© Academy of Management Journal
2007, Vol. 50, No. 6, 1334–1345.

# A MODEST PROPOSAL: HOW WE MIGHT CHANGE THE PROCESS AND PRODUCT OF MANAGERIAL RESEARCH

JEFFREY PFEFFER
Stanford University

I, and possibly many of my colleagues in the Academy of Management, have a dream for the future of management research—an aspiration that builds on, but is not satisfied with, the considerable accomplishments to date. I would summarize that dream for management research as including the following components: (1) having more effect on the actual practice of management in organizations in both the private and public sectors; (2) being at least as much "at the table" and as influential in the formulation of policy in both the public domain and private sector as our sister social science disciplines, and more specifically, on a par with economics; and (3) being as connected to, as engaged with, and as relevant for our profession—management—as our sister professional schools in fields such as engineering, education, and medicine are to their professions and constituencies.

The level of engagement contemplated might include actually creating or partnering in the development of business practices and techniques and thereby being an active participant in the management innovation process. This is the principle behind the Management Innovation Lab, recently founded at London Business School by Gary Hamel and Julian Birkinshaw, with the explicit objective of creating partnerships between academics and businesses and their leaders to cocreate better management practices. This role as a possible source, not merely an evaluator, of professional innovation is something that one sees in engineering and medical schools, for instance, where companies, products, technologies and devices, and drugs come, on occasion, from the schools, their faculties, and the research that they do. Many universities today have technology-licensing units that actively work to ensure the commercialization of knowledge on their campuses, at times in partnership with the faculty inventors. Although I don't know of any systematic data, I suspect that technology-licensing offices have only minimal interaction with business schools, at least in terms of commercializing research or capitalizing on the ideas produced in those places.

There is evidence that some of the Academy of Management's current and past leadership shares at least some elements of this aspiration. The goal of increased impact from our published research is implied in Sara Rynes's charge (to me and the other commentators in this 50th anniversary editors' forum) to consider "how management research might change to have maximal impact on the future of management." The goal of affecting public policy or at least public discussion is implied in the Academy's hiring of a public relations firm a number of years ago in an effort to get research findings more widely disseminated to a broader audience. Many presidential addresses given at the AOM's annual meeting (e.g., Hambrick, 1994; Pearce, 2004) and other articles (e.g., Van de Ven & Johnson, 2006) have considered the effect of the Academy and its activities on the larger society and business and the connections between the worlds of theory and practice. The recent effort to articulate and advance an agenda of evidence-based management (Rousseau, 2006) has as a goal both figuring out what we know and having that knowledge form the foundation for decisions and actions in both the public and private sectors.

In this paper, I argue that we have historically not done particularly well in fulfilling these aspirations. The structure and processes governing both the careers of academics and the prepublication review of their work limit the influence of management research on practice, social policy, and even the terms of public discourse about organizational issues. These limits prevail despite the good intentions and heroic efforts of journal editors and reviewers. If we are serious about our aspirations, we ought to implement what we know about building innovative organizations that are more effective in having knowledge turned into action. Thus, this essay lays out a set of modest—or possibly not so modest—proposals. But before I move on to these topics, it is important to first consider the legitimacy and appropriateness of the goals proposed here for management research.

The author gratefully acknowledges the extremely thoughtful and very helpful suggestions of Sara Rynes, Roy Suddaby, and Christine Quinn Trank.

## WHAT SHOULD THE ROLE OF BUSINESS SCHOOLS AND BUSINESS RESEARCH BE?

### Conflicts of Interest

As Roy Suddaby has so appropriately and persuasively noted in comments that I am sure others would agree with, the aspirations just described are not without controversy. Specifically, some might argue that these objectives for management research are (1) inconsistent with the historical role of business schools and their faculty as evaluators of, but not creators or originators of, business practice, (2) not shared by all in the discipline, and (3) risky in that closer professional interaction and a more active role in management innovation raises the chances that conflicts of interest will arise.

To take the last point first, the risks are clear: medical schools and, for that matter, engineering schools and indeed universities as a whole are fraught with conflicts of interest (see, for instance, Washburn, 2005) and have certainly been, to some substantial extent, captured by the industries they serve. Drug companies that sometimes have medical faculty with equity and/or managerial interests in them have funded clinical trials conducted by these same faculty using university resources. As public support for universities and university research has waned, the importance of private support has grown tremendously. For instance, between 1993 and 2003, industry-sponsored research at the University of California increased 97 percent (Washburn, 2005: 19).

These close relationships between industry and academia almost invariably entail some degree of mutual influence over the research that gets done and the questions that get asked as well as over how that research gets disseminated. Providing one example, Blumenthal and colleagues (Blumenthal, Campbell, Anderson, Causino, & Louis, 1997), surveying life science faculty, found that almost one in five had delayed publication of research results for more than six months sometime during the preceding three years to protect commercial interests and proprietary information. Their analyses showed that participation in academic-industry research relationships and engaging in the commercialization of university research were significant predictors of the decision to delay.

In the management research context, some might worry that in the effort to obtain careers as advisors or to get funding from external organizations, the objectivity with which business school faculty evaluate the ideas and practices of business organizations could be lessened. Therefore, Suddaby argued that we risk losing objectivity in becoming closer participants in the profession and that a more appropriate role for business school researchers is to evaluate the techniques and ideas of others, providing legitimation but not invention or development.

One can make at least three responses to this argument, without for a moment denying its validity. First, as the medical field illustrates, confining research to solely an evaluative rather than a developmental role does not ensure objectivity. For instance, research shows that when drug companies fund studies of the effectiveness of those drugs, the results are, not surprisingly, more favorable for the drugs than when such funding comes from other sources such as government grants (Bakalar [2007], and see Washburn [2005: 84] for an extensive review of studies showing how funding source determines outcome in drug efficacy research). Therefore, remaining solely in an evaluative role rather than assuming an inventing or developmental role does not guarantee an absence of conflict of interest.

Second, business schools have already been captured by companies and managerial interests to some extent, so we may already be paying the cost without reaping many corresponding benefits. Walsh, Weber, and Margolis (2003), for instance, documented the co-occurrence of two trends that may illustrate the existing influence of companies on what we study. They showed that, over the past several decades, a rise in the amount of funding from wealthy alumni and companies was accompanied by a decline in research on topics of social responsibility incorporating dependent variables assessing social impact, rather than economic efficiency. Washburn (2005) cited the comments of a professor occupying the Kmart Chair on marketing at Wayne State University: "'Kmart's attitude always has been: What did we get from you this year? Some professors would say they don't like that position, but for me, it's kept me involved with a major retailer, and it's been a good thing'" (Washburn, 2005: 5). The idea that business schools, heavily dependent on outside donations, are not already influenced by this dependence and are bastions of unsullied objectivity because management research is less engaged in the creation or innovation of management practices seems implausible (e.g., Pfeffer & Salancik, 1978).

And third, in business school disciplines other than management, most notably finance and economics but other areas of study as well, invention and the economic capture of the fruits of that invention are already well advanced. Finance faculty such as Nobel prize winners Myron Scholes and William Sharpe have decamped from academia to found or serve in important roles in firms that employ ideas they have been instrumental in developing, and in some instances, tenured offers in finance have been made to Ph.D.'s working on Wall Street. Economic consulting and forecasting firms have been cofounded by academics who did considerable original research in universities. And the successful strategy consulting firm Monitor has Michael Porter of Harvard Business School as one of its progenitors. A separation between research and practice, between the world of scholars and practitioners, that may possibly be true for some segments of management research or in some countries does not necessarily hold for all elements of business school faculty even today, at least in the United States.

And the argument that the closer connection between business and academics is a change from past practice may not be based on accurate observation. The change in the composition of faculty—from practitioners with experience in business to scholars with doctoral degrees—is relatively recent, occurring in part as a response to the Ford and Carnegie reports in the 1950s that criticized business schools as nothing more than glorified trade schools and pushed for more rigorous social science research. Even today, considering the substantial number of former entrepreneurs and business leaders serving in lecturer roles in schools, it is far from clear that there is as much separation as some believe, although we may not be organized to benefit from these ties. Moreover, the recent evolution of recruiting in management from scholars with degrees from business schools to scholars with degrees from the social science departments of economics, psychology, and sociology suggests that the boundaries between professional practice and business schools are not constant, either across institutions or over time.

All of this is not to say that Suddaby and others aren't correct in noting that some will not approve of aspirations for the role of management research I have articulated and that conflicts of interest aren't a problem. But it is clear, from considering other areas of research within business schools and other professional schools, that we have a choice as to what role we want management research to play and how to construct that role. Data about the effects of various governance arrangements and roles and responsibilities and, of course, values and preferences, should inform these decisions.

### The Place of Management Research in the Marketplace for Ideas

The concept that ideas "compete" in a marketplace and that empirical validity is only one—and maybe not even the most important—characteristic that determines which ideas win seems both useful and valid (see, for instance, Bangerter & Heath, 2004; Barber, Heath, & Odean, 2003; Berger & Heath, 2005). A consideration of the management idea marketplace suggests that management research produced by academics does not fare particularly well in this competition and that management scholars have not been the progenitors of the most important management concepts.

Recently, Mol and Birkenshaw (forthcoming) wrote a book briefly describing what they believe are the world's 50 most important management innovations—things such as lean manufacturing, activity-based cost accounting, T-groups, matrix organizational structures, and brand management. What is noteworthy is that in *none* of the 50 instances did the idea or innovation originate with an academic or in academic research, at least according to their brief descriptions of the innovations and how they evolved.

A similar, although not quite as dismal, picture of the role of academic research emerges in Davenport and Prusak's (2003) review of important contemporary management ideas. They noted that "most business schools . . . have not been very effective in the creation of *useful* business ideas" (Davenport & Prusak, 2003: 81). Pfeffer and Fong (2002) examined business school research's impact using several indicators: the proportion of business best-sellers and the proportion of books cited by *BusinessWeek* as best business books written by business school faculty, citations to books written by faculty compared to citations to other business books, and the proportion of leading management ideas and techniques covered in a Bain survey that originated with business school faculty rather than with a consulting firm or a company. Pfeffer and Fong concluded that business school research was making a modest contribution to management practice compared to research and ideas that came from consulting firms, journalists, and companies.

Without question, each of these compilations and assessments can be criticized for flaws in method, sample, or both. But when a number of different people look at the same basic question—the relative importance of academic research in producing useful managerial ideas or innovations—and come to essentially the same conclusion using different time periods, methods, and criteria, there must be some kernel of truth in the observation that management research has not played as prominent a role in the marketplace of ideas as it might, and possibly should.

## THE SOURCES OF THE PROBLEM

In seeking to understand *why* management research may have had less effect on practice than research in other professional fields, as well as other differences, one undoubtedly encounters many explanations. An idea that ought to be rejected immediately is absence of talent, effort, or attention on the part of individuals engaged in the enterprise. As evidenced by the enormous amount of self-reflection in editorial essays by Eden (2002), Bergh (2003), and Rynes (2006) and the insightful discussions on reviewing, theory, and the scientific process in virtually all recent issues of this journal, *AMJ*'s people are consciously concerned with the reviewing process and with what constitutes a contribution, and they have encouraged the use of multiple methods and a variety of theoretical perspectives. Recent research has shown that where (i.e., in which journal) an article is published has a large effect on its being cited, and the evidence is that the various Academy of Management journals, particularly *AMJ* and *AMR*, are prestigious and have high impact (Judge, Cable, Colbert, & Rynes, 2007; Podsakoff, MacKenzie, Bachrach, & Podsakoff, 2005; Tahai & Meyer, 1999).

In view of what we have learned from the quality movement, it is unlikely that explanations for problems with management research and its impact are to be found by looking to any sort of individual deficiencies or motivations. Instead, more structural explanations that are relevant to article review processes, career reward contingencies, and the effects of competition for status among schools seem like reasonable places to begin an inquiry into what may be going wrong.

### The Journal Review Process

Management research is published mostly in peer-reviewed journals and also in books and publications that are not reviewed, such as magazines, newsletters, and so forth. It is axiomatic that in scientific fields, the journal review process is important. Given the typically extremely high rejection rates (often over 90 percent) in social science journals, including those published by the Academy of Management, journal review essentially determines what papers get into print. In turn, the prestige of a particular publication outlet partially determines how much attention and influence the research it publishes has (Judge et al., 2007). But the journal review process is fraught with problems.

At the most basic level, much management and other social science reviewing is unreliable, a fact that has been well-documented and extensively noted. In one classic study (Peters & Ceci, 1982), 12 previously published papers (retyped with authorship changed to fictional names) were resubmitted to the same prestigious psychology journals that had previously published them. In just 3 of the 12 cases did reviewers even recognize that the already-published papers had been published. Of the other 9 cases, in 8 instances these previously accepted and published works were rejected. Starbuck (2003), with access to 500 pairs of reviews of papers during his tenure as editor of *Administrative Science Quarterly*, reported an interrater correlation of just .12. Miller (2006: 427) presented the results of a number of studies showing, for the most part, fairly low levels of agreement among referees. He noted that "if a journal submission has a true value in some abstract sense, reviewer dissensus indicates a lack of convergence on that value" (Miller, 2006: 426).

Journal reviewing also shows evidence of bias in data indicating that articles that agree with received wisdom are more likely to be accepted than those that challenge dominant belief. So, for instance, Mahoney (1977) found that referees were more likely to reject a study with evidence that disconfirmed widely held hypotheses and were likely to accept an otherwise-identical paper that supported existing beliefs. Goodstein and Brazis (1970) also reported a bias against controversial findings. Kuhn (1972), among others, has commented on the conservative nature of science, noting that scientists hold to old ideas even in the presence of disconfirming evidence. This conservative stance may be useful in the sense that most innovations fail, but it also constrains the likelihood that innovations in practice will arise from the academy. And other forms of bias exist in the review process. Ceci and Peters (1982), for instance, found a bias against authors from low-prestige institutions.

If journal reviewing is unreliable and biased against controversial or novel findings, then two empirical consequences logically follow. It should be the case that many important and new theoretical statements will be made in books or other outlets and not in journals, particularly the most prestigious and selective journals, and that originators of important theoretical work will report difficulty in getting that work published. This is precisely what Campanario (1993) found by examining more than 300 commentaries by authors of classic papers, many of whom reported having trouble getting their ideas into print. As Rynes noted, "It has been widely demonstrated . . . that the social and political forces associated with scientific progress tend toward conservatism" (2006: 1099), which makes it tough to get new ideas into print.

In the organization sciences, many of the major theoretical contributions have appeared in books or in less-prestigious journals. The resource-based view of strategy (Barney, 1991), the industry structure-conduct-performance paradigm in strategic management (Porter, 1979), transactions cost theory (Williamson, 1975), the relationship between agency theory and corporate governance (Jensen & Meckling, 1976), charismatic leadership (Bass, 1985), stakeholder theory (Freeman, 1984), organizational demography (Pfeffer, 1983), escalating commitment to ineffective courses of action (Staw, 1976), and resource dependence theory (Pfeffer & Salancik, 1978)—a partial list of important ideas—were all published either first in books or chapters or, if in articles, in journals that were not top-rated.

Second, unreliability and conservatism in the review process should lessen the differences in quality between papers published in more and less prestigious journals. Glick, Miller, and Cardinal (in press) summarized research that showed, using citation impact as the dependent measure, relatively small differences between more prestigious and less prestigious journals, with less than 10 percent of the variation in article citations being associated with a journal and its quality. Starbuck (2005) documented a decline in the citation advantage of the most prestigious journals during the period from 1981 to 2001. These results are not necessarily inconsistent with those reported by Judge and his coauthors (2007). In that study, the amount of variation accounted for by journal citation rate is less than 20 percent, and these authors did not explore whether the factors affecting article citation changed over time to provide fewer advantages to publishing in more prestigious journals, as Starbuck (2005) argued.

If the compilation of knowledge from research published in academic journals is to form a foundation for policy prescriptions and management practices, another issue in journal reviewing looms large: the overwhelming tendency to publish only results that show significant effects and to not publish papers that fail to find effects or replicate findings. Hubbard and Armstrong, summarizing empirical investigations of this issue, noted, "A number of studies have shown that peer review is biased against the publication of null . . . or so called negative results" (1997: 337). This means that published results are systematically biased in favor of those showing predicted effects, which in turn means that meta-analyses, which invariably rely mostly if not exclusively on published studies, can easily overestimate actual effect sizes. As McDaniel, Rothstein, and Whetzel (2006) noted, procedures exist for attempting to correct for this sampling error in summarizing what existing research implies about effects. Because knowing what doesn't work is often as important as knowing what does, it would be nice to encourage the publication of studies showing what ideas, particularly those that are widely believed, aren't true.

Finally, in the domain of management research, there is a preoccupation with theory as well as an interest in novelty, and both of these tastes appear to take precedence over the task of cumulating a lot of data and knowledge about what is actually going on and what does and doesn't work. Bergh (2003: 136) noted that to get published, one needed to offer empirical *and* theoretical contributions; that the work needed to be "interesting"; and that one screen applied to articles in the review process was "whether a contribution is surprising and unexpected" (2003: 136). Mone and McKinley (1993) commented on the downside of this search for novelty, and Hambrick (2007) wrote persuasively about some of the costs of an excessive preoccupation with theory over facts.

Consider a study of the effect of pay for performance on the quality of care and outcomes for patients suffering heart attacks (Glickman et al., 2007). Given the pressure to tie health system (hospital) reimbursement and physician compensation to performance in health care, the effect of pay-for-performance in this setting is a very important topic. Also, considering the importance of the dependent variable, mortality from heart attacks, the question posed has obvious policy and practical relevance. But there is nothing particularly theoretically "new" or innovative in this (published) study—pay for performance and even the conditions under which it might or might not work is an old topic in management research. The methods are rigorous and the data appropriate but not particularly new or inventive. And there is little "surprising" or "unexpected" in the results: "The pay-for-performance program was not associated with a significant incremental improvement in quality of care or outcomes for acute myocardial infarction" (Glickman et al., 2007: 2373). I doubt if this paper could or would be published in a major management or organizational research journal. And more's the pity, because accumulating evidence on what works, and what doesn't, is fundamentally important for learning about management, improving managerial practice, and actually providing the grist for the meta-analytic mill that the field so loves (Eden, 2002). And that view doesn't even consider the possible benefits for people who have heart attacks and depend on the medical system and its management for their care.

Unfortunately, this quest for "what's new" rather than "what's true" and a lack of interest in data and scientific findings also afflicts practitioner journals in management. As Rynes, Giluk, and Brown (2007) documented, practitioner-oriented publications in human resource management fail to disseminate fundamental and important research findings. As Guest (2007) noted, this is not just a U.S. phenomenon, but one that occurs in the United Kingdom as well. In talking to the editors and publishers of important practitioner-oriented publications, Guest found an interest in stories, case studies, relevant examples, and new ideas, but relatively little commitment to publishing the sorts of summaries and findings that one sees in medicine and that would be required to build an evidence-based practice. The fact that the interest in novelty rather than truth besets both academic and practitioner outlets does not diminish the importance of remedying these biases.

Finally, as Frey (2003) forcefully argued, the editing and reviewing process tends to distort or suppress the original insights and points of view of researchers even if they get their work published. The numbers of management journals and submissions are rising, and—because editors and reviewers in management volunteer the time they spend filling their important roles—reviewers and editors are scarce resources. That gives the occupants of these positions power. Casual observation suggests that when people assume significant editorial responsibilities, citations to their work in the journals they edit tend to go up; this observation could be systematically empirically examined. Editors and reviewers, in positions of power, have a tendency to engage in coproduction, to "help" an author write the paper they want to see or the paper they might have written had they done the particular study. As Frey argued, "Authors only get their papers accepted if they intellectually prostitute themselves by slavishly following the demands" (2003: 205) of people who have no property rights to the journals or, for that matter, to the works they print. The process Frey so eloquently described and that most readers of this article will have lived through almost assuredly curtails innovation and results in a conservative and homogenizing bias in the publication process.

## Academic Career Processes in Business Schools

Nor are problems confined to publication and reviewing issues. Career processes in business schools are not likely to provide incentives encouraging research that will have important effects on public policy or management practice. In fact, as Glick et al. (in press), among others, have documented, career processes are beset by problems and issues about as serious as those that beset the journal review processes. Glick and his colleagues showed that a relatively high proportion (43%) of people with doctoral degrees in management—even degrees from middle- and top-tier schools—leave the field within 16 years of graduation. Further, Glick et al. showed that talent is widely distributed among schools, in that the 32 charter members of the *Academy of Management Journal*'s Hall of Fame were dispersed over 25 universities and a listing of the top 100 scholars as assessed by their citation impact found them in 52 different universities, with only 2 schools having as many as 5 people from the list. Their findings are consistent with career processes of considerable randomness, something to be expected in a field with a low level of paradigm development (Pfeffer, 1993). Although Glick et al. appropriately worried about the consequences of the career processes they describe for people seeking to make a life in the organization sciences, there are also implications for the likelihood of producing important, relevant, managerial research.

As Laura Esserman, MBA, M.D., and director of a breast cancer research and treatment center at the University of California, San Francisco, has noted in comments to Stanford MBA students, research in science now entails much more collaboration than in the past. Research in medicine, engineering, and in many of the physical sciences is likely to be team-based. Teams permit more continuity in research efforts over time (since the research program is less dependent on a single individual), help bring more resources to bear on research questions (by drawing on more people), and permit the gathering and analysis of more data (through the efforts of more people). Larger research teams may also provide the advantage of multiple perspectives and skill sets, an advantage in achieving quality noted long ago in the literature on group decision making (e.g., Davis, 1969). One striking thing about the management innovations described in Mol and Birkenshaw (forthcoming) is the extent to which these ideas often developed across organizations and through the actions and interactions of a number of managers attempting to solve some problem.

Although teams and teamwork are things that management researchers have studied, participation in teams and teamwork is not something many of them *do* as a style of research—and for good reasons. Everyone who has participated in meetings involving the evaluation of people with extensive collaborative research records is familiar with the attempts to parse out the relative contributions of the various people who worked with the focal person and to ensure that the person being evaluated has not somehow been riding on the coattails of others. The penalties for collaboration are reinforced by a criterion often invoked in reviews: "Is this individual one of the 'x' best?" Being part of a research team makes it more difficult to stand out. And the criterion of relative status is inevitably and by definition zero-sum. So the competition for status that is part and parcel of the academic career process in management discourages collaborative research efforts and the building of the sort of laboratories that one sees in the physical sciences and medicine.

As Judge et al. (2007) noted, citations are of growing importance as a metric of performance. This is as true of individual performance as it is for the performance of academic institutions. Judge et al.'s data suggest that articles that are either qualitative reviews or meta-analyses are likely to garner more citations, and their structural equation results indicate that being a meta-analysis is one of the three most important factors affecting citations of an article. However, as Ilgen noted, researchers who tried to manage their careers on the basis of these findings would be led "toward nonempirical reviews and a journal whose primary audience is not management scholars" (2007: 508). So the incentives for career success rooted in maximizing citations have negative effects on the production of research that will affect management practice. The uncertainty and dissensus that characterize the journal review process also have other implications for the best strategies for constructing a life as a management scholar—implications that also may be at variance with the aspirations for management research outlined at the beginning of this article. Consider these recommendations for thinking about research in the context of career strategies from Glick and his colleagues (in press):

> Does the project effectively leverage my prior investments in one of my platforms?
>
> Did my colleagues get excited by my two-minute topic description in the hallway?
>
> Did I stimulate controversy with a quick sketch of the research model? Did I find an anomalous result in the literature that I might be able to explain? How much more work is required to complete this project?

Let me suggest that little about these criteria seems likely to produce research of importance to management practice or public policy, or maybe even research that advances the field's development. And don't misunderstand—I am in no way criticizing the interesting and informative analysis of Glick and his colleagues. Their recommendations follow logically from the data on careers they present. The problem is with the structure of the career process, not with its observers.

## The Competition among Business Schools

A third structural factor that both diminishes innovation and steers research in directions that are at best orthogonal to the concerns of the management profession is the competition among business schools for status and resources. Competition can often produce uniformity and stifle innovation. As DiMaggio and Powell (1983) noted, one source of institutional isomporphism is the quest for legitimacy, which an actor sometimes achieves by trying to look legitimate—or trying to appear similar to others. Doria, Rozanski, and Cohen (2003) commented on how business school curricula have become increasingly similar and how it is far from clear that everyone offering essentially the same product makes much strategic sense. The same thing has happened in research, where the pressure to conform to an American model and publish in United States–based journals has intensified over time (Leung, 2007).

In sort of a story of unintended consequences, this "Americanization" of research began in part with a quest on the part of schools, and in some instances governments, to improve the quality of business schools and managerial research. So, for instance, in the United Kingdom, the "Research Assessment Exercise" ("RAE"; Macdonald & Kam, 2007) is used to periodically evaluate the quality of research being done at U.K. business schools, with the results of these assessments determining research-funding levels for the ensuing years until the exercise is repeated.[1] It turns out that research quality is measured largely by publications and citations in high-quality journals, and virtually all of these are U.S. journals. As Macdonald and Kam noted, "Professional journals are decidedly out of favour" and "quality journals are overwhelmingly seen as publishing mainstream research rather than niche or interdisciplinary work" (2007: 647).

The consequences might be funny if they weren't

[1] In a way not dissimilar to practices in U.S. public schools, this system tends to ensure that the "rich" get richer. Instead of allocating resources to help schools improve, the system rewards those that have already achieved some degree of excellence.

so depressing. For instance, because there are real economic consequences linked to a U.K. school's ranking in the RAE, the competition for faculty—and faculty movement—seems to correspond to the periodicity of the assessments. Because "visiting" faculty—such as high-status individuals from U.S. universities—can be counted if they are doing research with a given school's faculty, there are incentives to regularly invite accomplished individuals who already have published in the "right" places back and to involve them in local research.[2] This behavior is not confined to the United Kingdom. Macdonald and Kam (2007: 644) commented on how schools in Australia and even some in France pay faculty on a piece-rate basis for publications in top journals, with the payments in Australia varying depending on the tier (the ranking) of the journal. Again, this makes perfect sense in a world in which real resources flow depending on faculty publications in prestigious outlets.

This pressure to publish in the ranked journals, which tend to be U.S.-centered or at least U.S.-centric, along with the recruiting of faculty in a global labor market, has contributed to the production of some degree of theoretical isomorphism. As Leung argued with respect to Asian management research, "The downside of the adaptive response to the pressure to publish in highly cited journals is that virtually all Asian management research falls within the confines of well-known Western theories" (2007: 512).

Theoretical isomorphism is, by the way, *not* the same as the consensus that characterizes high levels of paradigm development, and it is also not necessarily going to produce research that is useful for management practice. As institutional theory tells us, often what get imitated and signaled are only the most superficial aspects of something, and these imitated forms have little effect on deep, underlying processes. Meyer and Rowan's (1977) classic study of schools noted how these organizations could appear to be conforming to some institutionalized sense of what schools should look like, even when the formal structures that were imitated had precious little effect on what actually occurred. In management disciplines, what seems to attract imitation in the quest to signal quality is the attraction to theory (Colquitt & Zapata-Phelan, 2007), methodological sophistication and, judging by what journals are highly ranked and which are ranked farther down, a disdain for work that informs or that might inform professional practice and public policy.

## SOME MODEST PROPOSALS

I could go on about these issues at length, because the literature on the topics I have raised is both extensive and extends well back in time. Critiques of business school research, career processes, and peer reviewing are old news (e.g., Porter & McKibbin, 1988). But it was important to lay out some of the issues and an analysis of their root causes in sufficient detail to move to what we might—and note I don't say are—likely to do to change things.

Two general points inform these proposals. First, we ought to put what we know into use. There is extensive research on the innovation process, on what makes ideas influential, on what managers do, and on the problems organizations confront. We ought to use that knowledge in our own management and organizations. Second, the treatment ought to correspond, in some way, to the diagnosis of the problem.

Yet another way to frame a search for what might or should be different is to ask why medicine, engineering, and education are so different from management research. Or to ask why, within business schools, research and teaching about entrepreneurship seem quite different in their degree of connection to professional practice.

These are important questions that could form the basis of substantive research. My sense is that part of the answer in the case of entrepreneurship is the happy co-occurrence of two forces: strong, maybe even overwhelming, student and alumni demand coupled with the persistent inability to find "regular" faculty who could, or would, do research in the traditional mold on this subject. This is not to say that there is no research on entrepreneurship in the typical, elite academic journals or that there couldn't be. Rather, it is to note that a need, coupled with an inability to meet that need using customary approaches, produced—no surprise—innovation! Some of that innovation involved developing cotaught courses, where one of the instructors was a current or former executive from an entrepreneurial company. Some of that innovation entailed hiring people whom we would never have hired as colleagues using traditional criteria, often in lecturer roles—entrepreneurs and executives who were either retired from their primary roles or

[2] I have a few colleagues who visit the same U.K. university each summer. There is nothing malign about this—one could reasonably argue that their presence and collaboration on research will help improve the research skills of the local faculty. However, there is a price paid for this "training": namely, the homogenization of research topics and techniques as the schools in Europe mimic those in the U.S.

who taught part-time. Some of the innovation encompassed changing what we considered to be research, expanding our definitions to encourage clinical, qualitative research and case writing (see Vermeulen, 2007) as well as the use of qualitative field methods more generally. The closer connection with professional practice—not from an occasional lecture or executive program but from the coproduction of teaching and research and more regular interaction—are features that I see, at least to a somewhat greater extent, in engineering, medicine, and education.

These examples suggest that it is possible to be both relevant and rigorous, to serve the scientific enterprise even while doing work that informs policy and practice. So, what might it take, more specifically, to move us in that direction?

If one issue is that current review and status processes don't particularly reward the production of knowledge that anyone cares about, we need to change the rewards and how they are allocated. To take one small example, some years ago the *California Management Review* initiated an award for the best article in each volume. The academic editorial board nominates the three finalists, but a panel of *practitioners* selects the winner. Of course, *CMR* has a different mission than many of our journals, and I am not for a moment claiming this process is perfect. But it does seem that involving practicing professionals, at least to some degree, in determining awards and rewards is one reasonable step toward blending academic and professional values.

The research by Glick and his colleagues (in press) and others illustrates that a shockingly high proportion of papers, even those published in the elite journals, garner *zero* citations, with an even larger percentage obtaining very few. If we take these data seriously and want our tenure and resource allocation criteria to reward impact, then it seems somewhat inconsistent to have faculty evaluation standards that emphasize publishing papers in certain journals over evaluating the effect of an individual's written work, without considering where it first appeared. This logic suggests weighting citations more strongly than number of papers and where they have been published in review processes. And since citations measure scientific impact only imperfectly and, moreover, we are presumably concerned about the effects of research on professional practice above and beyond just its scientific impact, we ought to assess contributions along those broader dimensions measuring the effects of our work as well.

To take a case in point, consider David Kelley. David is the founder and former CEO of IDEO Product Development, a company that has not only won a large number of design awards but one whose ideas about innovation and brainstorming have been recognized and are influential in a large number of companies and professional service firms. Kelley, a member of the National Academy of Engineering and a full professor on the Stanford engineering school faculty, does not have a Ph.D., and I am not sure he has published anything, particularly in peer-reviewed journals. No self-respecting business school using normal academic criteria would have anything to do with him, even though one could plausibly argue that IDEO, through both its design and its management practices and culture, has had more effect on management than scores of academic articles combined. The engineering school may have wisdom that many business schools lack.

If the current reviewing process is at least somewhat unreliable and conservative, there are possible alternatives. Data suggest that innovations in products and services (and there is no reason to believe this would be less true in the domain of ideas and research) often come from peripheral actors who have less invested in existing ways of thinking and doing things (e.g., Christensen, 1998). Reviewing is in the hands of relatively few people who are selected in large measure for their demonstrated socialization into the prevailing topics, theories, and methodologies of a field. But the operation of prediction markets (Surowiecki, 2004), and the practices of companies such as Google that determine new products and new technologies in part through a voting process, speak to the desirability of leaving judgments about the worth of research and ideas open to more people in a more democratic assessment process. In fact, this is just what the Social Science Research Network (SSRN) does. Founded by Michael Jensen, an economist who definitely believes in markets as arbiters of quality and importance and who has had his own troubles in getting some of his more innovative work into print, SSRN posts pretty much everything and then tracks downloads, providing listings of the most frequently downloaded papers. Jensen maintains that leaving publication open and letting the marketplace for ideas determine the usefulness and worth of research papers is preferable to having such decisions reside in the hands of a few people.

The Academy of Management journals, and many others, have made substantial progress in cutting down review times and posting papers much more expeditiously. This is an important effort. Not only is innovation encouraged by rapid prototyping, but also, an inverse linear relationship may exist between the average publication delay in a field and the journal impact factor (Yu, Wang, & Yu, 2005). So it is important to maintain the focus on expediting review turnaround and Internet posting.

If we want to build more collaboration, two targets of intervention emerge. One is the physical design of our buildings. In business schools faced with a chronic shortage of space on university campuses, common areas and meeting rooms are often the first things to go. And business schools typically look more like traditional office buildings than like learning laboratories or places that would facilitate building communities of practice.

The second issue is the implicit message about collaboration: Collaborate, but not too much, and certainly not repeatedly with the same people, particularly if they are more senior than you. Personnel reviews are a necessary part of academic governance. We should, nonetheless, be conscious of the extent to which we may be trading work arrangements that might produce more useful and innovative knowledge for arrangements that make assigning individual credit easier.

## RECONSTRUCTING OUR ENVIRONMENT TO CREATE A DIFFERENT FUTURE

Environments matter. That is one pervasive lesson from our field. And different environments are possible, even in universities. Just look at our colleagues in other professional schools. People have accused me of romanticizing the success of some of the other professional schools, but I don't agree. It is certainly not the case that these schools have "solved" everything once and for all and that everything is perfect. But one cannot observe the advance of medical science and knowledge and its implementation in practice over the past several decades, including the almost 50 percent reduction in death rates from heart disease, and not be impressed. The thrust of the evidence-based medicine movement was to bring the best scientific knowledge to the bedside (e.g., Rosenberg & Anna, 1995). As evidence-based medicine has grown, the practical issues of treatment, diagnosis, and the understanding of disease processes have influenced the research—even the basic science, in some instances—that gets done. In turn, advancing scientific understanding has been implemented in practice and in the drugs and devices that help to deliver care. The link between science and practice is closer, as it seems to be in engineering and computer science as well, but I don't see any less academic legitimacy for these fields. If anything, their science has advanced at least as vigorously (if not more so) than has ours.

In the end, I am optimistic about our ability to do research that affects not only management practice but also public policy. This optimism stems from the remarkable body of knowledge that we and our colleagues in related social sciences have built over the past decades, including the 50 years of this journal. We know a lot about innovation, about the design of social and physical environments, about working in teams, about building communities of practice, and about a lot of other things that are relevant to doing research that is both scientifically and professionally significant. My vision is that we finally use that knowledge—turning our knowing into doing—to design our own systems, environments, and work practices. By so doing, we can act to fulfill the aspirations of many people in the Academy of Management and also provide substantial service to the world in which we live.

## REFERENCES


Bakalar, N. 2007. Review finds drug makers issue more positive studies. ***New York Times,*** February 27: F7.

Bangerter, A., & Heath, C. 2004. The Mozart effect: Tracing the evolution of a scientific legend. ***British Journal of Social Psychology,*** 43: 605–623.

Barber, B. M., Heath, C., & Odean, T. 2003. Good reasons sell: Reason-based choice among individual and group investors in the stock market. ***Management Science,*** 49: 1636–1652.

Barney, J. 1991. Firms, resources, and sustained competitive advantage. ***Journal of Management,*** 17: 99–120.

Bass, B. M. 1985. ***Leadership and performance beyond expectations.*** New York: Collier Macmillan.

Berger, J., & Heath, C. 2005. Idea habitats: How the prevalence of environmental cues influences the success of ideas. ***Cognitive Science,*** 29: 195–221.

Bergh, D. D. 2003. From the editors: Thinking strategically about contribution. ***Academy of Management Journal,*** 46: 135–136.

Blumenthal, D., Campbell, E. G., Anderson, M. S., Causino, N., & Louis, K. K. 1997. Withholding research results in academic life science: Evidence from a national survey of faculty. ***Journal of the American Medical Association,*** 277: 1224–1228.

Campanario, J. M. 1993. Consolation for the scientist: Sometimes it is hard to publish papers that are later highly-cited. ***Social Studies of Science,*** 23: 342–358.

Ceci, S. J., & Peters, D. 1982. Peer review: A study of reliability. ***Change: The Magazine of Higher Learning,*** 14(6): 44–48.

Christensen, C. 1998. ***The innovator's dilemma.*** Boston: Harvard Business School Press.

Colquitt, J. A., & Zapata-Phelan, C. P. 2007. Trends in theory building and theory testing: A five-decade study of *Academy of Management Journal.* ***Academy of Management Journal,*** 50: 1281–1303.

Davenport, T. H., & Prusak, L. 2003. ***What's the big idea?*** Boston: Harvard Business School Press.

Davis, J. H. 1969. ***Group performance.*** Reading, MA: Addison-Wesley.

DiMaggio, P. J., & Powell, W. W. 1983. The iron cage revisited: Institutional isomorphism and collective rationality in organizational fields. ***American Sociological Review,*** 48: 147–160.

Doria, J., Rozanski, H., & Cohen, E. 2003. What business needs from business schools. ***Strategy + Business,*** 32: 39–45.

Eden, D. 2002. From the editors: Replication, meta-analysis, scientific progress, and *AMJ's* publication policy. ***Academy of Management Journal,*** 45: 841–846.

Freeman, R. E. 1984. ***Strategic management: A stakeholder approach***. Boston: Pittman.

Frey, B. 2003. Publishing as prostitution? Choosing between one's own ideas and academic success. ***Public Choice,*** 116: 205–223.

Glick, W. H., Miller, C. C., & Cardinal, L. B. In press. Making a life in the field of organization science. ***Journal of Organizational Behavior.***

Glickman, S. W., Ou, F., DeLong, E. R., Roe, M. T., Lytle, B. L., Mulgund, J., Rumsfeld, J. S., Gibler, W. B., Ohman, E. M., Schulman, K. A., & Peterson, E. D. 2007. Pay for performance, quality of care, and outcomes in acute myocardial infarction. ***Journal of the American Medical Association,*** 297: 2373–2380.

Goodstein, L. D., & Brazis, K. L. 1970. Credibility of psychologists: An empirical study. ***Psychological Reports,*** 27: 835–838.

Guest, D. 2007. Don't shoot the messenger: A wake-up call for academics. ***Academy of Management Journal,*** 50: 1020–1026.

Hambrick, D. C. 1994. Presidential address: What if the Academy actually mattered? ***Academy of Management Review,*** 19: 11–16.

Hambrick, D. C. 2007. The field of management's devotion to theory: Too much of a good thing? ***Academy of Management Journal,*** 50: 1346–1352.

Hubbard, R., & Armstrong, J. S. 1997. Publication bias against null results. ***Psychological Reports,*** 80: 337–338.

Ilgen, D. R. 2007. Citations to management articles: Cautions for the science about advice for the scientist. ***Academy of Management Journal,*** 50: 507–509.

Jensen, M. C., & Meckling, W. H. 1976. Theory of the firm: Managerial behavior, agency costs and ownership structure. ***Journal of Financial Economics,*** 3: 305–360.

Judge, T. A., Cable, D. M., Colbert, A. E., & Rynes, S. L. 2007. What causes a management article to be cited—Article, author, or journal? ***Academy of Management Journal,*** 50: 491–506.

Kuhn, T. S. 1972. ***The structure of scientific revolutions*** (2nd ed.). Chicago: University of Chicago Press.

Leung, K. 2007. The glory and tyranny of citation impact: An East Asian perspective. ***Academy of Management Journal,*** 50: 510–513.

Macdonald, S., & Kam, J. 2007. Ring a ring o' roses: Quality journals and gamesmanship in management studies. ***Journal of Management Studies,*** 44: 640–655.

Mahoney, M. J. 1977. Publication prejudice: An experimental study of confirmatory bias in the peer review system. ***Cognitive Therapy and Research,*** 1: 161–175.

McDaniel, M. A., Whetzel, D. L., Schmidt, F. L., & Maurer, S. D. Publication bias: A case study of four test vendors. ***Personnel Psychology,*** 59: 927–953.

Meyer, J. W., & Rowan, J. 1977. Institutionalized organizations: Formal structure as myth and ceremony. ***American Journal of Sociology,*** 83: 340–363.

Miller, C. C. 2006. Peer review in the organizational and management sciences: Prevalence and effects of reviewer hostility, bias, and dissensus. ***Academy of Management Journal,*** 49: 425–431.

Mol, M. J., & Birkinshaw, J. Forthcoming. ***Giant steps in management: Key management innovations.*** London: Pearson Education.

Mone, M. A., & McKinley, W. 1993. The uniqueness value and its consequences for organization studies. ***Journal of Management Inquiry,*** 2: 284–296.

Pearce, J. L. 2004. What do we know and how do we really know it? ***Academy of Management Review,*** 29: 175–179.

Pfeffer, J. 1983. Organizational demography. In L. L. Cummings & B. M. Staw (Eds.), ***Research in organizational behavior,*** vol. 5: 299–357. Greenwich, CT: JAI Press.

Pfeffer, J. 1993. Barriers to the advance of organizational science: Paradigm development as a dependent variable. ***Academy of Management Review,*** 18: 599–620.

Pfeffer, J., & Fong, C. T. 2002. The end of business schools? Less success than meets the eye. ***Academy of Management Learning and Education,*** 1: 78–95.

Pfeffer, J., & Salancik, G. R. 1978. ***The external control of organizations: A resource dependence perspective.*** New York: Harper & Row.

Podsakoff, P. M., McKenzie, S. B., Bachrach, D. G., & Podsakoff, N. P. 2005. The influence of management journals in the 1980s and 1990s. ***Strategic Management Journal,*** 26: 473–488.

Porter, L. W., & McKibbin, L. E. 1988. ***Management***

***education and development.*** New York: McGraw-Hill.

Porter, M. E. 1979. The structure within industries and companies' performance. ***Review of Economics and Statistics,*** 61: 214–227.

Rosenberg, W., & Anna, D. 1995. Evidence-based medicine: An approach to clinical problem-solving. ***British Medical Journal,*** 310: 1122–1126.

Rousseau, D. M. 2006. Is there such a thing as "evidence-based management"? ***Academy of Management Review,*** 31: 256–269.

Rynes, S. L. 2006. "Getting on board" with *AMJ:* Balancing quality and innovation in the review process. ***Academy of Management Journal,*** 49: 1097–1102.

Rynes, S. L., Giluk, T. L., & Brown, K. G. 2007. The very separate worlds of academic and practitioner periodicals in human resource management: Implications for evidence-based management. ***Academy of Management Journal,*** 50: 987–1008.

Starbuck, W. H. 2003. Turning lemons into lemonade: Where is the value in peer reviews? ***Journal of Management Inquiry,*** 12: 344–351.

Starbuck, W. H. 2005. How much better are the most prestigious journals? The statistics of academic publication. ***Organization Science,*** 16: 180–200.

Staw, B. M. 1976. Knee-deep in the big muddy: A study of escalating commitment to a chosen course of action. ***Organizational Behavior and Human Performance,*** 16: 27–44.

Surowiecki, J. 2004. ***The wisdom of crowds.*** New York: Doubleday.

Tahai, A., & Meyer, M. J. 1999. A revealed preference study of management journals' direct influences. ***Strategic Management Journal,*** 20: 279–296.

Van de Ven, A., & Johnson, P. E. 2006. Knowledge for theory and practice. ***Academy of Management Review,*** 31: 802–821.

Vermeulen, F. 2007. "I shall not remain insignificant": Adding a second loop to matter more. ***Academy of Management Journal,*** 50: 754–761.

Walsh, J. P., Weber, K., & Margolis, J. D. 2003. Social issues and management: Our lost cause found. ***Journal of Management,*** 29: 859–881.

Washburn, J. 2005. ***University Inc.: The corporate corruption of higher education***. New York: Basic Books.

Williamson, O. E. ***Markets and hierarchies.*** New York: Free Press.

Yu, G., Wang, X., & Yu, D. 2005. The influence of publication delays on impact factors. ***Scientometrics,*** 64: 235–246.

**Jeffrey Pfeffer** (*pfeffer_jeffrey@gsb.stanford.edu*) is the Thomas D. Dee II Professor of Organizational Behavior at the Stanford Graduate School of Business. He received his Ph.D. from Stanford University. His research interests include evidence-based management, power and politics in organizations, and economic language and assumptions and their effects on behavior.