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RESEARCH NOTE

Counting footnotes: Citability in management studies

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Summary The primary purpose of academic citation, at least in Management Studies, is citation analysis. So much hangs on citation analysis as an indicator of academic performance – careers, funding, institutional survival – that papers are written as platforms for citation rather than to be read. To satisfy the requirements of referees, editors, and publishers, a paper must be, above all else, citable. This paper investigates the citation practices of some of the top authors of some of the top papers in some of the top journals of Management Studies. It finds citation by an elite of an elite for an elite. This is generally seen as evidence of the disciplinary strength of Management Studies. We interpret the evidence differently; we see convergence on papers that are citable. We consider what makes a paper citable. Most important of all is that the paper is cited by others.

Introduction

> "It was in 1968 or 1969, when I was a junior faculty member in the Department of Mathematics of the University of California at Berkeley. A mathematician from an Eastern European country had just given a 'colloquium talk' and, during the party that followed the lecture, I heard him explicitly beg his colleagues to cite his work in their papers. He claimed that, in his country, the number of citations was used to determine the salary of scientists, and asked his Western colleagues the personal favour of citing his work." (Figa-Talamanca, 2007, p. 83)

Papers published in the top journals of Management Studies have a value beyond their intellectual content. The journal in which a paper is published has become an indicator of merit, contributing mightily to the assessment of the academic quality of individuals, university departments and universities themselves. It also exerts a major influence on the process by which academic funding is allocated. But there is always pressure to render indicators more robust and reliable, especially when so much depends on their use. Enter citation analysis; the frequency with which a paper is cited is thought to relate more closely to the paper's merit than simple acknowledgement of the standing of the journal in which a paper is published. Indeed, so much faith is placed in this more sophisticated indicator that the quality of journals themselves has come to be judged by the frequency with which the papers they publish are cited. Thus, academic authors (and all those who employ academics) and editors (and all those involved in publication) share an interest in citation. So, too, does the Institute for Scientific Information (ISI). For over half a century, a single firm has collected all the citation data and provided all the citation analysis on which assessments of academic merit are made. ISI insists that the citation data it analyses are objective, of some importance when so much

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depends on citation analysis, and when citation analysis depends on ISI's citation data.

Let us wield Occam's Razor: citation analysis depends on nothing more sophisticated than footnote counting. Elsewhere, we have been equally radical in our exploration of gaming to publish in the top journals of Management Studies (Macdonald & Kam, 2007a, 2007b, 2008). A game based on counting footnotes is much simpler. Anyone can play, and most in the academic world do. We look at how this particular game is played, turning citation analysis on its head so that it reveals not academic performance, but gaming itself. We conclude that citation analysis has become a primary purpose of citation, and that this distortion undermines any other purpose. Citation is, more than ever, a social process serving the requirements of very vested interests. Profuse citation of a paper is no longer just an indicator of a paper's value: its citability is the paper's value. A citable paper is precious property, which can be made more valuable still by increasing its citability. We explain how this is done, then consider the extent to which it is done, then some of the consequences of doing it.

The Research Assessment Exercise (RAE) was launched 25 years ago in the UK, and has provided a model for measuring and rewarding academic performance (Barker, 2007; Elton, 2000). The RAE depended heavily on assessment of journal quality, in which citation analysis played a part. The successor to the RAE, the Research Excellence Framework, will make direct use of citation analysis to indicate quality. From modest beginnings (see Gross & Gross, 1927), citation analysis has become big business. We look at the way this business is conducted. We conclude by speculating specifically on the consequences of citation manipulation for the intellectual development of Management Studies (Ilgen, 2007). What Wade (1997) refers to as 'footnote counting' has fulfilled Kaplan's prophecy of 1965 (p. 183): "Whatever happens, it seems quite certain that we are less likely to look down upon the lowly footnote in the years ahead." Although we take our evidence whence we find it, our focus is on Management Studies.

Gaming

By gaming, we mean doing whatever is necessary to improve the chances of an outcome, in this case the outcome being a paper in one of the top journals of Management Studies. Authors game; rare is the author who simply conducts research, writes a paper, and then submits the result to a top journal. Authors try to improve their chances of publication by targeting a journal and then matching their research and their writing to papers published in that journal. They strive to demonstrate how comfortably their own paper fits with what else the journal publishes. Citation is crucial in this exposition. Citing papers from the targeted journal is an obvious tactic. More important is citability, ensuring that many others will cite the paper (Paul, 2008). Lengthy literature reviews and vast methodology sections are advocated (Judge, Cable, Colbert, & Rynes, 2007; Leff, 2005; Rossner, Van Epps, & Hill, 2007). Meta-analysis is always popular (Judge et al., 2007): empiricism is to be avoided. Cutting edge research is never going to be highly cited (Begley, 2006; Ronco, 2006). Nor is anything critical, which is why top journals favour positive, constructive papers (see Smart, 1964; Westrin, 1987). Prominent co-authors attract citation, but any co-authors at all are helpful in that they are all likely to cite their own paper. Self-citation counts like any other.

Editors also game. For the editor, journal impact factor, devised by Eugene Garfield in 1955 (Chew, Villanueva, & Van Der Weyden, 2007), but undeveloped for more than a decade (Garfield, 1996a), is now everything (Borokhovich, Bricker, & Simkins, 1999). Each year, Garfield's company, ISI, which was acquired by Thomson and now part of Thomson Reuters, uses the data it collects to calculate the average number of citations a top journal's papers receive in the two years following publication (see Cross, 2009). A high impact factor attracts citable papers, which raises the impact factor, which attracts yet more citable papers, which raises the impact factor, which attracts yet more citable papers . . . Over half of the 100 most cited papers in Economics and Business are published in just 6 journals (Ioannidis, 2006).

Editors must work to achieve a high impact factor (Judge et al., 2007; Monastersky, 2005). Mistakes in ISI's calculations — apparently not at all rare (Monastersky, 2005; Rossner et al., 2007) — can be rectified by editorial negotiation (Chew et al., 2007).

> "Every year, we have a formal conversation with ISI before their data are published . . . [When] the journal was redesigned . . . we had a chat with ISI to ensure they understood what's eligible for counts; we double-check ISI figures by estimating citable items ourselves then checking with ISI—there's not much variance now . . . We take on trust that the numerator is correct. We now know that [other] publishers do this with ISI—we'd been slightly naïve before." (editor of medical journal as quoted in Chew et al., 2007, p. 146)

Editors massage their impact factors by publishing long reviews (which are convenient to cite) and special issues focused on key papers (which really have to be cited) (Ronco, 2006), and by asking certain authors to make their papers as long as possible (to diminish the denominator in the impact factor calculation) (Ashkanasy, 2007). They can be quite shameless in publishing papers praising their journals (e.g., Daft & Lewin, 2008; Mangematin & Baden-Fuller, 2008). Take one paper published in the Journal of International Business Studies:

> "An examination of the most frequently cited articles published in the leading international business journals provides interesting insights. For example, of the 15 most widely cited articles (each with at least 49 citations), all were published in the Journal of International Business Studies, thus denoting the dominance of JIBS in driving the international business research agenda." (Griffith, Cavusgil and Xu, 2008, p. 1229)

It is no longer unusual for editors to insist that authors include citations to papers in their own journals (which inflates the numerator) (Smith, 2006).

> "I should like you to look at some recent issues of the Journal of Applied Ecology and add citations to any relevant papers you might find. This helps our authors by drawing attention to their work, and also adds internal integrity to the Journal's themes." (editor of Journal of Applied Ecology to author as quoted in Monastersky, 2005)

> "Manuscripts that have been published in Leukemia are too frequently ignored in the reference list of newly submitted manuscripts, even though they may be extremely relevant ... We have noticed that you cite Leukemia [once in 42 references]. Consequently, we kindly ask you to add references of articles published in Leukemia to your present article." (editor of Leukemia to author as quoted in Smith, 1997)

We have come a long way since Derek de Solla Price (1964) declared that editors have an ethical responsibility to ensure that bibliographies are neither insufficient nor padded. For the gaming editor, the next logical step is curtailing citation to competitor journals (Begley, 2006; Ronco, 2006).

Authors need little persuasion to be compliant: in rendering their papers as citable as possible, they act not only in the journal's interest, but also in their own. As the editor of the International Journal of Artificial Organs [sic] concedes: "It is in the best interests of academic researchers or editors to slightly manipulate citation indices." (Ronco, 2006). The public interest is not a concern.

> "We ... [used] ... to make our acceptance criterion those articles that we felt would make a contribution to the international literature. Now our basis for rejection is often 'I don't think this paper is going to be cited'." (editor of medical journal as quoted in Chew et al., 2007, p. 146)

Because top authors are cited more than other authors, editors are anxious to publish their papers. Mischievous empiricism has demonstrated that editors are much more likely to accept a paper under the name of a top author than exactly the same paper bearing an unknown name (Armstrong, 1984; Ceci & Peters, 1982). Actually, editors tend to run a dual submission system: one track for authors of papers that will not be published but whose rejection will increase the journal's rejection rate — a lesser indication of journal status (Lockett & McWilliams, 2005) — and a fast track for stars whose papers will be cited, thereby raising the journal's impact factor (Chew et al., 2007).

> "We introduced fast-track publication ... for high impact papers ... believe it's the most important thing [our journal] has done in my time as editor. It's transformed our relationship with authors." (editor of medical journal as quoted in Chew et al., 2007, p. 146)

Publishers also game (Cameron, 2005). Publishing is a business, no more and no less concerned with public benefit than any other business (Hurt & Schuchman, 1966). Academic publishers promote journals by advertising high impact factors, thereby attracting the attention that will lead to even higher impact factors. They encourage journal editors to raise impact factors by courting key authors (Laband & Piette, 1994), not least because of their propensity to cite themselves (Cookson & Cross, 2006). And they massage the data that goes into impact factor calculations; for example, by offering free access to selected papers, which increases citation of these papers and so boosts impact factors.

> "We are delighted to offer free access to the articles which made the greatest contribution to the 2007 IF for each of our most highly ranked Business and Management journals." (email from Wiley-Blackwell to author, 31 July 2008)

And universities game. They do this by hiring those academics most likely to publish in top journals, and by encouraging staff to cite each other. Many departments pay a bounty for every paper published in a top journal. University departments have become veritable factories in which the efficient production of papers is tightly controlled (Fearn, 2009). Warwick University (2009), for example, seeks to increase citations to Warwick publications by encouraging staff to write reviews, always to work on the same topic, to repeat phrases and terms (for the search engines), to take on editorships (authors cite editors), and to self-cite wherever possible. The campaign is co-ordinated by the University Library. Once, a long time ago, university libraries relied on citation analysis to guide their purchasing: now they work to distort citation analysis for the advantage of the university. Universities game when they host journals in that staff authors are more likely than others to be published in these journals (Morrison & Inkpen, 1991). Harvard Business Review boasts a 99% rejection rate, but this does not apply to authors associated with Harvard, who contribute about a third of all papers published in the journal. Not surprising, then, that about 40% of all citation involving Harvard University in the 30 top journals of Management Studies studied by Podsakoff, MacKenzie, Podsakoff, & Backrach (2008) is to papers in the Harvard Business Review.

Governments game too. Education is big business. To be competitive, a nation's universities must rank highly in the international lists. Citation is critical. Huge advantage lies with those countries in which top journals are based, the United States in particular (Ha, Tan, & Soo, 2006), and the Anglo-Saxon world more generally (Merilainen, Tienari, Thomas, & Davies, 2008). But governments do not shrink from creating advantage. In Australia, the reluctance of Philosophy academics to supply ranking lists of top journals resulted in government threats to construct and impose its own (Corbyn, 2008). In France, the problems academics experience publishing in English language journals (Venard, 2007) have been overcome by the government's elevation of French language journals on the lists. When citation analysis guides the allocation of resources, the allocation will be regressive. As Merton (1968) predicted in applying what he called the 'Matthew effect' to academic publishing, the rich get richer, whether they be individual academics, university departments or whole universities. The poor get poorer. Governments know this full well. The vast returns from the manipulation of citation provide not only incentive to play the game, but also — as we shall show in this paper — the means to win.

Citation analysis

Citation is fundamental to academic publication. Citation provides an intellectual audit trail, allowing authors to show whence their thoughts have come, and how these thoughts relate to the thoughts of others. Inasmuch as the academic paper is an addition to knowledge, its citation shows to what knowledge the addition is made.

> "Because citations are an acknowledgment that academic work is recognized and is contributing to subsequent

> scholarly research, citations serve as a proxy for advancement of the state of knowledge." (Lockett & McWilliams, 2005, p. 140)

Citation analysis has no such lofty purpose. Footnote counting can be no more reliable than the data on which it is based, and citation has never been objective (Kaplan, 1965). Citation has always been a social process, intended to persuade more than inform (Brooks, 1985). Citation analysis has made the social process more pervasive than ever: authors cite to influence citation analysis. We have already noted how authors and editors exploit citation in their gaming. To publish in the top journals, it is important to cite the right papers by the right authors in the right journals (Jones, Brinn, & Pendlebury, 1996). Put another way, citation behaviour demonstrates whether an author acknowledges the preeminence of the elite who dominate these journals. The citation that Mitra (1970, p. 118) once described as the entrance fee to the invisible college, has become the citation that reveals whether an author — as Margaret Thatcher might have put it — is one of us.

Citation is no more than an academic convention, dating from perhaps the mid-nineteenth century (Mitra, 1970). Citation analysis is yet more recent. Before ISI and computers, bibliographic analysis hardly extended beyond totting up the publications of individual authors (e.g., Brodman, 1944). Only the most general of associations was observed between the prominence of scientists and the number of papers they published. There was no evident reason why the prolific author should be brilliant as well. Gregor Mendel published only 7 papers in his lifetime: John Edward Gray, an English naturalist and nonentity, published 883 (Dennis, 1954). By the early 1960s, Garfield was selling the data needed for citation analysis, collected from editors by ISI (Harnad, 2007). For Garfield, quantity of citation indicated quality; he could never accept that quantity might be incompatible with quality:

> "Lord Kelvin is reputed to have published about 600 scientific papers in his working lifetime — almost one excellent paper every five weeks for a period of 67 years." (Garfield, 1978, p. 5)

Garfield's energetic promotion of citation analysis, and his strategic focus on top journals, influenced attitudes. Garfield was always, and still is, single-minded in his approach to citation analysis, publishing profusely in support of his system (Hopkins, 2005; Klein & Chiang, 2004). By the 'sixties, Price (1965)Price (1965, p. 510) was able to use "machine-handled citation studies" to calculate that the average paper was cited about once a year and about 10 times ever. The skew, however, was huge; the most cited academic paper of all (Lowry, Rosebrough, Farr, & Randall, 1951) was cited 29,655 times between 1961 and 1972, the second most cited paper a mere 6,281 times (Garfield, 1974). Lowry explained that his paper was so heavily cited not because it was brilliant, as it would be considered now, but for almost the opposite reason — it was routine:

> "It is flattering to be 'most cited author', but I am afraid it does not signify great scientific accomplishment. ... Although method development is usually a pretty pedestrian affair, others doing more creative work have to use methods and feel constrained to give credit for same." (Lowry, 1969)

By 2005, Lowry's paper had been cited something like 300,000 times (Garfield, 2006).

It used to be acknowledged that outstanding papers were rarely cited; everyone knew of them, so there was no point (Moravcsik, 1973). The most cited papers were certainly not considered the best papers (Lundberg, 2003). Copious citation was just as likely to highlight the worst papers, cited for their inadequacies (Rossner et al., 2007). The problem still exists, though it is less acknowledged. Woo Suk Hwang's two fraudulent and withdrawn papers on stem cells were cited over 400 times within three years of publication (Rossner et al., 2007). Garfield himself, stung by criticism of ISI, notes the syndrome in his reference to a spat in the pages of Science between David Hamilton and David Pendlebury, who worked for ISI.

> "Pendlebury published a note in Science which attempted to correct the false impression created, but like so many unpublished errors, Hamilton's report continues to be cited while Pendlebury's 'correction' is mainly overlooked." (Garfield, 1996a)

It was becoming apparent that papers were often cited because they had been cited. The more a paper was cited, the more it would be cited — and over a longer period than a less-cited paper (Price, 1976). For Price, exploring communication patterns in science, this posed problems:

> "... it would appear that the course of future citation successes is determined statistically by the past history of the cited paper; and so one is driven to suppose that citations are generated by a pull mechanism from previous citation rather than from a push mechanism of the papers that do the citing." (Price, 1976, pp. 304–5)

It was becoming *de rigeur* to cite certain papers to demonstrate knowledge of which papers were *de rigeur*, not because the papers were especially good or even relevant. In High Energy Physics, Moravcsik and Murugesan (1975) found that about 30% of citation was redundant (in the scientific sense of not being strictly necessary) and a further 40% perfunctory (in the sense of adding nothing). Nevertheless, the more these papers were cited, the more they tended to be cited. When Garfield (1974) came to update his list of the 50 most cited papers published between 1961 and 1967, only 10 had been displaced in the decade. By the early 'nineties, others had come to suspect that citation analysis was as likely to reveal what was fashionable as what was valuable (Perlman, 1991). Some considered the tendency to cite what others cited reduced the value of citation analysis (West, 1996). Not Garfield (1996b); he clung fast to the notion that the good was attracting the good, and that this made citation analysis the more useful in the identification of quality.

Over four decades, citation analysis has changed from servant to master (see Moravcsik, 1973). It now dominates understanding of academic publication. We dispute the usefulness of this approach: citation analysis reveals little beyond what those with vested interests in academic publication have engineered. Driving their efforts is Garfield's notion that citation analysis identifies the best. Yet, Swanson

(1986) finds that papers from different disciplines cite different literatures, even though they are looking at the same issue. Garfield (1989) admits that the most innovative work will cite papers well beyond the disciplinary boundary, but this is not his concern. As Merton came to realize, and perhaps Price did not, citation analysis was never intended to trace the flow of ideas. Citation analysis was to demonstrate the strength of discipline.

> ''The theory behind information retrieval by citation indexing is based on this key assumption — papers are linked together by what they cite. ... No one I know has the time to analyze all the serendipitous connections that could be made through the SCI [Science Citation Index], much less through semantic or other techniques. ... I have my hands full dealing with the riches uncovered by bibliographic coupling.'' (Garfield, 1989, pp. 126—7)

In Management Studies, most journals specialise in area of research and school of thought. The top journals do not; they are overwhelmingly generalist. In theory, they welcome a wide range of submissions, and might be expected to accommodate a wide range of citation: in practice, they tend to publish papers that resemble in content, approach and presentation the sort of papers they always publish. It is surely hard, though certainly not impossible, to express novel ideas without resort to novel citation (see Cronin & Meho, 2008; Lockett & McWilliams, 2005). When papers in top journals rely on a common pool of references, they may well be establishing orthodoxy in a discipline, but they can hardly be at the cutting edge of its thinking. But do the top authors of top papers in the top journals of Management Science practice this behaviour? Is their publishing driven by citation requirements? Here we do not use citation analysis to discover merit. Instead, we turn the usual approach on its head. We accept the merit as established and examine the citation analysis which allows the merit to be declared.

Methodology

While there is considerable dispute about the exact ranking of top journals, there is much less about which journals are in the top group. Garfield's 1994 list of the 50 top journals in science differs from his 1989 list in only 2 titles (Garfield, 1996c). Once a top journal, always a top journal (Garfield, 1998). For this study, we have selected the group of Management Studies journals awarded the highest rating of the UK's Association of Business Schools in January 2007. As Table 1 shows, they are in the areas of General Management, Organisational Studies, Marketing and Human Resource Management, with a single journal in Strategy. A total of 23 journals met the initial selection criteria. After some deliberation, three journals (Harvard Business Review, Human Resource Management and Personnel Psychology) were excluded from our sample because they contain a large proportion of the unconventional material that does not meet ISI citation criteria. We were dependent, as are others who attempt the analysis of citation, on the data provided by ISI, and excluded such publications as editorials, proceedings, book reviews, letters, notes, and corrections.

The authors on whom we focus are those who have published most in these 20 journals over the period 1987—2006. For convenience, and because that is how they are seen in the Management Studies community, we refer to these authors, their papers and the journals as 'top'. So, somewhat unimaginatively, we have 20 top authors publishing in each of 20 top journals over a period of 20 years. The 20-year timeframe is treated as one aggregated period, such a length of

Table 1 Internal citation (20 journals, 1987—2006).

		%
General management	Academy of Management Journal (AMJ)	51.9
	Academy of Management Review (AMR)	47.9
	Administrative Science Quarterly (ASQ)	50.7
	Journal of Business (JOB)	14.6
	Journal of Management (JOM)	14.4
	Journal of Management Studies(JMS)	10.6
Marketing	Journal of Consumer Research (JOCR)	34.8
	Journal of Marketing (JOMkt)	35.1
	Journal of Marketing Research (JOMR)	60.8
	Journal of Retailing (JOR)	16.7
	Journal of the Academy of Marketing Science (JAMS)	11.9
	Marketing Science (MS)	35.2
Organisation studies	Human Relations (HR)	11.4
	Leadership Quarterly (LQ)	10.3
	Organisation Science (OS)	16.5
	Organisation Studies (OSt)	9.1
Human resource management	British Journal of Industrial Relations (BJIR)	12.5
	Industrial Relations (IR)	10.4
	Personnel Psychology (PP)	15.1
	Work, Employment and Society (WEAS)	9.6
Strategy	Strategic Management Journal (SMJ)	33.4

time being considered necessary to cover gradual changes in an author's publishing behaviour. In all, then, the sample covers 366 individuals who were authors or co-authors of 2460 papers and responsible for a total of 115,056 citations in those papers. Identifiable errors in the downloaded data have been adjusted to cope with inconsistencies in authors' names and papers' titles. Recall that our sample is restricted to only 20 top authors in each journal. When there were multiple authors with the same publication count, we resorted to simple alphabetical order to bring the total to 20. Our interest is mainly in the skewed few, the crème de la crème of Management Studies. For this reason, our most detailed analysis uses a subgroup of top 20 authors from all 11 journals, taken from the larger group of 20 of each journals, a total of 194 authors. These 11 journals focus most closely on the subjects core to Management Studies. Analysis of the citation behaviour of this subgroup exposes to a more extreme test our hypothesis that the few dominate publication in top journals, and are able to maintain and strengthen their dominance. Our investigation looks inward, to who is citing whom within the group, and has no cognizance of citation in the world beyond.

Table 2 Reference lists (20 journals, 1987—2006).

Author	Journal	Papers authored	Total citations	Average citations per paper
Alvesson M	HR	5	258	52
	JMS	4	217	54
	OSt	7	396	57
Baum JAC	AMJ	6	450	75
	ASQ	5	326	65
Boeker W	AMJ	6	331	55
	ASQ	4	219	55
	OS	3	193	64
Carroll GR	ASQ	9	419	47
	OS	3	163	54
Child J	JMS	5	301	60
	OS	3	179	60
	OSt	5	194	39
Clegg SR	OSt	11	589	54
Devries MFR	HR	11	650	59
Furnham A	HR	14	483	35
Hambrick DC	ASQ	4	275	69
	SMJ	9	480	53
Hill CWL	AMR	5	260	52
	JMS	4	176	44
	OS	5	284	57
	SMJ	8	312	39
Hitt MA	AMJ	6	455	76
	JOM	6	413	69
	SMJ	13	805	62
Hoskisson RE	AMJ	8	638	80
	AMR	4	234	59
Knights D	JMS	7	406	58
	OSt	6	361	60
Miller KD	SMJ	9	464	52
Mitchell W	SMJ	12	736	61
Mumford MD	LQ	13	827	64
Reuer JJ	SMJ	9	539	60
Singh H	SMJ	12	634	53
Sutton RI	AMJ	6	268	45
	AMR	4	234	59
	OS	4	239	60
Zajac EJ	ASQ	7	529	76
	SMJ	11	539	49

Citation practice

An immediate discovery is that a paper published in a top journal often cites other papers from that same journal (Table 1), a phenomenon we term 'internal citation'. The papers of top authors in the Journal of Marketing Research, with 60% internal citation, have very little interest in anything that is not published in the Journal of Marketing Research. Papers in the Academy of Management's Journal and Review, and Administrative Science Quarterly are as inward-looking as the house magazine of some large corporation. In all these cases, internal citation of top authors is well over the 20% threshold that is supposed to make ISI spring into investigation of possible manipulation of citation data. Top journals may rest easy: the World Journal of Gastroenterology reached 85% internal citation before being dropped by ISI (Begley, 2006).

Analysis of over 2000 papers published in the top journals of Management Studies in 2003 confirms that they have almost no interest in wider issues of any kind (Dunne, Harney, & Parker, 2008). Nor do they make much contribution to knowledge in other disciplines (Lockett & McWilliams, 2005). Long, long ago, Price (1965) found that roughly half of scientific citation was to a tiny subset of the literature.

> "Thus each group of new papers is 'knitted' to a small, select part of the existing scientific literature but connected rather weakly and randomly to a much greater part." (Price, 1965, p. 149)

Price sees this 'knitting' process as necessary to the development of a discipline — a matter that has been a burning passion in Management Studies. The citation pattern of papers in the top journals of Management Studies seems to indicate that the subject is now well and truly knitted. There is discipline, but discipline that isolates Management Studies from the wider academic community, and even further from the outside world. Not one of the 100 most cited papers in Economics and Business was published in a multidisciplinary journal (Ioannidis, 2006); and almost nothing in the top Management journals is of any interest to managers (Bennis & O'Toole, 2005).

Such heavy internal citation is the more remarkable in that reference lists are very much longer than they used to be, and cover a greater timespan (Lariviere, Archambault, & Gingras, 2008). In the 'seventies, the average academic paper cited something like 10 papers (Price, 1976). Table 2 gives some idea of the length of the reference lists attached to the papers of the top authors of Management Studies; they have grown. Perhaps authors cite papers from the journal to which they are submitting simply because that journal is to hand. We know that authors are swayed by convenience in their selection of papers to cite (Odlyzko, 2002). Papers that are freely available on the Web are more than twice as likely to be cited as those that are not (Lawrence, 2001), though papers available only online are rarely cited at all (Speier, Palmer, Wren, & Hahn, 1999; Young, Ioannidis, & Al-Ubaydli, 2008). Yet, even the oldest, fattest journals (those offering most to cite) publish but a tiny proportion of all that is citable. And top journals in Management Studies are hardly niche journals; they cover broad swathes of the subject, vast tracts of publication offering almost limitless citation opportunities.

Table 3 Self-citation (11 journals, 1987—2006).

Author	Total citation (incl. self-citation)	Self-citation rate
Furnham A	101	80%
Reuer JJ	8	63%
Clegg SR	152	55%
DeVries MFR	106	55%
Knights D	174	53%
Hitt MA	147	46%
Mitchell W	70	40%
Mumford MD	203	40%
Miller KD	30	37%
Baum JAC	142	37%
Alvesson M	226	35%
Sutton RI	86	33%
Zajac EJ	95	27%
Singh H	42	26%
Hoskisson RE	112	25%
Boeker W	67	25%
Carroll GR	165	21%
Hill CWL	110	20%
Child J	179	20%
Hambrick DC	289	11%

We turn now to our subgroup of just 11 journals to look at the authors of the 20 most cited papers in each. Let us start with self-citation. It is clear from Table 3 that these authors are inclined to cite themselves. Many are prodigious self-citers. For example, there are 152 citations to the works of Clegg, but 55% of these appear in the works of Clegg.

Table 4 looks at the authors cited by the top 194 authors in the 11 journals. Our interest lies in what we call 'citation cohesion', the extent to which a group of authors is bound together by citation. The table shows, for example, that 281 of Hoskisson's 495 cited authors in the dataset are also cited by Hitt. Put another way, 57% of Hoskisson's cited authors also comprise 33% of Hitt's cited authors. Or another way, 39% of the authors cited by Hill, are also cited by both Hoskisson and Hitt. Table 4 suggests that top authors in Management Studies are a clubbable lot, and offers a reminder that citation is, above all, a social process (Brooks, 1985).

Table 5 takes a different tack: we compare the citation of the most prolific 10% of authors in the 11 journals with the citation of the other 90%. What do we find? Big names dominate the list, but the distribution of their citation is far from even. Something like 36% of citations to Porter in these 11 journals come from this small group of prolific authors, 46% of citations to Williamson, 44% of citations to Hannan, 47% of citations to Jensen, and 50% of citations to Hamel. Why should half the citations to these big names come from just 10% of authors? At one level, the answer is quite obvious: the 10% of authors publish more papers. We are not particularly interested in calculating anything like a Porter-per-paper ratio. We are interested in what may be the answer at another level: the top authors of Management Studies tend to cite not only themselves and each other, but eminent authors generally.

Table 4 Citation cohesion (11 journals, 1987–2006).

Shared authors											
281	Hoskisson	Hitt									
	495	850									
	57%	33%									
196	Hoskisson	Hitt	Hill								
	495	850	501								
	40%	23%	39%								
167	Hoskisson	Hitt	Hill	Zajac							
	495	850	501	459							
	34%	20%	33%	36%							
161	Hoskisson	Hitt	Hill	Zajac	Singh						
	495	850	501	459	358						
	33%	19%	32%	35%	45%						
158	Hoskisson	Hitt	Hill	Zajac	Singh	Mitchell					
	495	850	501	459	358	357					
	32%	19%	32%	34%	44%	44%					
150	Hoskisson	Hitt	Hill	Zajac	Singh	Mitchell	Hambrick				
	495	850	501	459	358	357	376				
	30%	18%	30%	33%	42%	42%	40%				
142	Hoskisson	Hitt	Hill	Zajac	Singh	Mitchell	Hambrick			Miller	Reuer
	495	850	501	459	358	357	376			270	282
	29%	17%	28%	31%	40%	40%	38%			53%	50%
134	Hoskisson	Hitt	Hill	Zajac	Singh	Mitchell	Hambrick	Boeker		Miller	Reuer
	495	850	501	459	358	357	376	343		270	282
	27%	16%	27%	29%	37%	38%	36%	39%		50%	48%
113	Hoskisson	Hitt	Hill	Zajac	Singh	Mitchell	Hambrick	Boeker	Miller	Reuer	
	495	850	501	459	358	357	376	343	270	282	
	23%	13%	23%	25%	32%	32%	30%	33%	42%	40%	
104	Baum	Hoskisson	Hitt	Hill	Zajac	Singh	Mitchell	Hambrick	Boeker	Miller	Reuer
	330	495	850	501	459	358	357	376	343	270	282
	32%	21%	12%	21%	23%	29%	29%	28%	30%	39%	37%

When Management Studies was new, its thinking was inevitably dominated by authors from other disciplines, from Economics, Sociology, Psychology, Anthropology. That was some time ago; it is now generally accepted that Management Studies is a discipline in its own right, with its own intellectual core and its own top journals. And yet, not all the authors most cited in Table 5 seem to publish much in the journals in which they are cited. Table 6 reverts to the larger group of 20 journals and explores this observation over 40 rather than 20 years. Pfeffer is far and away the most cited author, followed at some distance by Williamson and Porter. Among the 10 most cited authors in our database, only Pfeffer, Hambrick, Miller and Weick publish much in our set of 20 journals. Although Pfeffer publishes a great deal in top journals, he spreads his favours among them and does not register as one of our most prolific group. Neither do Williamson and Porter, but in their case because they rarely publish in the top journals of Management Studies. When they do, their papers tend to be spin-offs from their books.

Pfeffer (2007) has recently listed the major theoretical contributions to the Organisation Sciences announced not in top journals, but in books and lesser journals (Table 7). When his list is compared with our own list of most cited authors (Table 6), considerable overlap is evident (shown in bold in Table 7). The suggestion is — and it can be no more than a suggestion — that those who are most cited by top authors in the top journals of Management Studies tend to publish their own work elsewhere, especially their major work. Important new ideas may eventually be acknowledged in the top journals of Management Studies, but they are not announced there. Now, it is tempting to assume that it is the inherent conservatism of top journals that repels new ideas (Rynes, 2006), but an alternative explanation is worth considering. Perhaps there is little new in the top journals of Management Studies less because they repel the novel than because many of the top authors publishing in these top journals have little new to say (see Beyer, Chanove, & Fox, 1995). And perhaps the demands of citation analysis explain why it is that they have little new to say.

A pattern seems to be emerging. Prolific publication in the top journals of Management Studies is associated with a range of citation behaviour: heavy internal citation, self-citation, group citation, and citation of eminent authors who do not necessarily publish in these journals themselves. Neither our data nor its analysis is comprehensive and robust enough to prove anything, but they are suggestive, raising issues that warrant discussion.

Table 5 Citation of eminent authors (11 journals, 1987—2006).

	Citation by all 194 authors	Citation by top 20 authors	% citation by top 20 authors
ALDRICH HE	114	46	40%
BARNEY JB	166	67	40%
CHANDLER AD	90	34	38%
CHILD J	162	33	20%
CYERT RM	126	37	29%
DIMAGGIO PJ	154	39	25%
EISENHARDT KM	192	51	27%
GREENE WH	54	21	39%
HAMBRICK DC	275	77	28%
HAMEL G	64	32	50%
HANNAN MT	300	132	44%
HARRIGAN KR	95	47	49%
JENSEN MC	225	105	47%
LAWRENCE PR	101	32	32%
LEVINTHAL DA	80	31	39%
LEVITT B	53	21	40%
MARCH JG	262	67	26%
MEYER JW	120	43	36%
MILLER D	272	77	28%
MINTZBERG H	262	65	25%
NELSON RR	126	50	40%
PENNINGS JM	43	17	40%
PFEFFER J	454	123	27%
PORTER ME	291	106	36%
POWELL WW	67	18	27%
PRAHALAD CK	80	25	31%
RUMELT RP	147	61	41%
SCHERER FM	80	31	39%
SINGH JV	72	25	35%
STINCHCOMBE AL	48	20	42%
TEECE DJ	160	84	53%
TUSHMAN ML	158	43	27%
WEICK KE	238	38	16%
WILLIAMSON OE	288	132	46%

Table 6 Citation to publication of eminent authors (20 authors, 1967—2006).

Author	Citation to this author in our dataset	Papers by this author in 20 journals 1987—2006	Papers by this author in 20 journals 1967—1986
Pfeffer J	523	13	31
Williamson OE	399	3	0
Porter ME	361	2	0
Hannan MT	307	6	0
March JG	297	5	8
Hambrick DC	289	21	16
Miller D	289	28	19
Mintzberg H	275	6	8
Weick KE	251	12	5
Bass BM	244	6	3

Discussion

In defense of the current system it is argued that quality will out, that the papers published in top journals are actually very good papers. Indeed, they may be. But Starbuck (2005) finds that top journals also publish papers that are not good, and that good papers are also published in journals that are not top journals. Authors seem to have little incentive to write papers that add to the sum of human knowledge. The single aim is to publish papers in top journals. What these papers say is less important (Callaham, Wears, & Weber, 2002). We have noted the gaming that permeates publication

Table 7 Major theoretical contributions to the Organisation Sciences not announced in top journals.

Barney	Resource-based view of strategy
Porter	Industry structure-conduct-performance paradigm
Williamson	Transactions cost theory
Jensen and Meckling	Relationship between agency theory and corporate governance
Bass	Charismatic leadership
Freeman	Stakeholder theory
Pfeffer	Organisational democracy
Staw	Escalating commitment to ineffective courses of action
Pfeffer and Salancik	Resource dependence theory

Source: from Pfeffer (2007).

in the top journals of Management Studies. Peer review is meant to ensure that only quality papers are published in top journals. However, the efficiency of a modern refereeing system is assessed in terms of whether referees reach the same conclusion (Marsh, Jayasinghe, & Bond, 2008; Starbuck, 2003). When referees agree, it is likely to be because they prefer the familiar to the unusual (Miller, 2006).

> "Between about 1984 and 2007, I did a great deal of reviewing. At the beginning I was a very conscientious reviewer. Then I learnt to review very much more quickly and effectively. Then about 4 years ago it started to become quite impossible: almost nothing I saw seemed worth publishing except the stuff that broke all the rules. So in general I am saying no to any more reviewing nowadays." (referee for the journal Prometheus, April 2009)

When referees screen for the acceptable rather than search for the exceptional, radical papers tend to be rejected while the humdrum pass muster (Armstrong, 1997; Gans & Shepherd, 1994). Authors respond by dumbing down to the level at which referees reach consensus (Horrobin, 2001). Other disciplines squirm in embarrassment at the rejection of classic papers by their top journals (e.g., Frey, 2003). The problem is rarely acknowledged in Management Studies: in Management Studies the pretence is maintained that top journals publish nothing but the best, and all the best is published in top journals. Top authors of top papers in top journals are those who have sufficient control over the system to remain top authors.

> "It is important to identify the most influential scholars because these individuals are the thought leaders who have made major conceptual or methodological contributions to our understanding of management processes and are also the gatekeepers who in their role as reviewers, editorial board members, and editors determine what is published." (Podsakoff et al., 2008, p. 642)

Are papers in the top journals of Management Studies actually read? Even in the 1960s, when there was very much less to be read, Merton (1968) found that few academics waded through papers in the journals of Chemistry and Psychology. Nowadays, when so many authors publish to score rather than to communicate, there may be less interest in reading their papers. Papers in the top journals of Management Studies are certainly cited, but, like entries in an encyclopaedia, they are valued more because they are authoritative than because they are readable. Indeed, the more impenetrable a paper, the better that paper is reckoned to be (Armstrong, 1980; Miller, 2006).

Perhaps the top journals of Management Studies, like their referees, serve to screen, to rescue academics from information overload by packaging and labeling knowledge (Lockett & McWilliams, 2005).

> "This triumph of form over substance is one of the more bizarre initiatives of the audit culture we inhabit. Prompted by the prevailing norms of efficiency and economic rationality, it is believed that the reading of an article can be bypassed altogether in assessing quality by assigning an A+, A, B, C or NR to the journal in which it appears." (Thornton, 2008)

Frey (2003, p. 218) thinks of the papers published in top journals as irrelevant, unoriginal and — perhaps worst of all — boring. Consider what Smith calls the 'impacted journal':

> "Everything readable and entertaining is cut in favour of material that will be cited. This means that a journal is designed for citing rather than reading and for authors (who can cite articles) rather than readers (who cannot)." (Smith, 2006, p. 1130)

When practicing managers talk about 'reading', the activity to which they refer does not necessarily entail looking at every word on every page. For many managers, 'reading' means no more than being aware of a publication. Academics in Management Studies may have come to share this perspective, persuaded by the "time limitations of busy scholars" (Judge et al., 2007, p. 504). Simkin and Roychowdhury (2003), having to take into account different subjective understanding of reading, are forced to re-define 'reader' as someone who may have done no more that consult a trusted source in putting together a reference list, not that top journals are any more likely that lesser journals to have correct citations (Wright and Armstrong, 1997). Sharplin and Mabry (1985, p. 143) recommend an 'impact efficiency' index, "useful for the scholar who wishes to obtain maximum research value for a minimum amount of reading". The expert panel on which the UK's Research Assessment Exercise depends to assess the quality of papers in Management Studies does not feel it essential to read those papers (Mingers & Harzing, 2007). It relies, in large part, on the standing of the journals in which they are published.

We extend Vickery's (1969, p. 170) aphorism that much that is read is not cited to observe that much that is cited is

not read. For instance, Jones, Brinn and Pendlebury (1996) admit that they cite Vickery without having read Vickery, which perhaps explains why their citation of Vickery is incorrect. In the past, academic indolence has been the explanation for poor scholarship (Mitra, 1970).

> "How often are citations simply lifted from the bibliography in someone else's work without either reading or giving credit to the man who did the original search of the literature? How often are citations tacked on after the paper is completed as an afterthought and window dressing?" (Kaplan, 1965, p. 181)

In Physics, some 80% of researchers do not read the papers they cite (Knothe, 2006). In Fisheries Science, the most cited book of all has long been out of print and is not available in most libraries (Holt, 1998). Merton himself had brought to Garfield's attention the proclivity to cite not just what others have cited, but as others have cited:

> "I have enjoyed the irony that the Matthew Effect ... is evidently at work in the frequent mis-citation of our joint papers as being by 'Merton and Zuckerman' even though Harriet Zuckerman is explicitly designated as the first, 'senior' author." (Merton cited in Garfield, 2004, p. 58)

To Merton, we are indebted for the observation that the Matthew effect in academic publishing gives further prominence to those who are already prominent. To Eugene Garfield, who saw that this characteristic concentrated citation and therefore offered commercial opportunities, we owe the current exploitation of citation analysis. For Merton, how so many authors could have had before them papers emblazoned 'Zuckerman H. and Merton R.K.' and then typed 'Merton, R.K. and Zuckerman, H.' was a rhetorical question. He — and he assumed other academics — already knew the likely answer. Broadus (1983) finds 23% of citations to one paper to be incorrect, presumably because authors had never seen the original (see also Evans, Nadjari, & Burchell, 1990; Gosling, Cameron, & Gibbons, 2004; Wright & Armstrong, 2007). The conclusion of Lok, Chan, and Martinson (2001) that citation error hardly mattered as long as it was still possible to find the paper in a library betrays the bibliographic focus of much research in this area. The librarian's responsibility stopped with the finding of a publication, Garfield's with its correct identification. The reading of the publication was never a concern of either.

As the importance of citation analysis has grown, so too has the incentive to game with the input to citation analysis. Editors and publishers use their webpages to tell authors who is citing whom, and stock reference lists are readily available from the Internet (Cookson, 2009). As the ever-prescient Kaplan noted ages ago:

> "It seems fairly certain then that the literature explosion plus the new technical developments in response to this explosion may trigger changes in both the behavior and the ethics involved in publishing generally and in citation practices in particular." (Kaplan, 1965, p. 183)

Academics are actually reading more than ever — reading occupies about 30% of their work time (Houghton et al., 2009, pp. 145–6) — but they are spending far less time on each paper (48 min in 1977: 31 in 2005) (Tenopir & King, 2007, 2008). They skim. Since the seventies, academics have turned from browsing and personal subscriptions to journals to automated searching through libraries. Such systems supply authors with papers, and with details of which are most cited, and cited by whom:

> "The ability to follow citation links easily may cause a kind of herding behavior, where authors gravitate towards those articles selected by their peers." (Tenopir, 2008)

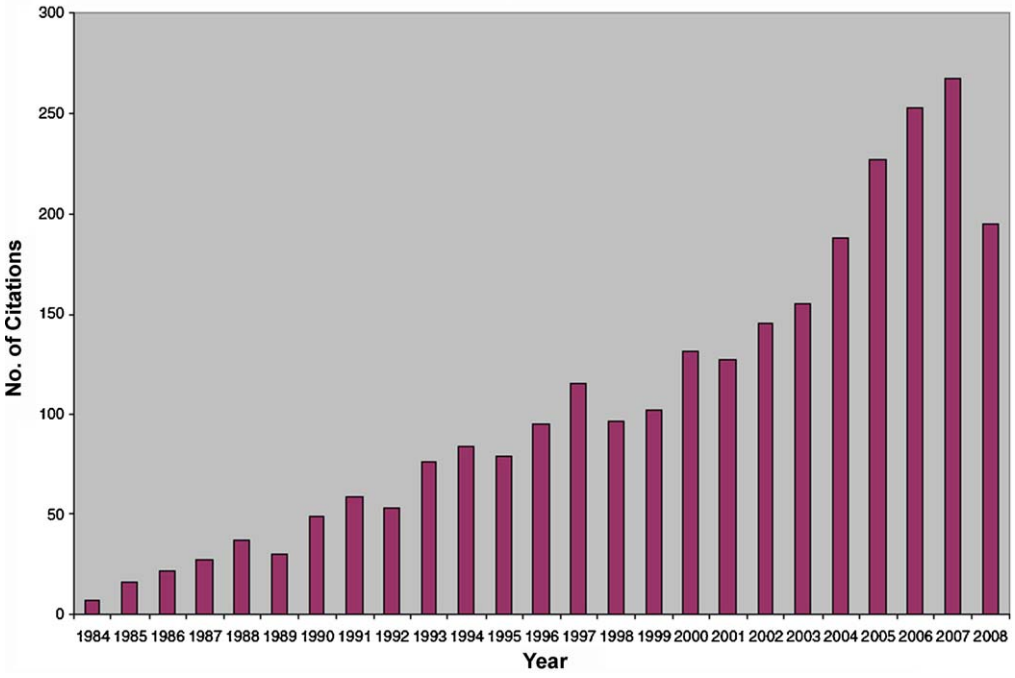

Figure 1 Annual citations to DiMaggio and Powell (1983) at 30th September 2008. Source: Greenwood and Meyer (2008), p. 259.

> "Choosing the best article to cite may be subject to peer pressure in the form of choosing more often to cite those that are cited by others. Following citation links in electronic journal articles may have proportionately more influence on citation behavior than reading behavior." (Tenopir & King, 2008)

Where there is common citation, common opinion cannot be far behind:

> "Searching online is more efficient and following hyperlinks quickly puts researchers in touch with prevailing opinion, but this may accelerate consensus and narrow the range of findings and ideas built upon." (Evans, 2008, p. 395)

Consider citations to a paper by DiMaggio and Powell (1983), 'The iron cage revisited' (Fig. 1): after 25 years, citations to most papers would be in steep decline. But top papers are an exception; the more authors cite DiMaggio and Powell (1983), the greater the incentive for authors to cite DiMaggio and Powell (1983). In theory, there will come a time when every paper in Management Studies cites DiMaggio and Powell (1983); trend projection suggests this will be achieved by about 2030.

> "... near-random differences in quality amplify when agents become aware of each other's choices. Agents view other's choices as relevant information—a signal of quality—and factor them into their own reading and citation selections." (Evans, 2008, p. 398)

Citing the right authors could at one time be dismissed as ceremonial behaviour (Small, 1978), cynical at worst. No longer. On citation all else depends. The consequence is a corrupted citation system, unable to support serious analysis.

Concluding thoughts

A great deal has changed in the half century since Garfield launched commercial citation analysis. The academic world is now much more competitive, much more managerial, much more market-driven. Universities have become businesses, selling research and education, tacit information whose production is difficult to measure and so to manage. Codification helps: education becomes qualifications, and research becomes publications. Just as students want to acquire a 2:1 degree (upper-second class degree according to British degree classification) rather than knowledge, so academics publish to score rather than to contribute to knowledge.

Fifty years ago, bibliographic data were of no particular use to anyone but librarians and specialists in scientometrics: now they dominate academic life. This is not the place to bewail a transition from a golden age that probably never was, but we do note that, amidst revolutionary change in the academic world, and in the use of these data, their provision and processing have hardly changed at all. The same company treats the same data much as it has always done. The work is still presented as bibliographic investigation, dispassionate and scholarly. Cultivated by Garfield over decades (Figa-Talamanca, 2007), the image survives the obvious commercial objectives of ISI, the vagaries of the ISI system (Rossner et al., 2007), and the predations of publishers (Cameron, 2005; Chew et al., 2007). Much resource allocation in higher education is based on data collected and processed by a monopolist whose operations are not subject to public evaluation (and never have been), a monopolist openly criticised for being secretive, illogical and inconsistent in its processes (Editors, 2006; Klein & Chiang, 2004).

ISI has never coped well with non-English language publications, books in any language, theses or dissertations, conference papers, working papers, grey literature or literature in any way irregular, non-print or multi-media publications, or even journals that are anything less than standard and established (Harzing, 2008; Schoonbaert & Roelants, 1996). While it was always in the public interest that the whole gamut of outlets for academic work be monitored, it was always in the interests of ISI that a small minority of bog standard academic journals be accepted as the only part of the universe of academic publication that mattered (see Garfield, 1973; Perkel, 2005) — 2.5% of the total by one estimation (Cameron, 2005). A recent Australian study concludes that ISI covers about 23% of Management journals (Butler, 2006). This matters: Harzing (2007) gives the example of Hector Garcia-Molina, a computer scientist who scores 240 citations according to ISI and over 20,000 according to Google Scholar.

For some years now, the Internet has offered more comprehensive databases than those of ISI, and a host of new bibliographic possibilities (Ashkanasy, 2007; Corbyn, 2009; Harzing, 2007, Norris & Oppenheim, 2006). These developments have been late and slow. Why should the academic and the wider community not rush to replace a system so weighted in favour of those who can win the game? Certainly the masses in Management Studies, struggling desperately — hopelessly — to publish in top journals, are positively deferential in their acceptance of reality (e.g., Klimoski, 2009). Academics have become, in the opinion of one observer (Frey, 2003), prostitutes. For a publication in the right journal, academics will give almost anything, and say even what they know to be wrong (Frey, 2003). Debate in the UK has focused on whether research quality is best measured by peer review or citation analysis (Mingers & Harzing, 2007), or some combination of the two, as seems to be favoured for the successor to the RAE. It matters not how a place in a ranking is determined: when reading is not required to assess academic papers, and academic papers are not written to be read (Segalla, 2008), the whole system has reached a pinnacle of pointlessness.

Garfield has published prolifically on citation analysis. His publications are a tribute to his energy and enthusiasm for citation analysis but, as a single body of work, they make little sense. His approach is undisciplined and contradictory (Cameron, 2005). For instance, Garfield warns against misuse of citation analysis, especially in impact factor form, and then happily misuses it himself (Cameron, 2005), even advising editors on how to fiddle the system (Garfield, 1996b). He insists that accuracy in citation is paramount — and then happily spells Lowry, the most cited author ever, with an 'e' (e.g., Garfield, 1996b). What Garfield does not do, despite his endorsement of almost any use of citation analysis, despite his personal supervision of its intellectual decline from Mertonian heights to commercial domination of academic life, is to admit that citation analysis can be anything other than

totally objective (Case & Higgins, 2000). On that fundament, the whole edifice of citation analysis is built.

> "Successful editors and publishers know that in order to improve the editorial quaky [sic] of journals, there is no substitute for judgment, quaky [sic] and relevance. Impact and other citation measures merely report the facts." (Garfield, 1996b, p. 412)

Well, no; citation analysis does not report the facts. Citation analysis reports what various interest groups make it report. Self-interest has eroded citation etiquette (see Pullum, 1988), not a matter that would normally raise hackles. But when so much hangs on citation analysis, it really is necessary to look closely at the citation on which the analysis is based. This paper has taken little more than a cursory glance.

The top journals of Management Studies offer no 15 min of fame for hundreds of authors, no memorable moment in the limelight. Instead, a very few authors dominate their pages, publishing many papers over many years, often with a particular concentration in a single journal. Mittal, Feick and Murshed (2008) examine the publishing record of 294 academic staff in 33 research-oriented marketing departments in US universities. They find that most have never graced the top four journals. More pertinent still is that a small group of 43 (14.6% of the total) have each published more than 10 papers in these journals. We have examined the citation behaviour of such elites, and we have considered the gaming this behaviour encourages. When academics are expected to say and do whatever will increase the chances of being published in a top journal, it is time to re-examine the whole system. Far too much has come to depend on what is basically footnote counting, and far too much has been lost as a consequence.

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