reported that managers complain that they cannot even read our journals—a clear indication, according to them, that we fail to produce relevant research. But who says that the academic journals are the appropriate vehicle for communicating research results to managers? Whatever happened to classroom teaching or executive education or conference speeches? Similarly, academics and practitioners complain that many of us develop our research questions without talking to managers. But since when has such discussion become the only way for academics to develop research questions that are relevant to man-

agers? Do authors of murder novels first talk to murderers before writing their novels? Can we not develop relevant research questions simply by reading the business press or by talking to our executive students?

A final reason for the perceived gap is the (mistaken) belief that managerially relevant research is all about developing great ideas that somehow redirect managerial thinking or attract wide attention and popular acclaim—*In Search of Excellence* and *Good to Great* come readily to mind. Although great ideas are always welcome, the truth of the matter is that most good managerial research is not of this kind. Rather, one should aim to ask managerially interesting questions, shed some light on them, and help progress the thinking on these issues in an incremental way. Successfully debunking popular myths in a rigorous way or helping people think about an issue in a better way are the hallmarks of good, managerially relevant research.

IMPLICATIONS

I do not believe that dismantling scholarly incentive systems in the hope of generating more or better managerially relevant research is necessary (or feasible). Nor do I believe that creating a two-tier system, with academics on one end producing academic research and adjuncts or professors of management practice on the other end producing managerially relevant research, will serve the field well. There's no need for such radical surgery! Rather, let me propose four simple ideas.

First, we need to recognize that young academics can be engaged in managerially relevant research without publishing this research in *HBR* or the *Academy of Management Perspectives* or other managerial journals. Rather than bemoan the fact, we should encourage young academics to avoid the time-consuming exercise of trying to publish their research in managerial journals and strive instead to bring their research findings into their classroom teaching. Books and management-oriented publications could be the preferred dissemination outlets *after* tenure. Before tenure, classroom teaching should be the preferred dissemination outlet. If we truly believe this, perhaps we could develop incentives that reward people who utilize their research findings in their teaching. Or maybe business schools should "forbid" faculty from developing and teaching elective courses unless they bring their own research findings into their elective teaching.

Second, we need to appreciate that there are different *types* of managerially relevant research. Translating our (academic) research into manage-

rial lessons is one type; successfully debunking popular myths or discrediting "conventional wisdom" is another type; developing new theoretical rationales for current business phenomena is yet another. Not everybody can do everything, so different researchers should choose the type that serves them best. *One* type of managerially relevant research is the one intended to develop grand new theories without the necessary empirical evidence to support them. The idea is to develop these theories and then have future researchers empirically test them for accuracy and validity (think of Darwin's theory of evolution). This type of research requires a writer to take creative leaps and offer ideas and insights not immediately supported by available data. This is risky business, and we should encourage young colleagues to avoid this type of research. It is better suited to academics who can afford to take such risks—perhaps academics who have already received tenure in the system.

Third, many fellow academics have already provided a lot of useful advice on how to make our research more relevant; this advice includes asking managerially interesting questions and pursuing them in a rigorous way, and testing our research questions and ideas on managers. Aldag (1997), Bennis and O'Toole (2005), Garland (1999), Hambrick (1994), Oviatt and Miller (1989), Rynes, Bartunek, and Daft (2001), Shapiro, Kirkman, and Courtney (2007), and Vermeulen (2005) have all offered advice along these lines. Academics who want to become better at this kind of work should take this advice on board. But at the same time, we need to remember that we are now advising companies to pursue "open innovation"—that is, to search for ideas outside their firm boundaries. Perhaps we can take some of our own medicine?

At the organizational level, what "open innovation" implies for business schools is the need to go beyond one's school boundaries for help. Either through allying with external bodies (such as consulting firms) or by working closely with corporate universities, a business school can bring its academic faculty closer to the managerial world. At the individual level, open innovation implies that researchers need to begin working on research projects with nonacademics, such as consultants and managers, or with academics from other disciplines.

Finally, we need to appreciate that the challenge of doing good, managerially relevant research is very difficult. It requires ambidextrous mind-sets and attitudes. Therefore, we should look for insights on what to do in the existing literature on ambidexterity. According to Tushman and

O'Reilly (2004), an organization can become ambidextrous by creating a unit separate from itself, the parent, with each focusing on what it does best. High differentiation is allowed to exist between the two, but they are also kept together through a common vision and targeted but limited integrative mechanisms.

What this implies for academic researchers who aspire and want to do managerially relevant research is *temporal separation* in the *type* of managerially relevant research they pursue and in the *outlets* they use to disseminate their work. Before tenure, academic researchers ought to focus primarily on doing research designed to translate their research findings into managerial insights and then disseminating it primarily through their teaching. After tenure, they could move into the "riskier" type of managerial research that I discussed above, and they should aim to publish it in books. The integrative mechanisms that they utilize would be the knowledge and research skills that they have accumulated as academic researchers. For this strategy to work, many business schools may need to change how tenured faculty are evaluated and rewarded, but this should not be as difficult a task as changing the whole system.

Perhaps none of my proposed ideas are new, and certainly they are not radical. But they *could* make a big difference. And that would be the best outcome to aim for: a major change achieved not by revolution, but by evolution.

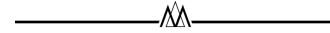
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