



## ESSAYS

# Publish or Perish Bane or Boon of Academic Life?

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*There are few more familiar aphorisms in the academic community than “publish or perish.” Venerated by many and dreaded by more, this phenomenon is the subject of the authors’ essay. Here they consider the publish or perish principle that has come to characterize life at many business schools. They explain when and why it began and suggest reasons for its persistence. This exercise elicits questions that appear as relatively neglected but are integral to our profession, namely, the effect of publish or perish on the creativity, intellectual lives, morale, and psychological and emotional states of faculty.*

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The year 1954, besides much else, saw the publication of Kingsley Amis’s academic novel *Lucky Jim*, to great acclaim. Five editions were published in just 3 weeks. Its leading character, James Dixon, came to epitomize the fate of faculty en route to tenure—that potent cocktail of ambition, restlessness, introspection, and insecurity inside an institutional framework where expectations with regard to tenure are anything but straightforward. In the following excerpt, Welch, his department chair, is speaking:

“If I were you, Dixon, I should take all the steps I possibly could to get this article accepted in the next month or so. I mean, I haven’t the specialized knowledge to judge. . . .” His voice quickened: “I can’t tell, can I? What it’s worth. It’s no use anybody coming to me and asking ‘What’s young Dixon’s stuff like?’ unless I can give them an expert opinion of what it’s worth, is it now? But an acceptance by a learned journal would . . . would. . . . You, well you don’t know what it’s worth yourself, how can you?” Dixon felt that, on the contrary, he had a good idea of what his article was worth from several points of view. From one of these, the thing’s worth could be expressed in one short

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hyphenated indecency; from another, it was worth the amount of frenzied fact-grubbing and fanatical boredom that had gone into it; from another, it was worthy of its aim, the removal of the "bad impression" he'd so far made in the College and in his Department. (Amis, 1954/1998, pp. 15-16)

During the past four decades, the publish or perish principle appears to have become the way of life in academia, including in many business schools (Darnill, 1996; Denning, 1997; Kaplan Educational Centers, 1998; "Teaching Spires," 1996). In fact, few aphorisms enjoy more familiarity within our academic community. "Publish or perish" signifies the principle according to which a faculty member's tenure is primarily a function of his or her success in publishing. It comprises a race against time that typically begins when the faculty member is hired and ends when the tenure decision must be made (usually after 6 years). Indeed, recruitment, promotion, and tenure appear to be decided primarily based on the number of articles published in a fairly select group of peer-reviewed journals, based on their relative impact, selectivity, and relevance to business school rankings.<sup>1</sup> As is regularly pointed out, this risks an emphasis on productivity at the expense of innovation, generating output that may be seen as relatively incremental, lacking in significance and substance, and too often restating the obvious (Bedeian, 1989; Byrne, 1990; Denning, 1997).

Junior faculty, in particular, are often forced to play a numbers game to earn tenure, where the criteria for tenure are likely to be some function of the number of articles published and the relative prestige of their outlets. More often than not, they may shy away from research that is less likely to see the light of day quickly, including interdisciplinary, ethnographic, or longitudinal approaches, or those with a distinct philosophical orientation. This trend, apparently also true of other disciplines, has impelled some to put pen to paper and express their disquiet in such well-liked novels as *Lucky Jim* and also David Lodge's *Changing Places* (1975/1978), *Small World* (1984/1985), and *Thinks* (2001/2002). The popularity of such works, particularly among academics, attests to a widespread awareness of this publish or perish ethos.

However, to what extent is the rhetoric of publish or perish a genuine representation of academic reality inside our business schools? Surely, our universities provide pressure to publish, the consequence of failing to do so adequately being the refusal of tenure. There may be some added pressure for business

schools that are part of universities to be seen as legitimate by their sister schools and departments in a scholarly sense. Yet publishing also grants faculty members mobility on an international scale and, by implication, bargaining power and promotion opportunities. Moreover, it is a means to reputation and influence. After all, publishing is our primary currency. Even if teaching and service are rewarded locally (and taken into account for tenure decisions), this multiplicity is not often found at national and international levels, where the principle determinant of recognition, prestige, and mobility is that of publication (Bedeian, 1996).

Successful publishing affords membership to increasingly privileged societies of scholars and, ultimately, serves an existential purpose in enabling us to leave our fingerprints on the intellectual history of our disciplines. As the late Harvard philosopher Robert Nozick (1981) wistfully recorded, "A significant life is, in some sense, permanent; it makes a permanent difference to the world—it leaves traces.... To be wiped out completely, traces and all, goes a long way toward destroying the meaning of one's life" (p. 582). The question as to why the more permanent should also be the more valuable is a philosophical one, and outside the scope of this essay. However, many of us would intuit that impact and longevity rank among the principal incentives for a profession such as ours.

Few academics have investigated the publish or perish phenomenon systematically or in depth, and relatively few have discussed its history and present state. The popular press, in fact, has emerged as one of relatively few critical voices of the publish or perish principle (e.g., Byrne, 1990; Sowell, 1995). However, surely, as participants in the academic enterprise we can be expected to take an interest in the questions it raises, such as, Why and when did pressure to publish begin in business schools, particularly (but not exclusively) in the United States? What are the consequences of this ethos for faculty in terms of their intellectual life, morale, and job-related stress? Is research conducted by business school faculty at risk of becoming increasingly irrelevant to practitioners? How frequently, if at all, do faculty at business schools with strong publish or perish traditions feel pressured to compromise their ideals to churn out manuscripts? Finally, in the applied context of a professional school, how is scholarship to be defined and evaluated?

In this essay, we discuss, and tender tentative responses to, these questions. We survey what has been said on them and outline a tentative agenda for sys-

tematic research to answer each. Rather than supplying a set of definitive statements about the nature and future of scholarship in business schools, a community to which we ourselves belong, we seek to provide a platform for reflection and dialogue. To paraphrase Socrates, the unexamined life, even in scholarship, seems hardly worth the effort.

### WHEN DID IT ALL START?

In the mid-1950s, the Ford Foundation (Gordon & Howell, 1959) and the Carnegie Corporation (Pierson & Others, 1959) each commissioned an inquiry into the status of collegiate business education in the United States. Both reports were published in 1959 and are still widely regarded as landmark studies in business education given their profound and enduring impact. Although the studies were carried out entirely independently, their authors were similarly scornful of business schools, lamenting their "creeping intellectual obsolescence" and absence of a stimulating intellectual atmosphere; teaching characterized by an emphasis on descriptive detail; and research based on feeble empirical evidence and insufficient analytical rigor and depth of penetration. They further noted the small volume of truly significant research, a general unfamiliarity with the latest analytical tools, and the conspicuous absence of high-quality journals in the organization sciences (Gordon & Howell, 1959, pp. 355, 379).

Equipped with a US\$35 million donation by the Ford Foundation and the Carnegie Corporation, several of the larger public and private university business schools began to stress the development of a stronger research climate. Research, rather than being a peripheral activity, became the decisive criterion of the academic enterprise and given equal or greater emphasis than teaching (Porter & McKibbin, 1988, p. 165). Larger business schools stepped up their recruitment from doctoral programs with a reputation for research rigor,<sup>2</sup> including industrial and social psychologists, applied mathematicians, and statisticians. The hypothetico-deductive method was to become a quintessential feature of so-called proper research. The profound effect of this endeavor is still felt today, with the use of hypotheses and focus on generalization (or the development of universal principles) as defining features of what is colloquially referred to as mainstream, or routine (Davis, 1971), research. Management, as a social science, came to perch the gap

between the narrative world of organizations and the natural sciences, using the context of organizations but methods of the natural sciences for reasons of transparency, legitimacy, and respectability. Uneasy with their vocational origins, many business schools grew into applied social science departments (Pfeffer & Fong, 2002, p. 79), importing a scientific appeal for precision, control, and testability (Bailey & Ford, 1996). A follow-up study by Porter and McKibben (1988), conducted for the American Assembly of Collegiate Schools of Business (AACSB), underscored the profound effects of the Ford and Carnegie studies: by the mid-1980s, research was of paramount importance; the nature of research itself had changed to exhibit rigor by relying on sophisticated statistical techniques; the number of scholarly journals had increased sharply, creating a top-tier among these journals; and tenure was decided mostly based on publications.

The impact of the Ford and Carnegie studies was felt in Europe also, albeit to a lesser degree. For example, in 1964 the United Kingdom pursued a government inquiry (similar to the Ford and Carnegie studies) through the Franks Commission (1966). A detailed report was published 2 years later and recommended the creation of two national business schools (in London and Manchester) to enhance the quality and provision of management education. France, too, responded by establishing a foundation to foster management education. The *Fondation Nationale pour l'Enseignement de la Gestion des Entreprises* (FNEGE) was set up in 1968 to provide research support and training for faculty. By 1973, it had provided in excess of 500 scholarships to fund French doctoral students at U.S. business schools. Recipients were expected to return to France, on completion of their Ph.D., to help bolster the level of management research domestically by leveraging their training.

### HOW ARE FACULTY AFFECTED BY THE PRESSURE TO PUBLISH?

A recurring criticism of our publishing principle is its emphasis on productivity, as a function of number and quality of publications. Any gains in efficiency are thought to come at the expense of innovation, boldness, heterogeneity, and, ultimately, so-called scientific progress (Bouchikhi & Kimberly, 2001; cf. Kerr, Tolliver, & Petree, 1977).

One of the most negative aspects of the pressure to publish is that it discourages any bold or original work on the part of junior faculty. When so much rests on not simply doing the research but, more important, getting it published, the risks of doing something unorthodox, something that might offend strongly held prejudices in a particular field, are great. (Smith, 1990, p. 191)

Bouchikhi and Kimberly (2001) insist that the central role business schools place on research and the intense pressure they put on faculty to publish in a select group of elite journals have increasingly led to the institutionalization of management research. As a result, they contend that management research has become less innovative and, therefore, is unable to effectively adapt to the world it attempts to understand. Pfeffer and Fong (2002), in a much discussed critique of business schools, suggest that our theorizing is at risk of becoming ever more irrelevant, not least because of the institutionalization of method. "To the extent that business school research increasingly resembles that of more paradigmatically developed social sciences, with the accompanying strictures, business school research is inadvertently disadvantaged" (Pfeffer & Fong, 2002, p. 89). They cited Weick as bemoaning the lack of relevance in business school research, as reward systems have tended to favor a narrowing of focus over eclecticism and versatility—an assessment that is particularly acute of journal publishing. As wryly put by Weick (1989), "Theorists often write trivial theories because their process of theory construction is hemmed in by methodological strictures that favor validation rather than usefulness" (p. 516). Astley (1985) similarly concluded that "the reputational work organization of administrative science...is committed to the production of intellectual innovations, and yet it must restrict the extent of such innovations to maintain its differentiated structure of reputations" (p. 508).

A lack of intellectual boldness and practical relevance (more on that later) may be due, in part, to the journal review process that, in turn, has obvious consequences for tenure. As Bedeian (1989) points out, our penchant for "theory thin and method driven" work risks fostering two significant shortcomings. First, a preoccupation with form may cause us to neglect substance. "In the last analysis, our discipline will be judged on the basis of the substantive enlightenment it has supplied, not on the basis of the methodological heat it has generated" (Bedeian, 1989, p. 3). This is consistent with Davis's (1971) explanation of

the nature of scholarly significance. As he, rather superbly, observes,

It has long been thought that a theorist is considered great because his theories are true, but this is false. A theorist is considered great, not because his theories are true, but because they are *interesting* [italics added]. Those who carefully and exhaustively verify trivial theories are soon forgotten, whereas those who cursorily and expediently verify interesting theories are long remembered. In fact, the truth of a theory has very little to do with its impact, for a theory can continue to be found interesting even though its truth is disputed—even refuted! (Davis, 1971, p. 309)

Miner's (1984) study supported Davis's claim, finding little correlation between the importance of a theory, as judged by 100 leading scholars, and its capacity to predict empirical findings. As Astley (1985) inferred "creativity, rather than information-gathering, drives scientific progress" (p. 510). Whatever happened to the scientific imagination?

Second, methodological rigor may be seen as able to compensate for theoretical weakness. Thus, one might apply sophisticated analytical techniques to manipulate data but without considering whether the enterprise is, in fact, worth the effort. An emphasis on methodological conventionality, cleverness, and rigor risks neglecting innovativeness and boldness as criteria for top-tier journal publishability. Bedeian's (1989) use of Campbell is instructive: "We seem to believe that truth will be discovered somehow through using more and more esoteric techniques of data manipulation rather than by looking for it in the real world" (p. 3).

While a partiality toward methodological rigor and quantity of output need not preclude innovation and boldness, a high throughput approach to publishing risks generating more trivial output. The pressure to publish, particularly for tenure purposes, may impel us to write articles that would be difficult for journal editors to reject (after all, the data sample and methodology are sound), rather than those that risk being returned (if only because the article, in the editor's viewpoint, does not meet a particular editorial policy). Yet, surely, innovation need not come at the expense of analytical rigor.

The exchange of reviewer comments and author revisions, typical of the peer review process, may also inhibit intellectual boldness and innovation. Because manuscripts generally are not accepted without revisions, authors must negotiate with journal editors and

reviewers about which revision requests to accept and how they should be made. As a result of this negotiation, "the published version of a manuscript is almost inevitably a compromise between what its authors intended to say and the mandates of an editor and a set of referees" (Bedeian, 2004, p. 199). Indeed, Meyers (cited in Bedeian, 2004, p. 208) found that authors' negotiations with reviewers and editors constrain acceptable content, leading to homogeneity of the scientific literature. Furthermore, Martinko, Campbell, and Douglas found that "editors' primary fields of interest were related to the content of the articles they selected for publication" (cited in Bedeian, 2004, p. 205). One would expect that these factors lead to the publication of research that conforms "to an orthodoxy defining what constitutes 'good' research" (Bedeian, 2004, p. 205) and thus to a decrease in research that is unconventional, intellectually bold, and innovative. Authors who regularly refuse to accept editors' and reviewers' revision suggestions are unlikely to publish enough articles in refereed journals to survive in academia.

Our journals exhibit a distinctly conservative bias in which reviewers and editors adopt . . . a "prosecution mentality" against authors, one that is typically overcome only through intense "negotiation and bargaining" . . . Though papers must be novel, they must not be offensively contrary or alienating to reviewers. (Astley, 1985, p. 507)

Aside from the effects of pressure to publish on innovation, intellectual boldness, choice of methodology, and practical relevance, academia remains relatively uninformed about its consequences for faculty morale and job-related stress given the lack of empirical research. Although critics of the imperative to publish often suggest that it causes faculty to experience high levels of job-related stress and a decline in morale (Smith, 1990), these consequences are mostly conjectural, and the evidence anecdotal. Publish or perish cultures in business schools may well have deleterious effects on the emotional state of faculty but may also produce effects that are rather more wholesome. For example, some faculty may experience greater psychological arousal that helps to increase the effectiveness of their work performance (Steers & Porter, 1991). For them, publishing is the fun part of the job. This could translate into a proliferation in the quantity and quality of research they produce for publication (Piganiol, 1971). Indeed, Taylor, Locke, Lee, and Gist (1984), having surveyed 278 faculty from a variety of

departments, including business administration, at a large, public university, found that those with high levels of Type A behavior perform better in response to publication pressures than Type B's with respect to quantity and quality of research published. On the other hand, those who lack institutional pressure to publish may become bored, apathetic, and, consequently, less productive (Moorhead & Griffin, 1995). A significant question, therefore, is how to achieve the proper balance between pressure to publish and productivity for individual faculty?

Future research, including surveys and in-depth case studies, should investigate the short- and long-term effects on faculty of the pressure to publish. Such research might help propel the debate on the trade-offs of our publishing culture outside of the realm of conjecture. Indeed, we believe that such research is essential to understanding and enhancing the quality of work life for business school faculty. Whether unorthodox research, or that based on less conventional methodological approaches, is less likely to see the light of day in our leading journals is an empirical question that should also be investigated.

## ARE WE CONTRIBUTING TO OUR IRRELEVANCE?

Questions about the effect of publish or perish on the intellectual life and morale of faculty are inextricably wound up with those of influence and relevance. How far are we removed from what Hambrick called the "incestuous, closed loop" of writing almost exclusively for each other (as cited in Pfeffer & Fong, 2002, p. 89) or what Coser (1975) referred to as "sect-like, esoteric ruminations" (p. 691)? If we fail to have an impact on the practitioner community, should this be a legitimate concern for management researchers?

It is perhaps ironic that the very success of the Ford and Carnegie studies may have bred a new criticism, namely, that much of the research conducted by business school faculty is insufficiently relevant to the concerns of practitioners. At the same time, few would want to return to the pre-1960s era of comparatively unscientific and anecdotal research, much of which was contributed by practitioners. Yet, our elite scholarly journals have, on the whole, become a means for communicating with those within our discipline, leaving our research at risk of failing any reasonable test of applicability or relevance to consequential management problems or policy issues (Behrman &

Levin, 1984; Queenan, 1989). Practicing managers spend around US\$1 billion each year on business books, yet remarkably few of these are written by university professors (Ford et al., 2003, p. 46). As passionately put by Sykes (1988), in tens of thousands of journal articles, the system of academic publishing has been perverted into a scheme that serves only to advance academic careers and bloat libraries with masses of unread, unreadable, and worthless articles. Yet, arguably, the very structure of our system is to blame for this: Publications intended for, and peer reviewed by, fellow academics count, while those intended primarily for practitioners do not (Porter & McKibbin, 1988).

The reason for publishing articles that count is clearly that of gaining academic respectability and tenure, these being seen to be part and parcel of today's research culture. Such criticisms, however, do force us to think more deeply about the purpose of business education and about the stakeholders of management knowledge. Reflecting on this in his presidential address to the annual meeting of the Academy of Management, Arthur Bedeian (1989) voiced his concern that by publishing research that is not relevant to practitioners "we may be 'talking to ourselves' [and] . . . may be constructing a closed system with no input or output [that] . . . lose[s] energy, decay[s], and, ultimately, end[s] in chaos" (p. 4). Or as playfully put by Socrates, "Academic persons, when they carry on study...become decidedly queer, not to say rotten; and...those who may be considered the best of them are made useless to the world by [this] very study" (Cornford, 1922/2003, p. 93).

At the same time, one wonders whether business schools are not also expected to generate some fundamental, "blue sky" research, the application of which is not immediately clear? After all, it is in our interest to ground our convictions in sophisticated and defensible theoretical structures so as to answer our critics. Good theory must exist if only to counter bad theory. So, maybe the issue is one of balance, where business schools need not just produce output that is of immediate practical relevance, but some that is.

#### **BY WAY OF CONCLUSION**

Although a fresh assessment of the status of business education is probably overdue, this is not our aim. Nor do we discount the importance of research.

We agree with Porter (2000) that academia has a unique responsibility to reflect on, and systematically study, business practices and policies. What concerns us is the publish or perish principle that grew out of the Ford Foundation, Carnegie Corporation, AACSB, and Franks Commission studies. Surprisingly few serious works (outside of the popular press) have investigated this phenomenon empirically, despite its familiarity to those "on the inside." We do not dispute researching and writing as a vital component of academic life. We fully endorse it. Nor are we concerned with the anxieties that come with a career involving research *per se*. Anxiety might be rather wholesome in fueling our efforts to write, never quite sure whether our articles will be sufficiently competitive to fend off rival articles en route to elite journals. We do worry about strangleholds on what it takes to gain promotion, tenure, and mobility, as a consequence of institutional demands and, more subtly, the persistence of mainstream criteria to defining so-called good research. We also worry about barriers to innovation in publishing because of deeply engrained commitments to so-called normal science research. Goshal's (2005) suggestion for a broader range of scholarship is instructive:

The social sciences, in general, and business schools, in particular, have lost their taste for pluralism—as would be manifest to any reader of this article who has participated in a tenure committee meeting. The challenge for the deans is to reinstate this taste [for intellectual pluralism]—not only in terms of theory and methodology but also in terms of what research questions are asked and where the answers are published. (p. 88)

Our concerns about the impact of publish or perish suggest a different approach to tenure and promotion. For instance, faculty who are applying for tenure and promotion could be asked to limit their submission to what they consider to be their three to five most important publications. In other words, there would be strict limits on what faculty can submit for tenure purposes. These articles (or even books) would be read and evaluated by the relevant committee members, in terms of perceived originality, quality, and actual or potential impact. Even if the impact of a journal, defined by number of citations, may be the most objective measure available to us, it need not, therefore, be the best measure of the competence of individual researchers. Hence, it is important that these arti-

cles be read and evaluated on their own merits. This assessment would be augmented with obtaining the usual references from a sample of peers. This approach contrasts sharply with those popular at many institutions where faculty are evaluated on all of their publications (and where quantity might compensate for lack of quality or originality, even if their articles are published in top-tier journals). Such a strategy requires faculty to focus on quality, contribution potential, and novelty, given that they only really need to generate a small number of high-quality publications within a tenure track period. This might encourage us to ask big, important questions, freed from the compulsion to confine ourselves to technically savvy, low-risk, manageable research projects (Pfeffer, 2005, p. 99). Incidentally, this might also allow work in progress more time to mature and "do the rounds" at various workshops and conferences.

Alternatively, faculty might be afforded more freedom of choice as to when to go up for tenure. The emphasis would shift from time (typically 6 years) to number and quality of output (articles, books, popular press contributions, policy reports), citation rates, innovativeness and impact on the discipline as judged by peers, impact on practitioners as judged by publications in professional journals (for which a separate ranking could be produced), or contributions to high-level policy making. Hence, the criterion for arriving at a tenure decision would shift from time to a negotiated number of publications, and faculty would elect to go up for tenure when they feel their record is sufficiently meritorious. This would allow their research programs a longer time to "bear fruit" and would remove the pressure of a fixed tenure clock. Some faculty may even be given the choice to never to go up for tenure and could be retained on a 1-, 2-, or 3-year revolving contract.

In sum, during the past four decades, business schools have changed significantly. While most faculty and administrators would probably agree that the changes have been primarily positive (e.g. a more stimulating intellectual environment, a stronger curriculum), some have legitimately questioned the merits of the publish or perish principle that has emerged. This essay has sought to provide (a) a review of the literature on the publish or perish principle in business schools, its causes, reasons for its persistence, and the trade-offs generated by it; (b) a call for systematic empirical research into its impact on the creativity, intellectual lives, morale, and psychological and emo-

tional states of faculty; and (c) an invitation for empirical research on the so-called publishing behavior of elite journals in the organization sciences. The latter may help assess the degree to which we are genuinely at liberty to write and publish as we wish, free from any epistemological and methodological hegemony. Clearly, in the absence of good data we too easily resort to our necessarily fallible and subjective experiences.

In *Lucky Jim* meanwhile, Dixon is about to deliver his opus—a high-profile public lecture to an audience of peers, students, administrators and, most fatefully, Welch. Jim's future within the department hangs in the balance. In what was to be the salvaging of Dixon's promising scholarly career, he blows it all in a miscalculated effort to please. What is one of the last and most celebrated episodes in the novel is worth quoting:

Dixon stood up. His knees began shaking violently, as if in caricature of stage-fright . . . With some difficulty, he took up his stand at the lectern, ran his eyes over his first sentence, and raised his head. . . . In his own general unease, Dixon, who could never understand afterwards how he came to do it, produced an excellent imitation of Welch's preludial blaring sound. The uproar, passing the point where it could still be regarded as legitimate applause, grew louder . . . Dixon's blood rushed in his ears. . . . When he'd spoken about half a dozen sentences, Dixon realized that something was still very wrong. The murmuring in the gallery had grown a little louder. Then he realized what it was that was so wrong: he'd gone on using Welch's manner of address. In an effort to make his script sound spontaneous, he'd inserted an "of course" here, a "you see" there, an "as you might call it" somewhere else; nothing so firmly recalled Welch as that sort of thing. Further, in a partly subconscious attempt to make the stuff sound right, i.e. acceptable to Welch, he'd brought in a number of favourite Welch tags: "integration of the social consciousness," "identification of work with craft," and so on. . . . The mounting murmur from the gallery indicated that these efforts were not passing unappreciated. Sweating and flushing, he struggled on a little further, hearing Welch's intonation clinging tightly round his voice, powerless for the moment to strip it away. . . . As he talked, he began glancing round the front rows . . . he looked further afield, trying to pick out Bill Atkinson. Yes, there he was. . . . Over the whisky-bottle an hour and a half earlier, Atkinson had insisted, not only on coming to the lecture, but on announcing his intention of pretending to faint should Dixon, finding things getting out of hand in any way, scratch both his ears simultaneously. "It'll be a good

faint," Atkinson had said. . . . Of the staff contingent, the senior members looked up with similar expressions, the junior wouldn't look up at all. Shouts, whistles, and applause came from the gallery. Dixon raised his hand for silence, but the noise continued. It was too much; he felt faint again, and put his hands over his ears. . . . Half-way down the hall Bill Atkinson, unable at that distance, or unwilling, to distinguish between the scratching and the covering of ears, collapsed full length in the aisle. . . . I've done it now, he managed to think. . . . He drew air into his lungs; if he could push out again he'd be all right, but he couldn't, and everything faded out in a great roar of wordless voices. (Amis, 1954/1998, pp. 222-227)

Needless to say, Welch was unable to recommend Dixon's retention on the faculty.

## NOTES

1. Journal lists may be significantly influenced by the criteria applied in ranking business schools. For example, the *Financial Times's* ranking of the world's 100 leading business schools includes a measure for research productivity based on a selection of 40 journals. Given the impact of rankings on MBA applications, publishing in these journals may be particularly emphasized.

2. A recent study by Long and Bowers (1998) examined the relationship between affiliation and research productivity. It concluded that the status of academic affiliation (or the institution where one completes one's graduate training) has a relatively strong association with research productivity in terms of the number of publications in top journals and citations counts of these publications.

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