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EDITORS' CHOICE

Thoughts on the Making and Remaking of the Management Discipline

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Those who have known me as a colleague are aware that I have long been concerned with the current and future state of the management discipline. I have previously addressed concerns such as (a) the mindlessness of much of our discipline's research, (b) the ever-greater specialization of fields and subfields within our discipline, and (c) the peculiar nature of our academic discipline wherein we treat publications like piecework, emphasizing quantity rather than quality (Bedeian, 1989). The present effort is an exploration of additional concerns that I have been accumulating during the last 15 years. It is my conviction that these concerns, like those previously expressed, have serious implications for the current and immediate future of the management discipline.

My reflections make no pretense at systematic analysis. Rather, they represent a personal statement of someone who has been a participant observer and active "member of the guild" for some time. My comments deal, therefore, with what we as management scholars, taken altogether, truly believe should be the fundamental character of our discipline and its institutions.

The three concerns I wish to address each relate to the sociology of management as a science and are meant to apply to our discipline as a whole, as well as its numerous subfields. Although the management discipline has long looked outside at the world of work, it has seldom looked inside at the nature of its own collective enterprise (for exceptions, see Bedeian & Feild, 1981; Bowers, 1994). I believe most emphatically that the future integrity of the discipline depends on its members being consciously aware and openly debating the discipline's perceived foibles.

It is in the constructive spirit of open debate that the following three concerns are considered: (a) particularism within the management discipline, (b) management journal manuscript review and decision-making practices, and (c) the trivialization of human experience as mirrored in management research.

PARTICULARISM VERSUS UNIVERSALISM

As is true of all academic disciplines, the social structure of the management discipline is a reflection of its scientific norms (Hull, 1990). Although these norms may not always be explicitly detailed (and less rarely elaborated in print), they are part of what Merton

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(1942/1973a) described more than 50 years ago as the "ethos of science" (cf. Berardo, 1989, p. 249). As standards of scientific behavior, the management discipline's norms are transmitted through professional socialization and training, as well as by precept and example.

Underlying the ethos of science is the fundamental canon that truth is to be sought in an independent and rational manner through the use of preestablished, impersonal criteria. This belief finds immediate expression through the norm of universalism in science, which requires that scientific considerations of merit be the sole criterion for judging role performance. Ascriptive criteria based on the personal preferences of judges or particularistic (i.e., nonscientific) characteristics associated with the persons being judged, such as age, race, gender, politics, religion, nationality, class, degree origin, and social attributes, are condemned as irrelevant.

The norm of universalism also finds expression in the belief that, because rewards are a form of recognition bestowed by an academic community, they likewise should be governed by universalistic criteria. In theory, rewards (e.g., prizes, eponymy, fellowships, editorships and editorial appointments, election to professional offices, and citations in the works of others) should follow a purely universalistic-achievement pattern that reflects a commitment to the ideal of advancement by merit, wherein "scholarly performance is the only legitimate claim to recognition" (Caplow & McGee, 1958, p. 224). In such a system, "recognition and esteem accrue to those who have best fulfilled their roles, to those who have made genuinely original contributions to the common stock of knowledge" (Merton, 1957/1973b, p. 293).

Reflecting on the management discipline's normative commitment to universalistic criteria, an increasing number of observers have voiced this concern: In its efforts to constitute itself as a representative body, at least as evidenced in the reward structure of the Academy of Management, has the management discipline incorporated particularistic criteria (e.g., geography, gender, and public/private school distinctions) into its distribution of rewards? To some observers, the answer is "yes." For instance, Pfeffer (1993) recently argued that such transgressions of the scientific ethos underlying the management enterprise are explicit in the academy's slating of officers and selection of editorial board members for academy publications.

Concern has also been increasingly expressed that perceived violations of this ethos are likewise evident within management departments at individual institutions. Purported deviations include forming tenure and promotion committees on the basis of particularistic criteria such as race and gender; requesting "outside letters" of evaluation for tenure and promotion such that a certain number are secured from external reviewers of the same gender and race as the focal candidate; and rewarding tenure and promotion to candidates based on personal or social attributes rather than scientific considerations. With respect to the last instance, in my own experience, faculty members may be inclined to compromise what arguably should be a strictly universalistic-based judgment to what they believe is humaneness, especially if a close associate is being evaluated. This practice, however, obscures an ironic truism. As explained by Simon (1991), "Retaining a faculty member who is less able than others who could be recruited is as inhumane to the (possibly unknown) replacement as it may be humane to the incumbent" (p. 252).

Beyond this, of course, retaining comparatively less able faculty members also works to the detriment of both their institutions and their students. Faculty members who cannot or do not remain current in their fields of study harm their students in both the short and long term. Those with poor classroom skills also do obvious damage. Even more to the point of the present concern is the damage that results to the management discipline as a whole when its ranks swell with faculty occupying positions that could be filled with more gifted incumbents.

In the case of the Academy of Management, if (as Pfeffer, 1993, ventures) its leadership has chosen to consider particularistic criteria as a basis for rewards in a drive toward less "elitism" and more "egalitarianism," our entire discipline consequently suffers. Whereas particularists may argue that a consideration of social and personal factors has desirable effects vis-à-vis representation, a broader issue is involved. Using particularistic criteria in granting recognition is a misallocation of resources. Such abridgments of universalism are not only damaging to the credibility of the management discipline as a scientific enterprise, but also, to the degree that recognition is granted to those less deserving, unfair to individual scholars.

Furthermore, following Beyer (1978), it should be realized that over time even a small number of particularistic decisions may give substantial cumulative advantages to some individuals because such advantages themselves are "convertible into the scarce 'evidence' of competence that makes future selection for

further advantage then based upon competence and, therefore universalistic" (p. 75). In this manner, as Beyer (1978) observed, "a particular advantage can soon be transformed into a universalistic one" (p. 75).

This is not to say that particularism is not an inevitable reality in how researchers discover the real world. Indeed, critics argue that rather than discovering reality, researchers construct it, doing so on the basis of almost everything except "reason, argument, and evidence" (Hull, 1990, p. 344). At the extreme, these critics contend that men cannot help but produce male science; Blacks, African American science; and Episcopalians, Episcopal science. To believe, however, that competing claims to validity cannot be subject to universalistic criteria not only denies the transcendence on which knowledge depends, but also dismisses the very foundation of a representative body, which rests on individual rights, not group status. Moreover, one wonders: If particularistic representativeness is necessary because only women, for instance, can understand women (and presumably only men can understand men), how then do women discover the limitations of male knowledge? In effect, an insistence on the use of particularistic criteria for allocating recognition (in terms of slating officers, selecting editorial board members, or granting tenure and promotion) essentially denies the shared experience of the common world that constrains us all and makes knowledge possible (cf. Silber, 1990, p. 40). Furthermore, it makes science a partisan issue, thereby affecting the very quality of academic life.

What is especially unsettling about an emphasis on particularistic criteria for allocating recognition is that doing so is counter to the Academy of Management's own Code of Ethical Conduct, as well as equal opportunity laws and policies. Both incorporate and reflect a commitment to universalistic criteria. Emphasizing individual rights over group status, the academy's Code of Ethical Conduct (1993) explicitly states that "the responsible professional promotes and protects *the rights of individuals* without regard to race, color, religion, national origin, handicap, sex, sexual orientation, age, political beliefs, or academic ideology" (p. 1697; italics in the original). With regard to equal opportunity laws and policies, Title VII of the 1964 Civil Rights Act explicitly shuns particularistic criteria for universalistic processes, requiring that individuals be judged on their qualifications without regard to race, gender, age, and so on.

Merton's (1942/1973a) notions concerning the ethos of science admittedly rest on an unachievable ideal. To reject this ideal, however, requires an indifference to the implications of scientific outcomes and an abdication of ethical responsibility. Furthermore, to suggest that an individual's career success may have been determined by particularistic criteria is demeaning, casting a pall over true accomplishments and deserved recognition. To contend that this is harmful to society in general and the management discipline in particular hardly seems controversial.

JOURNAL REVIEW PRACTICES

In the management discipline today, as in other sciences, professional journals serve as the major channel for disseminating knowledge. Management journal manuscript review and decision-making practices are a subject of frequent discussion and criticism (e.g., Cousins, 1994). Focusing on the social sciences in general, Bornstein (1991) noted, "Critics have charged that manuscript reviews are . . . , unconstructive . . . , illogical . . . , and nasty," and that "reviewers are reluctant to recommend publication of nonsignificant findings . . . and replications of previous research . . . , and are biased against new, innovative and unpopular ideas . . . , and against unknown authors and less prestigious institutions" (pp. 430-431). Such accusations are sufficiently common, and of such a long-standing nature, to conclude that they reflect an unknown, but palpable, degree of dissatisfaction across the social sciences. My purpose here is not to retread worn ground.

My concern regarding management journal manuscript review and decision-making practices stems from two issues: (a) the extent to which reviews of the same manuscript have few critical points in common, and (b) the question of where detailed editing and reviewing leave off and ghostwriting begins.

Independent Reviews

Closely associated with the ethos of science that Merton (1942/1973a) described are general norms for the evaluation of manuscripts submitted for publication. In general, authors increase the probability of their manuscripts being accepted for publication if they submit a high-quality manuscript grounded in a well-established paradigm (Beyer, Chanove, & Fox, 1995). Herein, of course, lies the rub.

As Pfeffer (1993) noted, "Paradigm development is, after all, an indicator of consensus" (p. 604). This state-

ment suggests that the relative independence of the comments made by journal manuscript reviewers (my first concern) results from the possibility that reviewers are unable to reach a consensus regarding the quality of management research, given the preparadigmatic nature of our discipline as compared with more "mature" sciences (cf. Bornstein, 1991, pp. 431-432). This conjecture squares with the reality that paradigm development has been found to be closely related to journal rejection rates (Pfeffer, 1993, p. 605).

Anyone doubting the relative independence of separate sets of reviewer comments can confirm my concern by taking available reviews for a previously submitted manuscript and attempting to match critical points across reviews. Fiske and Fogg (1990) did exactly this for 402 reviews of 153 manuscripts submitted to 12 American Psychological Association journals. In the modal case, separate reviews had no critical points in common. Rather, reviewers addressed different topics, with the result being that their recommendations regarding manuscript acceptance showed "hardly any agreement."

Whether Fiske and Fogg's (1990) findings generalize to the management discipline is open to speculation. My own guess is that few management scholars would be surprised by the notion of relatively independent or unique reviewer comments. As both a former editor (viz., *Journal of Management*, 1978-1979) and published author, I find such nonoverlap quite common. Although reviewers may be selected for their complementary expertise and thus be somewhat likely to emphasize different aspects of a manuscript, the limited agreement seemingly common among management reviewers does raise certain questions, not the least being whether there is reason to consult presumed experts in making editorial decisions if they cannot agree at a level that exceeds chance (Whitehurst, 1984). If this last sentiment seems harsh, consider that, based on a theoretical model of the editorial review process, Stinchcombe and Ofshe (1969) estimated that about half of the good manuscripts submitted to a journal will be rejected. Along this same line, Marwell (1992) has concluded that "at most major journals in the social sciences, the overall recommendations given by reviewers of the same paper correlate only about .25" (p. iii). This perhaps, at least partially, explains why both Campanario (1993) and Gans and Shepherd (1994) reported that many articles now considered to be classics initially encountered publication problems, being rejected by one or more journals.

A prime consequence of the diversity and uniqueness in reviewer comments and recommendations is the prevalent belief that the decision to accept a submitted manuscript depends on the "luck of the reviewer draw" (Cole, 1989, p. 59). Given anywhere from two to five reviewers, and suspected levels of disagreement, manuscript acceptance or rejection depends on which small sample of reviewers is commissioned. As Cole (1989) observed, "The essential point is, of course, that if the number of reviewers sampled is small, then the estimate of [a] population's opinion of a [manuscript] can be quite biased" (p. 59).

A derivative, but not to be minimized, consequence of the high level of variability in reviewer opinions is a potentially debilitating cynicism on the part of aspiring management scholars (Berardo, 1989, p. 258). This cynicism is evident in the portrayal of the publication process as a game or, in the words of one observer, "a socially approved form of intellectual sadomasochism" (Holbrook, 1986, p. 105). Indeed, guidelines for academic career success (e.g., Bedeian, 1996) and articles offering tips for beginning scholars (e.g., Chambers & Herzberg, 1968; Remus, 1977) and neophyte reviewers (e.g., Remus, 1980) are often presented in satirical or sardonic tone. Such seems to be the fate of a less well-established discipline that remains at a preparadigmatic stage.

Ghostwriting

My second concern related to management journal manuscript review and decision-making practices deals with the question of where detailed editing and reviewing leave off and ghostwriting begins. As expressed by Garfield (1985), this concern "touches upon the domains of writing, editing, and reviewing, as well as the ethics of authorship" (p. 3). No one denies that reviewers are an editor's "insurance policy," providing a reservoir of knowledge no single individual could hope to equal. Demanding reviewers can also protect authors from themselves, as in cases involving slipshod work. Skilled copyediting can likewise enhance a manuscript's clarity. At the same time, as Day (1983) contended, "an author of a paper should be defined as one who takes intellectual responsibility for the [work] being reported" (p. 16). In this sense, all the authors of a manuscript bear responsibility for its quality.

Related to this responsibility is the consideration of authorship attribution. How editors and reviewers who contribute to a manuscript should be acknowledged is not a new question (Garfield, 1985). What does seem to be a relatively new puzzle is whether editors and reviewers ever cross the line of authorship and function as ghostwriters. In my own case, this quandary has risen again and again as editor and reviewer comments have become increasingly more detailed and demanding. It is not at all uncommon, for example, to receive a set of editor and reviewer comments that combine to exceed the length of the submitted manuscript. Editors and reviewers (not to mention copy editors if a manuscript is finally accepted for publication) seem to think nothing of rewriting and even retitling an author's work (for details of one example in a related discipline, see Perrow, 1985). In puzzled response, Garfield (1985) asked, "Should explicit acknowledgement be done on a line-by-line or word-by-word basis?" (p. 8). Acknowledgments could easily reach absurd lengths, recognizing not only significant contributions but also points at which material was omitted on an editor's or a reviewer's demand.

Although this commingling of the legitimate roles of author, editor, and reviewer is troublesome, what is even more disturbing is the final product: a manuscript that its author may not have intended to write, expressing in someone else's language thoughts the author may not have intended to convey, under a title the author may not have selected. Such situations, as humorously described in the accompanying manuscript resubmission letter (see Figure 1) by an anonymous author to an unnamed editor, turn editors and reviewers into ghostwriters, thus blurring the responsibility for a manuscript's content and raising the question of legitimate authorship. This scenario seems to me to push the role of editing far beyond maximizing the clarity of an author's ideas.

Neither novices nor established scholars appear to be exempt from the generally chastening process of reviewing and editing. Although editors are fond of advising authors that they need not "slavishly" conform to reviewer comments and suggestions (Dalton, 1995, p. 614) and to "remember that reviewers are not gods," as Sternberg (1992) noted, the latter fact seems to have escaped some reviewers. In retrospective commentaries, Steve Kerr (1995), author of "On the Folly of Rewarding A, While Hoping for B," and Jerry B. Harvey (1988), author of "The Abilene Paradox: The Management of Agreement," both articles now con-

Dear Sir, Madam, or Other:

Enclosed is our latest version of Ms #85-02-22-RRRRR, that is, the re-re-re-revised revision of our paper. Choke on it. We have again rewritten the entire manuscript from start to finish. We even changed the [bleeping] running head! Hopefully we have suffered enough by now to satisfy even you and your bloodthirsty reviewers.

I shall skip the usual point-by-point description of every single change we made in response to the critiques. After all, it is fairly clear that your reviewers are less interested in details of scientific procedure than in working out their personality problems and sexual frustrations by seeking some kind of demented glee in the sadistic and arbitrary exercise of tyrannical power over hapless authors like ourselves who happen to fall into their clutches. We do understand that, in view of the misanthropic psychopaths you have on your editorial board, you need to keep sending them papers, for if they weren't reviewing manuscripts they'd probably be out mugging old ladies or clubbing baby seals to death. Still, from this batch of reviewers, C was clearly the most hostile, and we request that you not ask him or her to review this revision. Indeed, we have mailed letter bombs to four or five people we suspected of being reviewer C, so if you send the manuscript back to them the review process could be unduly delayed.

Some of the reviewers' comments we couldn't do anything about. For example, if (as reviewer C suggested) several of my recent ancestors were indeed drawn from other species, it is too late to change that. Other suggestions were implemented, however, and the paper has improved and benefitted. Thus, you suggested that we shorten the manuscript by 5 pages, and we were able to accomplish this very effectively by altering the margins and printing the paper in a different font with a smaller typeface. We agree with you that the paper is much better this way.

One perplexing problem was dealing with suggestions #13-28 by Reviewer B. As you may recall (that is, if you even bother reading the reviews before doing your decision letter), that reviewer listed 16 works that he/she felt we should cite in this paper. These were on a variety of different topics, none of which had any relevance to our work that we could see. Indeed, one was an essay on the Spanish-American War from a high school literary magazine. The only common thread was that all 16 were by the same author, presumably someone whom Reviewer B greatly admires and feels should be more widely cited. To handle this, we have modified the Introduction and added, after the review of relevant literature, a subsection entitled "Review of Irrelevant Literature" that discusses these articles and also duly addresses some of the more asinine suggestions in the other reviews.

We hope that you will be pleased with this revision and will finally recognize how urgently deserving of publication this work is. If not, then you are an unscrupulous, depraved monster with no shred of human decency. You ought to be in a cage. May whatever heritage you come from be the butt of the next round of ethnic jokes. If you do accept it, however, we wish to thank you for your patience and wisdom throughout this process and to express our appreciation of your scholarly insights. To repay you, we would be happy to review some manuscripts for you; please send us the next manuscript that any of these reviewers submits to your journal.

Assuming you accept this paper, we would also like to add a footnote acknowledging your help with this manuscript and to point out that we liked the paper much better the way we originally wrote it but you held the editorial shotgun to our heads and forced us to chop, reshuffle, restate, hedge, expand, shorten, and in general convert a meaty paper into stir-fried vegetables. We couldn't, or wouldn't, have done it without your input.

Sincerely,

Figure 1: Sample Cover Letter for Journal Manuscript Resubmissions
Source: Baumeister (1990).

sidered to be management classics, have lamented the initial reception and editorial treatment their works received. Both articles were rejected by the outlets to which they were initially submitted, and both were subject to what Kerr (1995) referred to as a copy editor who "strangled" the very life from his writing style (p. 14). How the high court of history will judge either Kerr or Harvey, based on the ideas and language reflected in their works, remains an open question. The anonymous reviewers and copy editors involved will nevertheless escape authorship responsibility.

TRIVIALIZING HUMAN EXPERIENCE

In extension of an earlier topic on which I have commented elsewhere—that is, the mindlessness of much management research (Bedeian, 1989)—my final concern has to do with the discipline's seemingly unlimited ability to trivialize the most meaningful aspects of human experience. In agreeing with Hirsch (1989), whose theme I am closely following here, I am concerned that our discipline (much like the social sciences in general) is seemingly "obsessed with the rigors of paradigm-building and methodology" (Sanders, 1988, p. 19). Indeed, method seems to have replaced content as its primary concern, as human emotions and feelings have been reduced to techniques of measurement (cf. Hirsch, 1989, p. 74).

Although there is unquestionably a place for method and measurement, they must not be allowed to become more important than human problems and thereby replace human beings as our primary focus. All too often, human emotions and feelings are reduced to abstract statistics and immersed in "oceans of references." At the same time, we have persisted in being "stubbornly reductionistic and mechanistic" (Bevan, 1991, p. 426) in our analyses, as our science-making process has become, in the apt words of Ghiselli (1974), "as stylized as the courting dance of the whooping crane and often . . . just as awkward" (p. 83).

Missing from most of our discipline's research is a "feel for people" (Sanders, 1988, p. 19). To an appreciable degree, humanity has been forsaken for method. Although, as Hirsch (1989, p. 75) noted, method is important, it cannot communicate the human experience. Indeed, in most rigorous management analyses, method so abstracts the experience of the focal subjects (a dehumanizing expression, to be sure), that real-life people are no longer recognizable, being reduced to nameless and faceless respondents or actors—a true irony among researchers who so strongly espouse "atomistic individualism" (Mills, 1993, p. 802).

Such formalized and rule-bound methods seemingly reflect a belief in "science by the numbers." Methodized science, however, belies the notion that our methods and measures must be, by definition, as "deeply human as the human stuff that is being studied" (Murphy, 1969, p. 530). My apprehensiveness, shared by Bevan (1991) with respect to the social sciences as a whole, is that "ultimately the integrity of our scholarship must depend on it being set in the real world of everyday experience; yet our present science-making strategies persist precisely in separating it from that domain (p. 475).

As a simple test of my apprehensiveness, pick an article from a recent issue of a scholarly management journal and compare it with the early work of William F. Whyte, Donald F. Roy, Melville Dalton, or Alvin W. Gouldner. The work of these scholars was systematic and rigorous, but not abstract and uninvolved. Their concern for people was not overshadowed by a concern for pet variables.

In my judgment, it is quite possible that the principal issues confronting our discipline will never be adequately addressed by blinkered scientific ("official") methodologies that, by abstracting and oversimplifying complex human affairs, deny the fundamental character of management as a human activity (Bevan, 1991, pp. 477-478). Rather, it is my belief that an investigative equilibrium balancing the detachment required for intellectual rigor and a concern for the essential humanity of people as individuals will be required to advance our common enterprise into a postmodern era of social scientific rationality (Lifton, 1987, p. 64).

CONCLUDING REMARKS

The preceding discussion addresses three broad concerns relating to the sociology of management as a science. It contends that the future integrity of the management discipline depends on its members being consciously aware and openly debating the discipline's perceived foibles. In this regard, worry is expressed over particularism within the management discipline. If the management discipline operated in a completely rational and universalistic manner, merit would be the sole criterion for judging role perfor-

mance. Rewards, as a form of recognition, would be governed by scientific considerations rather than personal or social attributes. As a scientific ideal, such a system would be a utopia. Admittedly, however, the management discipline is not a utopian enterprise. The proposition that the management discipline should be "open to talents" and that "recognition and esteem" should "accrue to those who have best fulfilled their roles, to those who have made genuinely original contributions to the common stock of knowledge" (Merton, 1942/1973a, p. 272; 1957/1973b, p. 293) is nevertheless a matter of ethical responsibility.

In a second section, concern over management journal manuscript review and decision-making practices is expressed. Based on the observations that (a) reviews of the same manuscript often have few critical points in common and (b) a spectrum exists from reviewer or editor to ghostwriter to coauthor, it is argued that the discipline's journals must strive to develop and enact practices that protect the integrity of the scientific enterprise while simultaneously respecting the prerogatives and ethics of authorship. In particular, authorship is a scholarly endeavor in which the true origins of thoughts and the words used to express them should be known. As forms of human expression, words are explanatory constructs that reflect ideologies. To tamper with these constructs or to color an author's logic and rhetoric with the overly invasive demands of editors and reviewers denies the author full intellectual responsibility for his or her work and permits subrosa influences to be exerted on both the current character and the future development of our discipline.

Finally, the trivialization of human experience as mirrored in management research is lamented. For 25 years in this profession I have watched as the management discipline has become bogged down in the pursuit of elusive variables and as the fundamentals of everyday human behavior have been increasingly eliminated from scientific analysis. The predictable result is that much of what passes as management research is arguably sterile, simple-minded, and, consequently, increasingly irrelevant to management practice (Queenan, 1989). Whether policymakers understand much of what we pass as scientific analysis may not be a concern to all within our discipline. The possibility, however, that they care not should issue a warning to all of those committed to advancing both management knowledge and skill.

In addressing each of the preceding concerns, we should also consider their relationship to one another (R. T. Mowday, personal communication, September 5, 1995). Taken together, they present a dilemma. It has been argued that as management scholars we should seek universalistic judgments based on scientific merit. At the same time, it has been observed that we find it very difficult to reach agreement on merit, given the preparadigmatic status of our discipline. If, however, the management discipline were to define universally accepted criteria for scientific merit, one suspects the result would be a set of rather simple and straightforward standards that would contribute, in the end, to more rather than less trivialization of the human experience. As with so many complex issues, addressing one challenge may make another worse. This dilemma, of course, underscores the inherent dependency of knowledge systems and the essential importance of dialogue in defining a discipline's intellectual character.

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