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Working Paper Series

Moulding the One-Dimensional Academic: The Performative Effects of Journal Ranking Lists

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Moulding the One-Dimensional Academic: The Performative Effects of Journal Ranking Lists

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The paper critically examines the case for using academic journals lists. Its focus is upon their development and use in principle, and not upon the detail of their construction. It is argued that an effect of their “one size fits all” philosophy is to endorse and cultivate an academic monoculture in which *particular* criteria, favoured by a given list, assume a *universal* relevance and legitimacy. We show, with reference to the Association of Business Schools (ABS) ‘Journal Guide’, how the use of a particular journal list can come to dominate the scholarly terrain of a particular discipline and that this can be economically as well as culturally detrimental to scholarship and innovation. We argue that this is driven by the managerial anxieties of business school leaders and can create a kind of list fetishism in which the publication journal assumes a greater importance than the content of the paper.

Introduction

“If we are to evaluate the quality of academics’ research, as I think we are destined to do by the nature of our work, then I believe we owe ourselves the courtesy of actually reading their work and making up our own minds. And if we can not be bothered to do this, then we should refrain from making a judgment in the first place. Remote judgments about the quality of academic work on the basis of where it has ended up are simply inexcusable” (Milne, 2002: 84)

Journal lists exemplify a formal, standardized measure of overtly meritocratic elitism. Such elitism is endemic to academia, a world in which journals dominated by “White men from North America and Britain” (guilty as charged) claim to “serve international audiences” (Özbilgin, 2009: 114; see also Özbilgin, 2004). Entry to higher education is restricted to an elite possessing the required certification, and only a small fraction of students qualify to register for a PhD, the minimum requirement when applying for an academic position. So, there is little point in criticising the *elitism* of journal rankings lists, or indeed the competitiveness for scarce resources that they induce (Adler and Harzing, 2009; Giacalone, 2009), unless the critique is extended to the organization of higher education and beyond. Here we stick with journal lists in an attempt to show what, beyond their elitism, is specifically objectionable about them.

We will argue that it is not the detail of how journal lists are constructed that merits the closest scrutiny but, rather, their development and use in principle. Why? Because their effect is to cultivate (or more deeply embed) a monocultures (see Nkomo, 2009; Macdonald and Kam, 2007) promoted by their “one size fits all” philosophy. Our target, then, is the

performative consequence of using a single, universal metric to calibrate academic work. Subscription to this logic assumes and fosters a one-dimensionality where *particular* criteria, underpinned by a specific value-orientation, assume a *universal* relevance and legitimacy.

Architects of journal lists may pay lip-service to academic diversity but this is suppressed or undermined wherever a journal list monoculture takes hold. Regardless of the methodology of list construction (see Morris, Harvey and Kelly, 2010 for a typology), horizontal diversity (e.g. in business and management research) is shoehorned into a single vertical list which is then often subdivided into “higher” and “lower” quality bands, or starred groups, equivalent to the divisions found in sporting league tables.

To the extent that the values institutionalized in the most highly ranked journals, and also favoured by reviewers of rankings lists when selected on the basis of their publication records, peer recognition is reduced to the proxy of publishing in the most highly ranked journals. In this article, we elaborate upon these concerns using a comparatively informal and accessible style. Our purpose is to stimulate reflection and debate by drawing together elements of a critique that, to our knowledge, have not been previously articulated in a systematic manner.

Scholarship - just like football?

The “one size fits all” logic asserts that there is a single basis for comparing the quality of one journal against that of others. A metric is constructed upon some combination of (1) measures of impact derived from numbers of citations in selected journals and (2) forms of peer evaluation. The application of the metric produces a rank order which is often then hierarchically divided into “top” (A/world elite) “middle” (B/international) and “bottom” (C/national) categories.

This emphasis upon citations, it could be argued, is blind to any particular values or criteria as it simply counts the times an article has been found sufficiently relevant to reference. However, wherever citations form the basis of a ranking metric, the list privileges journals that publish topics which are widely researched, use methodologies that are frequently deployed, and engage theoretical frameworks that are well established. Correspondingly, the metric marginalizes journals dedicated to less popular / emergent areas of research and methodologies. In this respect, the journals which contain the greatest proportion of highly cited articles rely, like football teams, upon a wide fan base and tend to benefit from well resourced and influential sponsors – notably, organizations like the *Academy of Management* and *European Group for Organization Studies*, (whose membership automatically receives a selection of its journals). And, like globally branded football teams, these journals tend to attract the star players - the editors and board members who command the greatest intellectual capital.

Citation-based metrics reward journals that happen to have large readerships in absolute terms, which could be by virtue of the generality as opposed to the specificity of the research area; or it could be simply that they are primarily American in focus, America having the

largest number of researchers in any area of business. And, in the form that citation-based metrics are most commonly used as journal impact factors (JIF) (one of the major inputs to the Association of Business Schools (ABS) list, for example, which we consider below), they also reward journals whose papers have a degree of immediacy. Whilst this may be justifiable in fast-moving areas such as the natural sciences, it is less defensible for the social sciences. Mingers (2008) studied 600 papers published in 1990 in six well-known management science journals. Three significant findings were: that the citations for these papers did not reach their peak until six years after publication; that many papers were still being cited fifteen years after publication; and that it was not possible to predict the eventual number of citations a paper received from citations in the early years. There are “shooting stars” that are cited highly and then fade into obscurity; and there are “sleeping beauties” which may be so innovative that their importance is only recognised some years later. Surely papers that make a sustained contribution, in terms of citations, over many years are of the greatest value?

What happens, then, if a specific area of scholarship or a favoured approach to its investigation, does not share the particular set of values privileged in the most highly cited journals, or which is favoured by a particular set of reviewers? Simple, journals dedicated to such areas and approaches are invisible to citation indices and so are comparatively marginalized (poorly rated) within, or even excluded by, the ranking list. An example is scholarship on sustainability (Wells, 2010) but it applies to a wide range of innovative work which embraces non-mainstream methodologies and perspectives. It also includes multidisciplinary scholarship that falls between tightly policed disciplinary silos. (see Meriläinen, Tienari, Thomas and Davies, 2008). Notably, new journals, in which innovative topics and approaches are more readily solicited and accommodated, are invisible in most citation indices, and so are missing from or lowly ranked in journal lists. As Adler and Harzing (2009: 80) have observed, the ranking of journals “dramatically skews scholarship as it implicitly encourages conservative research that asks familiar questions using accepted methodologies rather than research addressing new, often controversial questions that are investigated using innovative methodologies”.

In the UK, the imagery of football, with its organization into divisions, is frequently used to characterize journal ranking lists. But the relevance of the sports league image is limited since, in competitive sports, team managers have the same objective – for example, in football the aim is to score goals without conceding. The objectives of journal editors, in contrast, are, despite intensifying “isomorphic pressures” (Rowlinson, Hassard and Mohun, in press: 167) comparatively heterogeneousⁱ. Diligent editorial teams seek to attract and publish “good” or “the best” papers for their journals which are often established in response to an emergent topic, approach or innovative perspective, and not to attract work that might otherwise appear in the top ranked journals. We acknowledge that editors are also under reputational and commercial pressures, which are intensified by journal lists, to increase their “impact factors” either by reputable and, if the accusations are to be believed, more underhand meansⁱⁱ. Still, and this is the critical point, editors and contributors to journals participate in diverse epistemic communities (Knorr-Cetina, 1999) where divergent views of

what is “good” or “the best” are developed and defended. One size does not, and cannot, fit all. The assumption that diversity can be shoe-horned into a list simply advantages scholarship published in journals that share and “tick” the favoured criteria (see Grey, 2010). Conversely, such shoe-horning exercises exclude and devalue work that departs from, and therefore scores low, on the favoured metric.

A good example of the hegemonic power of journal editors is that of the American journal *Operations Research*, one of the supposed top two in the world in its field. The subject of operational research (OR) developed from scientific and mathematical roots, but since the 1970s, especially in the UK, the limitations of mathematical modelling for complex real-world problems has been recognised and a whole new area of OR has developed, known as “soft OR” or “soft systems” (Checkland, 1981). No papers on soft OR have *ever* been published by *Operations Research*, or the other top US journal, *Management Science*. When challenged by a letter in *OR/MS Today* (Ackerman and 48 others, 2009), signed by 49 academics from around the world, the response from the Editor was that, as far as *Operations Research* was concerned, non-mathematical OR was simply not OR and therefore not publishable. Given that these two journals are the only two in the Association of Business Schools (ABS) list to be ranked 4* for OR, soft OR academics are, by definition of the list, incapable of, or disqualified from, producing work of world standard.

One best list : Taylorizing business school scholarshipⁱⁱⁱ

At worst, this key point about suppressing or inhibiting diversity (see Van Maanen, 1995) is ignored, or it is briefly acknowledged before being disregarded by the architects of journal lists. Here we focus upon the list prepared by the ABS in the UK. Its creators note that any ranking of journals courts “the danger that highly original work fails to make a significant contribution to the field because it is damned by the name of the publication it appears in” (Morris, Harvey and Kelly, 2009: 1449); and, conversely, they observe that work appearing in a highly ranked journal can be seen in a better light than it would otherwise merit. Nonetheless, their production and refinement of a single list, which aspires to be definitive and of universal applicability, is *justified on the grounds that those charged with a responsibility for making decisions about academics’ careers do not prioritize time for reading and assessing the work itself*.

Devising and using the ABS list is warranted, its architects declare, because “people do not always read all that they are expected to read” and “it is surely a good thing if a systematic method of determining journal quality, like the ABS guide, is used”; the alternative being “the unsystematic and imprecise methods that might prevail in the absence of ranking journal titles” (ibid: 1449). Furthermore, the creators of the ABS list assert that, “although high quality research may on occasion be published in lesser ranked journals and *vice versa*, these exceptions to the rule do not invalidate the overall assessment of the quality of the research published in a journal” (Harvey, Kelly, Morris and Rowlinson, 2010: 4).

We beg to differ (see also Starbuck, 2005). In doing so, we acknowledge that the desire to construct a list is difficult to resist on pragmatic grounds. Yet, like Taylorism which shares a passion for standardization, the impulse is remarkably naïve, inherently problematical and, in our view, ultimately indefensible. The appeal of such lists stems from an alluring conflation of formal consistency and substantive diversity. The formal features of precision and systematicity are prioritized over an appreciation of what the architects of journals lists recognise and then conveniently overlook: that is, the “differences in values, theoretical reference points, methods, and writing styles” (Morris, Harvey and Kelly, 2009: 1449).

If the *heterogeneity* of the field is taken seriously – that is, valued - and not brushed aside, then the “overall assessment of the quality” of articles published in a particular journal cannot convincingly be made by using a single, *homogenising* measure. The empirical “fact”, if it is taken to be one, that lists are “frequently used in the UK and other countries to aid internal and external reviews of research activity and the evaluation of research outputs” (Harvey, Kelly, Morris and Rowlinson, 2010: 2) is not itself a compelling basis for advocating or justifying their use. To the contrary, the observation that the place of publication substitutes for an assessment of an article’s substantive contribution or merit is a provocation to critique and reverse the degeneration of an academic culture in which we set aside insufficient time for reading and debating the contribution of scholarly work; and where heterogeneity is sacrificed at the altar of contrived precision and systematicity. The outcome of repressed diversity is “list fetishism” where, in common with other (e.g. sexual) fetishes, an awesome power is attributed to an object – in this case, the power of a list to measure the quality of scholarship. List fetishism is exhibited whenever the standing of the journal (the equivalent of the fetishised shoe) in which an article is published assumes an importance greater than its specific contents (the equivalent of the erotic qualities attributed to a shoe). Whenever list fetishism is unchallenged, debate is confined to sterile speculations about, and endless arguments over, the selection or weighting of the criteria used to create the list – what Giacalone (2009: 124) terms “metricality”. The reductionist, “one size fits all” logic is retained and indeed reinforced even if the shoe size, as it were, or the design elements of the fetish object, are modified^{iv}.

List fetishism and performance anxiety: Users and pushers

So, why is there list fetishism? It would be over-ambitious and contradictory to suggest that there is a single, universal answer to this question, and we would not wish to exclude other elements of an explanations that might focus, for example, upon our own over- identification with a genre of scholarship that is widely taken (for granted) to be exemplary. Indeed even if we are cynical about such scholarship and the journal ranking systems which affirm its first-rate status, “we may still act *as if* we believe in them (Fleming and Spicer, 2003)” (Nkomo, 2009: 110). We will also limit our comments to the context with which we are most familiar - business schools in the UK.

What we have to say may, in part, be applicable to other circumstances but that is for readers to judge. Our suggestion is that the appeal of list fetishism (and its presenting disorder, list

mania, where academic conversations increasingly turn upon journal rankings and “hits” rather than the substance and merit of scholarly work) is that it offers an antidote to uncertainty. Yet, in providing the antidote, the fetish perversely exacerbates what it seeks to relieve: anxiety associated with precarious self-esteem. Performance anxiety then prompts further resort to the fetish object – go shopping for shoes! - to ease the pain. Relief is sought by producing work that will “hit” a journal that is highly rated in a (fetishised) list.

When bound by this fetishism, scholarship is shaped and bent to examine the topics; to address the issues; to use the methods; and to adopt the presentation formats most likely to find favour with the targeted journal. To obtain the gratifying buzz of acceptance, there is a price to be paid as well as compromises to be made along the way. Most obviously, cowed compliance with referee demands that the author assesses to be irrelevant or detrimental to his or her research may induce feelings of self-loathing and guilt. The gratification provided by the journal “hit” is temporary and its effects are bitter-sweet. Like the addict, thoughts immediately turn to how the next “hit” is to be obtained. In short, a common effect of such game playing (see Macdonald and Kam, 2007) is increased dependence upon continuing hits for self-esteem that is repeatedly undermined where compliance is forced.

If, in an era of rankings list fetishism, academics are the “grade junkies” then who are the “pushers”? Our suggestion is that a key (not *the* key) to understanding rankings fetishism is the insecurities of managers, notably Dean’s and their Associates, about performance. With the commercialization of higher education (Willmott, 2003), rankings have come to assume an increased importance which should not, in the UK context, be abstracted from the Research Assessment Exercise^v (RAE) in which the performance of University departments, including their business schools is evaluated. These periodic exercises are materially, and not just symbolically, important as they influence the allocation of central government funding for research. RAE rankings are also used directly, or are selectively mined and manipulated, as part of marketing efforts to attract students, to hire and retain staff, and to obtain research funding from other sources. Indeed it is hard to overstate the effects of successive RAE’s on the fortunes of UK universities, their business schools, and on the research and publication practices of academics.

For a majority of UK business school Deans, it is the RAE ranking that signifies the status of their institution, and it also indicates their school’s rise or decline in the pecking order since the previous exercise^{vi}. Submissions to the UK’s RAE, distinguish “research-active” from “inactive” staff and a number of “research outputs” (e.g. publications) is selected for each staff member who is submitted (for the 2008 exercise four research outputs were normally submitted, with allowance made for early career staff, periods of leave and illness, etc). These outputs, of which 90% were journal articles in the 2008 exercise, are then evaluated by a Panel. In the case of the 2008 RAE, the Panel comprised 18 specialists across the broad terrain of business and management research. As the research outputs submitted for evaluation by the Panel account for a high percentage of the full assessment (for the 2008 exercise it was 70% with Research Environment (20%) and Esteem (10%)) it is unsurprising that choosing which staff to enter and then which of their research outputs (e.g. publications)

to include was a great concern to Deans whose performance, as institutional managers (or academic leaders, if you prefer), would hinge upon the outcome of the RAE. The fetishising of lists, we suggest, presents an attractive remedy for the performance anxiety of Deans.

Deans in the hot seat

A feature of business and management research is its dizzying diversity. This heterogeneity is acknowledged by the architects of the ABS list who note that “[t]he field has resisted normative pressures to coalesce around a set of ontological, epistemological and methodological norms (Tranfield and Starkey, 1998)” (Morris, Harvey and Kelly, 2010: 1444). The field is seen to resemble “an urban sprawl” (see Becher, 1989) that manifests “conspicuous and persistent differences in values, theoretical reference points, methods and writing styles” (Morris, Harvey and Kelley, 2009: 1444). To this observation, it might be added that such differences are often as acute and delicate *within* business schools as they are between them. Faced with diverse specialisms, disciplines, methodologies and factions, the issue facing Deans when preparing the RAE submission is a stark and uncomfortable one. How is the eligibility of individual members of faculty to be determined; and where is the line to be drawn which effectively declares the low value ascribed to the research undertaken by excluded staff? And for those who are submitted, on what basis are their “outputs” to be selected from CVs that may include authored books, edited books, book chapters, conference presentations reports, etc. as well as a significant volume of journal articles spanning a period of several years (e.g. 2001-7)?

Perhaps a hint of an answer to these questions lies buried somewhere in the terms of reference of the assessment exercise itself. In the case of the 2008 exercise, the stated objective of the RAE was to “assess [the] quality” of research outputs, the research environment and esteem, with each element being rated using a common scale (Exhibit 1).

PLACE EXHIBIT 1 HERE

For Deans and their associates charged with preparing their school’s submission to the RAE, the challenge was to select the staff and the outputs that would deliver the most highly rated outcome. When selecting the outputs for each members of staff, it was relevant to bear in mind that outputs judged as 4* would attract a significantly higher (but yet to be stated) weighting for determining funding levels than those judged to be 3*, 2* and so on. So, the pressure was on to submit outputs that would most likely be judged by the RAE Panel as “world-leading in terms of originality, significance and rigour”; and to minimize the number of outputs that would be regarded by the Panel as 2* or below. When deciding which staff and outputs to select, a tricky and uncertain balance had to be struck between, on the one side, maximizing the number of staff submitted as this quantum would provide the multiplier for funding based upon the overall profile and, on the other side, minimizing the staff submitted with outputs likely to be assessed as 2*, 1* and unclassified (the so-called “tail”).

Did the assessment procedure, published by the Panel in advance of the exercise, point to a possible solution to this vexing problem? It was known that the Panel would have a very large number of submissions and inputs to evaluate (in the event there were 12,500 outputs, of which 92% were journal articles, up from 80% submitted to the 2001 RAE). But the size of its membership (18 supported by a smaller number of specialist advisors) would not be correspondingly greater than other Panels which had far fewer outputs to assess. Moreover, the Panel for the previous (2001) exercise publicly acknowledged that they read “15-30 per cent of outputs with some reading as much as 75%” (Bessant *et al*, 2003: 53). They were silent on the question of how they assessed outputs that were not read. Given the anticipated high volume of outputs and the comparatively small size of the Panel for the 2008 RAE, would its members find the time to make a detailed assessment of all the outputs? Surely, they would have to rely upon some other, proxy measure of quality?

PLACE EXHIBIT 2 HERE

The 2008 Panel’s Working Methods (see Exhibit 2) explicitly excluded the use of any journal lists, and clearly stated that “the assessment will be one of expert review based on professional judgement”^{vii}. Yet, a lingering question remained about how practically this could be achieved in the time available to the Panel (a few months). In its statement of Working Methods, the Panel undertook “to collectively examine in detail at least 25% of the submitted outputs”. So, how were the remaining 75% to be assessed?

It could be inferred that a majority of outputs would be examined in less detail. Their evaluation might rely upon the “professional judgement” of panel members but might also extend beyond its exercise. Such calculations pointed in the direction of Panel members’ probable reference to, and expedient use of, journal lists. Whatever the explicit and repeated pronouncements of the Panel, they would surely have to resort to journal lists simply in order to get the job done. And, as (the suitably named) Dean Worrell has joked, “The Dean may not know much about research but at least he or she can count” (Worrell, 2009: 127 cited by Peng and Dess, 2010: 288). Even basic numerical skill is redundant when the only competence required of a Dean is the capacity to match staff publications with the status conferred upon them by a journal rankings list.

So, for Deans who examined the declared Working Methods of the RAE Panel, or at least relied upon others to advise them, a possible remedy for their anxieties, in the form of a journal rankings list, presented itself. There remained, however, a significant difficulty. Numerous journal lists were in circulation (see [http:// harzing.com/](http://harzing.com/)). This surfeit induced a vertigo of uncertainty about which list, or combination of lists, Panel members might adopt. Facing this uncertainty, the strategic objective of business school Deans was, we conjecture, collectively to establish, endorse and promote a preferred list – in the form of the Association of Business Schools list, the ABS being, in effect, the UK trade association of business school Deans. This initiative happily coincided with the endorsement of journal ranking lists by influential bodies such as the Science and Technology Committee of the House of

Commons which, in 2004, put its considerable weight behind the use of metrics to support or even replace peer review” (House of Commons, 2004: 3 cited in Taylor, 2010: 2), and this endorsement was subsequently supported by the Treasury which in 2006 favoured a more “cost-effective” assessment process based upon quantitative data (Treasury, 2006 cited in Taylor, 2010: 2-3).

The ABS list: Construction and defence

The ABS list (*aka* “Journal Quality Guide”) gathered support during the years running up to RAE 2008 to the point that it was widely embraced as the “*de facto* standard” (Mingers, Watson and Scaparra, 2009: 3). The journals that appeared in the ABS list were derived initially from those in which articles submitted to RAE 2001 had appeared as well as other lists that had been developed by six UK business schools (Aston, Cranfield, Durham, Imperial, Kent and Warwick). The ABS list created an impact factor index based upon the mean citation impact for the previous three years for each journal. In addition, journals were added from the websites of major publishers and other sources. With this master list, consideration was given to a number of other factors such as length of publication, links with a research association, the status of the editor, and “the quality of articles in at least three recent issues by reference to research design, analytical methods, theoretical underpinnings, and significant findings” (Morris, Harvey and Kelly, 2010: 1447).

Following a provisional listing of the journals, 22 sub-field groupings were identified and experts from these areas were approached to assess the criteria and assign rankings to the journals allocated to their respective specialisms. Differences were identified in the ranking of only 30 journals and these were then resolved through “a further round of reviewing of the publication and seeking opinion from other experts” (*ibid*). These machinations may account for why, for example, the journal *Business History* which, according to the *Web of Science*, had 161 citations and an impact factor of 0.250 during the period 2001-2007, received a 4* rating in the ABS rankings list whereas *The Journal for the Theory of Social Behaviour*, which had 361 citations and a 0.615 impact factor for the same period, did not appear on the list, although submissions from both journals were made to RAE 2008 (see Rowlinson, Harvey, Kelly, Kestinova, Morris and Todeva, 2010: 10)^{viii}. Finally, the scores generated by a combination of citations and peer evaluation were placed upon a five point scale that, according to Rowlinson, Hassard and Mohun (*in press*: 157) was adopted “in anticipation of the rating system to be used in RAE 2008” (see Exhibit 3).

PLACE EXHIBIT 3 HERE

The architects of the ABS list have recently claimed that, “The Guide has enjoyed considerable success. It has been widely adopted as a policy tool in UK business schools and indeed in business schools in many parts of the world...In preparing for the RAE 2008, many university and business school managers...made use of the ABS guide in planning their submissions” (Kelly, Morris and Harvey, 2009: 2). This claim is supported by a modelling

exercise that purports to show “a high degree of congruity between the judgments reached by the RAE 2008 panel and the journal quality rankings of the ABS guide” (ibid: 3).

It is worth looking in some detail at the RAE results and the ABS list. Mingers, Watson and Scaparra (2009) conducted a study in which the overall results for each departmental submission, only available in aggregate form as the percentage of work awarded a particular grade from 4 to 1 and ungraded, were compared with the actual journals that were submitted by each department to see whether the results could be recreated through an imputed ranking of the journals. The main results of interest here were:

- A much greater concentration of outputs as journal articles and correspondingly less as books or reports than in previous years – from 69% in 1996 to 92% in 2007. Whilst this cannot be attributed to the ABS list in particular, it does perhaps indicate increasing risk-avoidance behaviour of business school managers – “at least you are safe with a refereed-journal paper, especially if it’s in ABS”.
- The number of journal titles submitted has increased from 1275 in 1996 to 1639 in 2007. This presumably reflects the greater number of staff and papers submitted, but what is significant is that only 50% of these journals are actually included in the ABS list. This shows the vast range of research carried out within business and management that does not have the “ABS stamp”. We welcome this diversity but are concerned that it will decrease in the future as the (ABS) list becomes even more hegemonic.
- There was evidence that the ABS list was used in making submissions – for instance comparing the ABS journals that were submitted with those that were not, 45% of those not submitted were ABS 1*, while only 4% were ABS 4*. So clearly journals ranked as 1* or 2* will generally not be submitted even though they may in fact be good quality journals. This especially militates against newly developed journals and innovative work.
- There was also evidence of an association between the proportion of a department’s submission that was in ABS journals and the result it achieved – i.e., in general, departments with a greater concentration of ABS journals generally did better. There is not necessarily a direct causal effect here – a high quality department will tend to publish in journals which are included in the ABS list. However, some submissions were virtually 100% ABS journals, surely indicating a high degree of managerial selectivity.
- Finally, there was some evidence from the Mingers *et al* model of dispersion around the grade of a journal, i.e., papers from the same journal being awarded different grades, as the RAE Panel expected *a priori*, and claimed *a posteriori*. This is obviously one of the dangers of reliance on a list in selecting papers for submission rather than making judgements about their merit.

Reductio ad absurdum^{ix}

The creators of the ABS list contend that it “combin(es) the virtues of different approaches” and has been developed through an “iterative process” in which it has been “validated by successively broader processes of benchmarking and peer review” (Morris, Harvey and Kelly, 2009:1446). More recently, Rowlinson, Hassard and Mohun (in press: 157) have declared that “the ABS list represents a long-standing consensus in relation to the perceived quality of a journal”. These claims echo the view of Howard Thomas, ex-Dean of Warwick Business School and the then Chairman of the ABS Research Committee, who, in his Foreword to Version 2 of the ABS list, asserts that a “consensus now exists that a carefully edited Guide, with in-built mechanisms to change on an annual basis, can bring significant benefits to business schools and individual academics”. He goes on to note that this guide responds to “Deans and other university senior managers [who] need a reliable means of assessing the achievements of their academic staff” (Harvey, Morris and Kelly, 2008: 1).

It is interesting to note that while the ABS originators appear keen to point out the positive value of the list to Deans and other managers, they have a different response when criticized. The Committee of Professors in OR (COPIOR) have become very concerned at the way the list is being used in certain business schools; for example, insisting that probationers need to have a 4* paper *before* they can pass probation (who said that American tenure was severe?). When COPIOR wrote to ABS pointing out these kinds of practices, the response was that ABS only produced the list, it was not their responsibility as to how it was used! Now, what did the designers of the atomic bomb say and later come to regret?

To support the claim that the ABS list is authoritative, Morris, Harvey and Kelly (2009) note that it “records the highest mean correlation with other lists (0.72) and the highest correlation with the citation impact factor index (0.77).” This observation is hardly surprising, however, given the way in which the ABS list is constructed (see above). Morris, Harvey and Kelly also remark that none of the 300 comments on a provisional list made available from January to September 2007 received from researchers in the business and management field “sought to fundamentally question the legitimacy of the list or its potential usefulness” (ibid: 1448). Its architects’ discussion of the list is silent on the issue of its reductionism and how the compilation of one list is supposed to address what, earlier, they acknowledge to be the “conspicuous differences in values, theoretical reference points, methods, and writing styles” (ibid: 1444) found within the field of business and management.

We are tempted to ask: did the experts from the 22 sub-field groupings forget these differences? Or could it be that the selection of the experts tended to share and reflect the “values”, etc. of their selectors? Or maybe the experts were too polite, or perhaps intimidated by the Dean’s trade association, or just insufficiently engaged to take issue with the vaunted list? Whatever the reasons, there is surely an inconsistency between recognition of “conspicuous and consistent differences” and consensus on the grading provided by a single list.

It is also a claim of the ABS list's creators that "by promoting a broader consensus,..researchers will benefit collectively both *culturally and economically* (ibid: 1446, emphasis added)". But, it is difficult to imagine how researchers collectively will benefit economically as the pool of funds made available to the field of business and management depends upon the judgments of the RAE panel. Perhaps the intention of the ABS list is to influence the Panel by identifying as many journals as possible as 4* or 3*, and thereby encourage its members to use the ABS list rather than exercising their own judgments when assessing the outputs? We return to this question in our concluding comments. For the moment, we consider the claim that, "culturally" the list will enable early career researchers to "have a better understanding of the journals in their field" (ibid: 1446). The question begged concerns the meaning of "better understanding". Presumably, it means that the existence of a single, authoritative list will convey the wisdom that the place of publication is of greater importance than the substance of the scholarship; and that early career researchers in particular will embrace an aspiration to publish in the journals most highly ranked in the ABS list. That is to say, journals that are predominantly based in the USA, dominated by editorial boards trained in the USA and dedicated to scholarly traditions that can appear narrow and scientific to many European academics (see, for example, Hinings, 2010, esp. Table 3, p 668). The extent to which the editorial boards of major journals are insular and US-centric is well illustrated by a recent social network analysis of the FT Top 40 journals (Burgess and Shaw, 2010: 644) which provided "evidence of domination of this set of high status journals by male academics drawn from a limited number of disciplines and based at US universities".

In our view, a likely outcome of such a "better understanding" is the accelerated formation of an academic monoculture in which business school faculty are induced to emulate values and forms of scholarship that dominate the most highly ranked (largely US) journals. When unsuccessful in publishing in those journals, due to the very high rejection rates, lower tier journals will be flooded with their rejected articles. This scenario may be welcomed by editors (and their publishers) who aspire to emulate the contents of the most highly ranked journals. However, as Grey (2010) has forcefully argued, the prospect of such an aspiration being fulfilled is remote because, perversely, it "fails to recognize the game being played" (ibid: 689). Using a macro-political analogy, he observes how, regardless of the editorial strategy and the efforts of editors, reviewers and publishers, such emulation "is never going to be anything other than a sideshow when compared to the USA. The significance of these efforts is quite different: they act to provide legitimacy for the dominant partner through a show of support...The very act of a marginal player competing in a space in which it cannot win legitimizes the notion that the space is the only one that matters, that it is, indeed, the "centre"." It is exactly this notion is endorsed in the construction and promotion of the ABS list.

The trend can already be seen in some of the major UK-based journals, such as the *Journal of Management Studies* which, it seems, have become a haven for papers that were initially prepared for US journals or which seek to emulate standards of scholarship championed by

those journals. In this, journals in the second and third tiers of the ABS list are trying to emulate journals such as the *Academy of Management Review* wherein, for example, Kacmar and Whitfield (2000) found that only 9% of theoretical models were ever tested. This obsession with a repetitious, narrowly conceived and often irrelevant conception of “theory development” may explain why up to 20% of papers published are never actually cited at all, even by their authors (Mingers and Burrell, 2006).

Conclusion: Deal or no deal?

One tentative conclusion to be drawn from these reflections on rankings lists is that they produce more significant problems than those that ostensibly they are developed to address. The architects of the ABS list claim is that it can replace unsystematic forms of evaluation. They also commend the list as an effective substitute for the time-consuming process of careful reading and consideration of work that forms the basis for making key decisions about staff selection and resource allocation. Notably, it is claimed that the ABS journals list was used by business school managers to inform and justify their selection of submitted staff and outputs for RAE 2008. We will return shortly to the question of why it might be that the Panel declined the offer to participate in journal fetishism - that is, submission to a universal, reified authority attributed to the particular scholarly values privileged by the metric of a given list. First, though, we underscore what we have identified as some perverse and unjustifiable consequences of using journal lists. The most objectionable of these, we have suggested, is their encouragement to the cultivation of a scholarly monoculture. For use of lists to identity research quality intensifies pressure to emulate the genre of scholarship that is published in the journals most highly ranked by the favoured list. Associated with this effect is a mania, driven by heightened competition to succeed with this emulation, and performance anxieties associated with the fear of failure manifest in academic discourse becoming saturated by talk of rankings. As Adler and Harzing (2009: 7) chillingly observe, “it has become common to refer to a scholar’s worth by saying that he or she has two AMJs, three JIBSs and one ASQ without ever mentioning the content, quality or impact of the article itself”.

So, to return to the earlier question, is there evidence that the RAE panel assigned a high grade to some articles in journals that were not ranked highly on the available ABS list and gave a low grade to articles published in the more highly ranked journals; and if so why did they do so?^x With regard to the evidence, we can never know definitively since the actual grades awarded to individual papers will not be published, so we must rely on estimates and modelling exercises. Taylor (2010) used regression analysis to assess the extent to which similar *aggregate* results could have been obtained by simply using the ABS list together with two other variables measuring the size of the submission, and membership of university groupings such as the Russell group. He found that he could explain around 90% of the variation in mean score per department using these variables, which certainly suggests a high degree of correlation at least in aggregate but does not shed light on differences at particular grade levels – e.g., percentage of 4*. Mingers et al’s (2009) showed that, for those ABS

journals that were entered in the RAE, the ABS list gave the following proportions – 4* 15%, 3* 31%, 2* 37% and 1*17. The equivalent proportions estimated from the RAE results were – 4* 18%, 3* 31%, 2* 28%, 1% 22% and 0* 2%. These results do suggest that the Panel awarded more 4* (if repeated across all entries this would have been about 377 extra 4* outputs) as well as more 1* (and 0*). In other words, that the Panel was more positive or generous in its assessments than the list at the top end. Although these differences may not seem great, the weights for awarding funding were strongly skewed towards 4* (9:3:1:0) and so this would have had a significant financial effect for business and management research across the UK.

With regard to the Panel’s motivation, we offer the following conjecture. First, there is an issue of professional judgment and self-respect. Why bother to agree voluntarily to become an unpaid committee charged with conducting a time-consuming review if, in the end, the assessment of outputs produces the same outcome as consulting the “*de facto* standard” (Mingers et al, 2009: 3), in the form of the ABS list? A cynic’s answer might be that use of the ABS list recovers the opportunity cost of reading the outputs and debating their grades without sacrificing any prestige, or notoriety, derived from Panel membership. In short, the cynic’s suggestion is that the list acted as a voucher for a free lunch. However, what if, as analysis of the Panel’s decisions has indicated, the Panel declined this generous offer.

In a world where Panel members are purely self-interested and/or concerned only with pleasing university *managers*, including the business school Deans who had advocated reliance upon the ABS list in assessing research quality, then devolving their decision-making to the list presents a compelling solution. For Panel members, however, there was a wider constituency of colleagues – that is, *peers* - to be considered. We conjecture that fetishistic deference to a journal list by Panel members would not have been well received by business and management academics or, indeed, and perversely enough, by most Deans. Why not? Precisely because, as we have emphasised, so many of the top tier journals in the ABS list assume and privilege a conception of scholarship that most UK business school academics have not, as yet, been trained or induced to follow, and so are ill-prepared and/or disinclined to embrace. The top-ranked journals on the current ABS list – led by the 22 journals that are, in the latest version of the list are categorized as “world elite” (Harvey, Kelly, Morris and Rowlinson, 2010: 8) – are restricted by, or aspire to, standards of scholarship that are remote and alien to all but a tiny number of UK business school academics. As we have noted, use of the list available to the RAE Panel members would have resulted in a significantly smaller proportion of UK business and management scholarship (and attendant levels of funding) being graded as “world-leading in terms of originality, significance and rigour” (4*). To return to the earlier football metaphor, for the Panel to have relied upon the ABS list would have been the equivalent of scoring multiple own goals not only in terms of the prestige of management and business research but, and perhaps more significantly, for its future funding.

It can be confidently stated that clocking up own goals was not exactly what the Deans of business schools had in mind when, with Howard Thomas's ringing endorsement, they put their collective weight behind the ABS list. Apparently, their strategic management skills did not, and do not, equip them to think far beyond establishing a "*de facto* standard" that would mitigate their performance anxiety and provide a justificatory fig-leaf for their selection of staff and outputs for the RAE. It is ironic then, that it was only the recent appearance of Version 4 of the ABS journals list, with its Super League of 22 "world elite" journals, that has provoked some overdue reflection on an unintended consequence of its development and use. Perhaps minds will become more concentrated, and critically reflexive, on how subscription to a particular, narrow and culturally alien conception of scholarship effectively restricts research funding. If the 22 Super League journals become the benchmark for scholarship, then resources will be funnelled to a handful of institutions which employ members of a tiny number of UK business school researchers possessing the capability and inclination to produce articles that stand any chance of publication ABS "world elite" list.

We have also reflected on the wider system of evaluation that leads Deans to champion the construction and use of a journals list. Prior to the first RAE (in 1992), academics were expected to publish, and if their work appeared in a well established journal so much the better. But beyond that, they were free to pursue research that interested them and to publish where they expected to find a receptive audience. Much of that research was innovative, pushed the boundaries of the disciplines at the time, and shaped the future intellectual landscape. Perhaps we are viewing this period in a nostalgic manner. We are certainly not claiming that intellectual innovation has ceased but we see even less of it appearing in the top ranked journals. But we do contend that the RAE has changed the research culture of UK business schools, and not entirely for the better. Positively, it has made diverse kinds of work respectable. The playing field for research-active staff is now more level as it is not so easy to dismiss work on ideological grounds simply because it is self-defeating to exclude it from an RAE submission when it has a good chance of being well regarded by the Panel. In this respect, an initial effect of the RAE, during an era when journals lists had not been constructed, was to expand the space for innovative forms of research and so somewhat diversify the dominant managerialist culture of business school scholarship. Less positively, the RAE has become the predominant driver of strategy and tactics in universities, and hence the biggest single influence on vice-chancellors, departmental heads and thence the research activities and stratagems of individual academics. The reputations and careers of Dean's jobs depend on it, and so ultimately do those of young academics who are not in a strong position to question or challenge the demands and associated threats placed upon them. So today, the kind of advice, or instruction, increasingly given increasingly by Deans to all staff in UK business schools, and not just probationers, is to aim their papers at journals ranked as 4* or 3* in the ABS list or risk "staying on probation" / "not being submitted in the next Exercise" / "being moved to a teaching only contract", etc.

We would like to see the next RAE – that is, the Research Excellence Framework (REF) explicitly exclude any use of journal lists across all subjects, and for the business and

management Panel positively to encourage a much broader range of research styles, particularly emphasizing the importance of research that addresses the problems the world faces (otherwise known as impact!), and not just the managerially defined problems identified by executives. We would also like to see the business and management Panel explicitly encourage the submission of research monographs which, currently, may be viewed as too risky to prepare and submit. Finally, we would like to see the Panel recommend the abolition of journal lists from the field as, in our view, such lists are on balance detrimental to scholarship. If the latter recommendation is regarded as too radical or unrealistic, then we suggest that numerous lists are devised to address particular topics, fields and approaches. This, we acknowledge, would affirm rather than eliminate the “one-size fits all” principle which informs the construction of journal list and is reinforced by their use. But it would surely help to undermine the perverse authority attributed to any single list (see also Adler and Harzing, 2009: 90-1). If there were, say, twenty or fifty lists, each covering all the journals relevant to a particular topic, field or theoretical orientation, it would effectively subvert and disrupt the worst excesses of list fetishism and so refocus academic discourse on the content of scholarship rather than its place of publication.

EXHIBITS

Exhibit 1

4*	Quality that is world-leading in terms of originality, significance and rigour
3*	Quality that is internationally excellent in terms of originality, significance and rigour but which nonetheless falls short of the highest standards of excellence
2*	Quality that is recognised internationally in terms of originality, significance and rigour
1*	Quality that is recognised nationally in terms of originality, significance and rigour
Unclassified	Quality that falls below the standard of nationally recognised work. Or work which does not meet the published definition of research for the purposes of this assessment.

Exhibit 2

“The assessment will be one of expert review based on professional judgement. Each sub-panel member will be expected to form a view on all submissions. Sub-panel members with relevant specialist knowledge will assess cited work from all submissions... They will focus detailed examination on work which has not undergone peer review, or is published in new and less familiar media, or which is deemed to be potentially of the very highest standards. In conjunction with specialist advisers and members of other sub-panels to which work is cross-referred, the sub-panel expects to collectively examine in detail at least 25% of the submitted outputs.”

extracted from paras 47 and 49 of <http://www.rae.ac.uk/pubs/2006/01/docs/i36.pdf>

Exhibit 3

Quality rating and meaning

4 * A top journal in its field Publishes the most original and best executed research papers. Journals typically have high submission and low acceptance rates. Papers are heavily refereed and the journals have high citation impact factors in their sub-field.

3 * A highly regarded journal in its field Publishes original and well-executed research papers. These journals typically have good submission rates and are very selective in what they publish. Papers are heavily refereed and the journals have fair to good citation impact factors.

2 * A well regarded journal in its field Publishes original research of an acceptable standard. Papers are fully refereed and the journals have modest citation impact factors or do not carry one at all.

1 * A recognised journal in its field Publishes research of a modest standard or has yet to establish a reputation by virtue of being launched recently. Few journals in this category have an impact factor.

0 * A journal not recognised as an authentic research publication Journals aimed at practitioner audiences which attract academic contributors and which do not generally rely on peer review.

(Morris, Harvey and Kelly, 2009: 1448)

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ⁱ Despite the emergence of many new journals, their heterogeneity is under threat as the adoption of journals by libraries becomes increasingly depend upon citation counts and journal rankings.

ⁱⁱ In a recent (October 2010) personal communication to the Associate Editors of the journal *Organization*, its Co-Editors-in-Chief recently noted how journal editors `have adopted policies which artificially inflate citations. At the mellow end, this is no more than the injunction that if you want to engage in a journal’s “conversation” you need to be aware of it, so referencing the journal you are publishing in may simply be a sign of engagement. However, there is much evidence that high impact factors are being produced by editors insisting on journal citations as a condition of the review, perhaps even suggesting papers that could be cited. Or, editors publish short editorials with references to the journal in every issue, or introductions to sub-sections of the journal. Review essays are also very good for generating citations, and there is now a common practice of publishing work that looks like it might be cited in early issues of the volume, in order that it has plenty of time to have “impact” by the time Thompson do the census.”

ⁱⁱⁱ The allusion here is to Taylor’s advocacy of `one best way’ that would win consensus for eliminating diversity and conflict from the workplace. See Kanigel, 1995.

^{iv} The association here between fetishism and shoes is not coincidental.

^v As the distinctiveness of Universities resides primarily in their engagement in research activity, ranking in the RAE list assumes preeminent importance amongst a host of other ratings (e.g. student satisfaction, teaching quality, employability, etc). The next exercise, planned for 2014, is called the Research Excellence Framework (REF).

^{vi} In the UK, the few exceptions comprise an elite of MBA-centric schools (e.g. London, Cambridge, Oxford) for which the *Financial Times* and *Business Week* rankings, rather than the results of the RAE, can assume greater importance for their global standing and appeal.

^{vii} The application of this working method was reiterated in the report that followed the 2008 exercise. It is stated that the panel `assessed virtually all the submitted work by examining it, and did not use its place of publication as an evaluative criterion”. The report then makes the observation that `It is worthy of note that there was not a perfect correlation between the quality of a piece of work and its place of publication. Although much top-quality work was indeed published in what are generally regarded as leading journals, top quality work could also be found in journals occupying a lower position in conventional rankings. Similarly, some of the work considered that had been published in so-called leading journals was thought to be of less than top quality (RAE, 2009: 1-2).

^{viii} Articles published in these journals formed part of Alistair Mutch’s publications for the period 2001-2008 during which time he published 11 journal articles. The example illustrates how , if the Panel had relied upon the ABS list, it could have had a significant impact on the submission from Nottingham Trent University if, for example, Mutch’s four outputs had

included the *Journal for the Theory of Social Behaviour* article and excluded the *Business History* article, or vice-versa (see Rowlinson, Harvey, Kelly, Kestinova, Morris and Todeva, 2010 for details)..

^{ix} An argument based upon the view that a thesis must be accepted because its rejection is untenable. In short, an argument that denies the existence of options, or competing priorities, by asserting that “there is no alternative”.

^x In a detailed analysis of ABS rankings and gradings in relation to the outcome of the 2008 RAE Mingers, Watson and Scaprara (2009) conclude that “there is evidence of extensive selectivity in submissions” guided by the pecking order of journals on the ABS list. And they suggest that the presence and position of a journal on the ABS list was a “possible bias” with regard to the judgments of Panel members. Their analysis also indicates “differences [between the ABS ranking and RAE grades] for particular journals with some being two or even three grades apart” (ibid: 25) and also considerable dispersion of grading amongst papers published in the same journal (e.g. *Industrial Relations* – 4* 53.6; 3* 0.0; 2* 8.8; 1* 36.7 O*0.0) (ibid: 20-22).