

The skewed few: people and papers of quality in management studies

Organization

18(4) 467–475

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DOI: 10.1177/1350508411403533

org.sagepub.com



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Abstract

Publication in the top journals of management studies is highly skewed. Very few authors publish in these top journals. They are said to be the best few, on the assumption that skew indicates quality. Yet, skew is natural in any distribution and would occur in the absence of all quality. Peer review is supposed to ensure that this cannot happen, but pressure to publish in top journals puts demands on the peer review system it was never intended to bear. One result is that the skewed few tend to be the same few. We look at how this is arranged. We investigate the citation of the skewed few. We find much self citation, mutual citation and group citation. This behaviour is encouraged by the paramount importance of the journal impact factor. The article looks at how this indicator has been contrived for commercial rather than academic reasons, and considers some of the consequences.

Keywords

journal, quality, skew

This article is about skew in academic publishing; about how the same few authors are published in the same top journals. We have dealt elsewhere with the circularity of the argument that top journals are top journals because they publish the best papers, and that best papers are best papers because they are published in top journals (Macdonald and Kam, 2007a, 2007b). Here we focus on the screening that is supposed to solve this problem. The guardianship of editorial boards and journal referees assures us that quality will out, that only the best will be published in top journals. And yet, as we have noted (Macdonald and Kam, 2009, 2010), a collegial system of peer review is sore

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pressed in a managerial world, and statistical obstacles confound the identification of quality when rejection rates soar beyond 90% (Macdonald and Kam, 2007a). Here we argue that peer review has been subverted to serve requirements that are not those of scholarship.

At the heart of the modern system of academic publishing is the journal impact factor (see Borokhovich et al., 1999), on which other journal rankings are based. Devised by Eugene Garfield in 1955 (Chew et al., 2007), but undeveloped for more than a decade (Garfield, 1996), the journal impact factor measures the quality of academic journals, which is allowed to indicate the quality of the journal's papers and their authors and the institutions and even the countries whence the authors come. Each year, Garfield's company, ISI, now 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). Garfield may have created the tool for measuring academic quality, but Garfield was never able to enforce its use, to impose the system that is now almost universal. We will argue that this was never required, that the skew and all that springs from the skew, did the work for him. Although we take our evidence whence we find it, our focus is on management studies, and although our argument is applicable in many countries, most of our evidence has been found in the UK.

From public to private property

Why do academics publish? The academic's reward—the esteem that leads to career progression, public recognition and some degree of self-satisfaction—was always reckoned to be a function of what the academic's publication contributes to society. Of course, no single academic could ever hope to add much to human knowledge. As with ants, the collective effort is what matters and the cumulative result is what benefits the group. Adding to knowledge is no longer the primary reason for academic publication. Academic papers are written less to be read than to be counted. The academic publishes to 'score a hit' (Leung, 2007), and much the highest scores are awarded for publishing in top journals.

Collegiality has been indispensable to academic endeavour, and the independent academic has been a contradiction in terms (Merton, 1936). Thirty years ago, it was usual to depict other academics as co-operating in common cause (Hagstrom, 1974): now they are seen as competitors. The invisible hand has groped its way into the invisible college. A simple indicator of academic performance is essential to the manager's control in the modern university (Willmott, 1995). The lay manager needs a number to tell him what academic 'output' is good and what is not. As employees, academics are paid to publish, just as salesmen are paid to sell, and with very similar performance targets. And much like salesmen, they now look to their employers to reward their performance. Employers oblige because papers published in top journals are valuable for them too; they direct government funding and help determine ranking in the league tables that so influence the decisions of the university's customers (see Wells, 2010).

So great are the rewards for publishing in top journals that gaming is rife (Macdonald and Kam, 2008) as the many struggle to join the few (Segalla, 2008). We consider here the role of the cartels that have come to dominate academic publication. As competition to publish in top journals grows ever more fierce, it might have been expected that demand would have created a few more top journals. Not a bit of it. As reference lists grow ever longer, it might have been expected that they would contain a greater variety of names. Not a bit of that either (Segalla, 2008). A slot in a top journal is a valuable commodity only as long as it remains scarce. It is in the interest of those who can realize this value to maintain exclusivity (Young et al., 2008).

Limiting supply to boost prestige might be an appropriate strategy for expensive handbags or fashionable nightclubs. But is it right for the exchange of scholarly thought ... ? (Segalla, 2008:126)

The skewed few

It is not to be expected that academics publish equally. They never have. Their publication is highly skewed (Seglen, 1992) or, as one commentator carelessly but aptly puts it, 'screwed' (Hansson, 2006: 273). Some publish a great deal: many publish very much less. A very few publish a great deal in top journals: most do not publish in top journals at all. Many are rarely cited, some never. ISI has no truck with the intellectual argument that an uncited paper may be just as important as a cited paper (e.g. Garfield, 1998; Hamilton, 1990, 1991; Pendlebury, 1991); for ISI, citation analysis distinguishes the aristocracy of academic publication from its trailer trash.

The idea of top authors publishing top papers in top journals has a definite neatness to it. Narin and Breitman (1995: 509) relate this symmetry to the normal distribution of intelligence, and to the 'well-known 80:20 rule of thumb that 80% of the work in any organization is done by 20% of the people', a moral stance also adopted by Garfield (2006). They further observe (1995: 519) that this skew 'codifies a great deal of intuitive perception on the part of many laboratory managers', and that 'the key role of a few researchers seems to be a law of nature'. Derek de Solla Price (1964: 657) was utterly captivated by the ability of the new citation analysis to identify the very best academics: 'We must realize soon that the existence of these groups is both natural and good'. William Shockley, winner of the Nobel Prize for his co-invention of the transistor, was a kindred spirit. According to Shockley (1957), an advocate of sperm banks and an energetic donor, skewed distribution in creativity and skewed distribution in academic publishing are as natural as the skewed distribution of tall men. Shockley is often cited, not unfavourably, in early work on citation analysis (e.g. Allison and Stewart, 1974), which—following ISI logic—affirms the quality of his papers. Curiously, Judge and Cable (2004) also examine the relationship between height and career success, and what brings success in publishing in management studies (Judge et al., 2007).

Pareto, Lotka, Zipf, Yule, Bradford and others show skew to be the natural order of things (Simon, 1955; see Martindale, 1995). What is unnatural is even distribution. No matter how Herculean their efforts, the many will never join the few. Of course, this does not mean that the skewed few need always be the same few. When that happens, skewed distribution is no longer the product of mathematical law, but of human intervention (see Verdaasdonk, 1985; Barker, 2007). The few have an obvious incentive to resist the rabble, defending standards in an honourable cause. Podsakoff et al. (2008) have no trouble depicting the few as heroes protecting quality.

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: 642)

The skewed few are understandably ready to accept that quality explains their distinction. More curious is that this is also accepted by the many, who desperately game to join the ranks of the few. They surely realise there will be skewed distribution in academic publishing whatever the criteria—red hair and bad breath will produce quite as much skew as brilliance.

... the publication and citation measures are calculable consequences of a given set of communication patterns and of a given number of people in the communicating group, and are independent of whether the group is set to do science or set to determine how many angels can stand on the head of a pin. (Moravcsik, 1973: 274–275)

There is no shortage of examples of the really rotten becoming the accepted standard of quality. There is VHS, a second rate product that nevertheless came to dominate the market (Martindale, 1995), in large part because VHS camcorders could provide the spontaneity required by the pornography industry. Then there is the QWERTY keyboard that condemns us all to a distribution of keys least likely to jam the levers of nineteenth century typewriters (David, 1985).

Where we observe the predominance of one technology or one economic outcome over its competitors we should ... be cautious of any exercise that seeks the means by which the winner's innate 'superiority' came to be translated into adoption. (Arthur, 1989: 127)

Peer review

Editors select referees ('reviewers' in American parlance) to assess the papers submitted to their journals. These are traditionally other academics, experts in the subject of the paper. Refereeing has two basic purposes: to determine whether the paper adds to knowledge (and therefore that it builds—and builds soundly—on existing knowledge), and to offer the author and editor advice on how the paper might be improved. But the referee's role has changed. The referee is now less mentor and more border guard, screening to ensure that the journal publishes only papers that are good, not necessarily for society, but for the journal. In practice, the referee roots out whatever smacks of nonconformity (Hansson, 2006; Miller, 2006) in favour of papers that are, according to one member of the Business and management studies panel for the UK's Research Assessment Exercise, 'bland, self-evident and endlessly citable' (Paul, 2008: 328). What was once advice to the author is now demand (Frey, 2003), to be accepted on pain of rejection (Merilainen et al., 2008). And frequently it is the referee, rather than the editor, who will judge rounds of revisions. Peer review has come to be, as Oswald and Jalles (2008) understand it, any assessment of a paper between submission and either publication or rejection. Referees have joined editors on the journal's executive team. A collegial role has become a managerial role (Miller, 2006; Starbuck, 2003) and when referees have managerial authority, it can never be quite certain that the esteem in which they are held stems entirely from their expertise. *Quis custodiet custodiam?* Actually, no one.

Pressure to publish makes impossible demands on peer review, made many times worse by cascade submission. Authors typically send papers to top journals first, and then work their way down with each rejection, making further demands on the refereeing system with every iteration. Ceci and Peters (1982) consider an 80% rejection rate too high for peer review to make sense, so prone to random error is the system. At 90%, a rejection rate exceeded by many top journals in management studies (Judge, 2003), the system is just too noisy to work (Miner, 2003).

And yet, a high rejection rate is accepted as confirmation of journal quality:

I would like to thank you for your ongoing support of EMJ, and update you on how well the journal is going ... The rejection rate has raised from 80% to 86%. (Laroche, 2010)

Even conferences have come to see quality in terms of what is excluded rather than what is included:

ICEIS is interested in promoting high quality research as it can be confirmed by last year [sic] acceptance rates, where from 448 submissions, 14% were accepted as full papers. (ICEIS, 2010)

A high rejection rate also strongly suggests that many submissions never go anywhere near a referee; they are summarily rejected by other members of the editorial team ('desk rejected' is the term), perhaps by editors, more likely by editorial assistants. Monastersky (2005) finds that about half of papers submitted to some top journals are desk rejected. Authors not among the few are much more likely to be desk rejected.

... the same old researchers taking the same old line tend to get published. My experience is that there are only about three or four names in my field who get published. If you are not one of them or you are not connected with them, you haven't got much chance. (Academic quoted in Bunting, 2005: 18)

Citing the few

When citation analysis was still young, Moravcsik (1973) predicted its control by elite groups.

Authors cite not necessarily quality articles, but 'network' articles, i.e. they cite potential referees, friends, colleagues, or editors. (Jones et al., 1996: 598)

Certainly it seems that authors of papers in the top journals of management studies are inclined to cite their own publications. There is, of course, nothing wrong with self citation. It can be an efficient and considerate alternative to repetition of information already published. But much as a dog marks its territory, self citation is also a claim to property, and our society is keen to protect property rights.

... the citation practices of scientists today are in large part a social device for coping with problems of property rights and priority claims. Only incidentally do these citations serve as a careful and accurate reconstruction of scholarly precursors of one's own contribution. (Kaplan, 1965: 181)

ISI is unfazed by self citation: 'A high volume of self-citation is not unusual or unwarranted in journals that are leaders in a field because of the consistently high quality of the papers they publish ...' (McVeigh, 2009). The few also tend to cite each other, happily engaging in what the economists call a 'citation exchange' relationship (Diamond, 1986). Intriguingly, the few also tend to cite within their own elite group. These citation arrangements make the papers of the few particularly likely to be cited (see Medoff, 2003). Above all else, editors of top journals need to publish citable papers to match the impact factors of other top journals. The more the papers of the few are cited, the more authors desperate to be published in top journals are obliged to cite them. Their citation signals to editors and referees that the author knows the natural order of things and his place within it. Only obtuse authors need reminding: '... it is paramount that you make several explicit references to other articles published in SPP' (*Science and Public Policy*, 2009). Electronic lists tell the uncertain what has been most cited and thus what should be most cited. Publishers help too by providing free access to promote their most citable papers. The more the few are cited, the more the impact factor rises, as does the journal's dependence on such citation to maintain its impact factor.

The skewed few have been able to turn what was always their 'accumulative advantage' (Merton, 1942/1973; Cole and Cole, 1973; Allison and Stewart, 1974) into something more insidious. There are other pertinent observations. The first of these is that most papers are now co-authored. The number of authors of the average scientific paper has doubled in the last 20 years (Jump, 2010), and the higher the journal impact factor, the more authored the paper (Amin and

Mabe, 2000). More authors mean more self citing, which means more citation of the paper, which boosts the impact factor of the journal publishing the paper.

Those who frequently publish in top journals often write with junior colleagues. Indeed, top authors seldom write alone. Long ago, Merton (1968) noted the enthusiasm to publish with top authors. Given the current value of a paper in a top journal, and the relative ease with which top authors publish in these journals, junior authors can be no less anxious to ally themselves with their seniors. Put bluntly, top authors are able to grant entitlement to publish in top journals.

I remember telephoning a famous professor because I had been sent a paper for my journal with his name on, and there was something in it that puzzled me. So I rang and he said 'What paper?' and when I told him he clearly had no knowledge of it at all. (Harvey Marcovitch as quoted in Tahir, 2008: 33)

The academic tradition of some disciplines is that everyone associated with a research project becomes an author of papers emanating from the project. No one writes all of the paper, and some may write none of it. Shockley (1957), for whom the skew was Nature's way of identifying quality, observes that it hardly matters if the writing is left to junior partners because the eminent could easily write the paper themselves if they had to. It was the ability to write papers published in top journals rather than the actual writing of the papers that defined a star. About a quarter of papers in the top journals of Medicine have guest authors (US Senate, 2010). Ghosting is also rife in Medicine (Kmietowicz, 2004), commercial sponsors of academic research providing technical writers to generate papers for eminent academics (Lacasse and Leo, 2010), no doubt quite capable of penning their own papers if they really had to. The *Academy of Management Perspectives* now supplies a professional writer to assist its authors.

Conclusion

This article has looked at the skew in academic publishing. Skew may be natural, but there is nothing at all natural about its association with quality in academic publishing. That is the accomplishment of Machiavellian Man. Eugene Garfield was the first to use citation analysis to identify the skewed few. Had he not been able to convince others that skew denotes quality, or can be portrayed as denoting quality, the commercial prospects of citation analysis, and of ISI, would have been limited.

Always this skew in academic publishing is presented in terms of quality coming to the fore. It is presented this way by the company providing the citation data, a company whose business model is dependent on this conclusion being reached (see Corbyn, 2008). And it is presented this way by the skewed few themselves, who are not at all averse to the idea that skew is determined by quality. The situation is accepted by a lay world uncomfortable with the unmeasurable and deeply suspicious of the professional, a managerial world that demands control over professional labour. And—let us be frank—it is accepted by academics themselves, many of whom now have more capital in gaming skills than in scholarship.

We have not dwelt on the problems this situation poses for the intellectual development of management studies. It is probable that new ideas are being discouraged and those who suggest them disadvantaged. But spare the tears; the key players in this tragedy are not editors or publishers, universities or government. Heading the *dramatis personae* are academics themselves. They have allowed this situation to develop; the few have entrenched themselves, but the many have been complicit in the hope that they will profit from knowing the rules of the publishing game and from being unscrupulous in playing it. The way out of this mire involves the academics of management studies writing papers actually worth reading, and publishing them where they will reach the most

suitable audience (Tourish, 2010). The skewed few might do something to redeem themselves by leading the way.

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