

**<sup>1</sup>Measuring research output in New Zealand: An ethical perspective**

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**Abstract**

This essay critically looks at the explicit and implicit claims that have motivated the selection of peer-reviewed publications as the most important indicator of research performance in government initiated research assessment exercises such as the Performance-Based Research Fund (PBRF) in New Zealand. It is argued that the PBRF involves issues of justice, which seriously undermine its ethical and moral status. Furthermore, the government initiated research assessment exercises have the potential to inflict serious damage to the creation and dissemination of knowledge, the most valued academic activities. These have gained the status of *valued activities* due to an underlying assumption concerning their motives - that is, academics engage in such activities on the basis of broadly construed intellectual and social merits of such activities and *not* on the basis of narrowly selected criteria to gain short-term benefits to themselves and their institutions. Specifically, research assessment exercises that are linked to public funding of universities can interfere with maintaining the authenticity of *truth claims* in accounting and management in two ways. Firstly, the narrowing of the scope of knowledge creation exercise by luring academics to focus on what gets published, and secondly, the insulation of emerging truth claims from wider scrutiny by discouraging critical work because they are not favourably received by the reputed academic journals. As a consequence, instead of promoting academic freedom and subjecting knowledge claims to wider scrutiny, the PBRF encourages academics and their institutions to do the converse – to pursue research agendas set by the editors of a handful of reputed journals. In addition, there is a major concern for small countries as domestic issues may not receive adequate research attention for they are of little interest to international audiences.

*Key words:* accountability; accounting and management; knowledge creation; PBRF; research performance; truth claims.

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## **Introduction**

The performance Based Research Fund (PBRF) in New Zealand aims to recognise, promote and reward research excellence in the tertiary education sector (Tertiary Education Commission or TEC 2004). 'The primary [goal of the PBRF] is to ensure that excellent research in the tertiary education sector is encouraged and rewarded. This entails assessing the research performance of TEOs [tertiary education organisations] and funding them on the basis of their performance' (TEC 2004, p.3). The research performance of a TEO is obtained by summing up three research-related component scores: (1) a score for academics' research performance (based on evidence portfolios assessed by a panel of experts), (2) a score for research degree completions, and (3) a score for external research income (TEC 2004). The performance scores thus obtained are used to allocate a portion of public funds available for all tertiary institutions in New Zealand <sup>1</sup>. Although there are claims to recognise all types of research output when evaluating researchers' evidence portfolios, the reality is that the peer-reviewed journal articles in general, and publications in highly reputed journals in particular, are perceived to receive more weight than other research output (Adams 2008, p.28). The process of allocating public funds entirely based on student enrolment, the previous model used in New Zealand, is assumed to be defective in that it fails to emphasise the importance of research, thus failing to hold academics and tertiary institutions accountable for this important component of their work. An independent review of PBRF conducted in 2008 states that as a result of the measurement exercise: 'Research resources are directed more selectively to institutions judged by the PBRF process to have delivered better research.... The PBRF has shifted research funding towards universities and away from institutes of technology and polytechnics (ITPs), wānanga (publicly owned Maori tertiary education organisations) and private training establishments (PTEs), and shifted the balance among universities' (Adams 2008, p. 6). Accordingly, the PBRF provides significant incentives for universities to maintain and improve research excellence.

This paper critically examines the implications of the PBRF as they relate to the creation and dissemination of accounting and management knowledge in New Zealand universities. It is argued that, on the one hand, the PBRF involves issues of justice, which seriously undermine its ethical and moral status. On the other hand, the PBRF can interfere with the authenticity of *truth claims*; first, it restricts the scope of knowledge creation exercise by luring academics to focus on what gets published in reputed journals, and second, it has the potential to insulate emerging truth claims from wider scrutiny in so far as they do not receive a favourable reception from journals.

### **Researching, Teaching or Learning?**

When construed broadly, although teaching and research may be distinguishable from one another, arguably the two activities are not separable; that is, teaching and research are complementary activities. Even the very act of disseminating research findings constitutes a form of teaching. Equally, teaching is an activity that enters one into a dialogue with others; for example, be they ideas, inventions or discoveries they need to be tested in public forums to allow adequate scrutiny and thus teaching involves elements of research. Teachers attest to moments that led them to re-discover issues or sparked an original thought that guided them to a new discovery whilst engaged in teaching, preparing for a lesson. For example, the idea of a new theory of how writing came about – the theory of tokens that replaced the pictographic theory – first occurred to Denise Schmandt-Besserat when she was preparing material for a lecture (Schmandt-Besserat, 1996). Therefore, if one separates teaching from research it leads to construing teaching without recognising its dependency on learning; a position that is difficult to maintain. The separation of teaching from research is also problematic as both activities are important not because of some value intrinsic in them, but because they are means to a much valued public good. Such a valued public good can be described in various terms – for example, knowledge, scholarship or learning. Yet the meanings of the three terms are not necessarily interchangeable. One meaning of both knowledge and scholarship is learning. In

this paper the term *learning* is used as it ably captures the activity involved, and it is comparatively less tainted with the connotative meanings attributable to knowledge and scholarship. For example, the term *scholarship* may signal that it is the domain of a selected group, while the term knowledge can create problems as it is closely linked with wisdom. To assess performance of an academic fairly, one must therefore look for indicators that reveal his or her sustained commitment to learning.

### **Learning-related activities**

The activity of learning, and an endeavour to sustain it, involves engaging in a variety of activities. Recognition of activities entailed in learning is therefore necessary if we are to make academics accountable in an intelligent way. Although not necessarily exhaustive, learning-related activities of an academic can be listed as follows (not based on their importance):

- Reading
- Reflecting
- Thinking
- Writing
- Disseminating knowledge or publishing
- Teaching (preparing lessons, planning, organising, examining)
- Curriculum design and development
- Student and staff mentoring
- Researching
- Designing methods to collect data
- Collecting data
- Developing/identifying tools for analysing data
- Analysing data
- Participating in conversations (verbal and written)
- Debating (verbal and written)
- Examining others' work (dissertations and theses)
- Securing external funds to conduct research

Reviewing and commenting on others' work

Organising and participating in forums (conferences, workshops and colloquiums)

Workplace administration

These activities must receive some recognition, even though all activities need not be treated with equal importance, when determining how well academics perform their role, either taken narrowly to include their role in a specific institution, or taken broadly, to include their role in society. Obviously some of these activities are less observable than others, thus posing a risk of less observable activities getting eschewed in any assessment exercise. This is an important concern. The fact that a particular learning-related activity is difficult to observe (that is, reading) does not mean either that it is less important, or that it must receive little recognition in a process designed to assess academics' performance. Instead, problems of visibility demand that greater care is exercised when determining indicators of performance. A failure to do so may involve far-reaching consequences, some of which are unintended but nevertheless highly predictable. The predictable consequences of selecting a handful of indicators thus cannot be ignored. Often-heard pleas, such as 'consequences are unintended', are inadequate to defend highly predictable behavioural consequences when they begin to unfold (for example, gaming the system). A failure to adequately consider predictable consequences also leads to a well-known peril that can seriously undermine accountability; that is, leaving room for excuses. Academics when put under pressure to defend their lack of performance can claim innocence citing the defects in the process, or showing that their attempts to sustain learning commitments do not receive due recognition. Notwithstanding these issues, recent measurement exercises such as the PBRF place greater emphasis on a handful of comparatively more visible learning-related activities, the most important indicator of research performance being the evidence of peer-reviewed publications. Publications also vary in terms of their status depending on the type of outlet (for example, A\*, A, B and C

journals as agreed by the Australian Business Deans Council of ABDC, 2002) and the mode of publication (for example, journal articles, books, book chapters and conference papers).

### **Indicators of Research Performance**

The selection of an indicator of performance is inevitably influenced by its quality and reliability, parsimony, measurability, comparability and practicality. There is certainly merit in selecting some as opposed to all indicators of academic performance. To begin with, the issue of quality is important. For example, all writing is unlikely to be of equal quality. One of the most widely used methods of quality assurance, the double-blind peer-reviewed process (both reviewers and authors are unaware of each other), perhaps adds the strongest support for using peer-reviewed journal articles as a high-quality indicator of research performance. The issue of quality however need not be confounded with that of visibility. For example, as an alternative to peer-reviewed external publications we can consider peer-reviewed in-house publications. The issue of quality is present whether we use writing (available in-house) or external publications to assess research performance. As the peer-review process could be used for writing that remains in-house (see Mathews 2007), as well as for external publications, any criticism against quality has to come not from the availability of peer-review process per se, but from a difference in its operational effectiveness when used in in-house vis-à-vis external publications. This deserves consideration, as the quality of the peer-review process need not be substantially different whether it is used by an in-house or by an external publishing outlet. Some may argue that there is a lack of conflict of interest by the decision-makers involved in the external publishing outlets than those involved in in-house publications. As a result external publishers may have incentives to remain impartial gatekeepers of quality in academic writing. However, there is an extensive body of literature that casts doubt about both the quality and the impartiality of the peer-review process used by well-known journals. That issue will be discussed later.

Next, there is the parsimony argument; some key learning-related activities deserve special focus as they meaningfully embrace a variety of learning-related activities. Then, looking at more indicators (as opposed to a few) is not an intelligent way to deal with issues concerning performance. Output-based signals, for example, find strength from this type of reasoning. Accordingly, a specific amount of time spent by an academic in classroom teaching can be defended as an adequate signal of that academic's commitment to learning, as it embraces many learning-related activities (for example, planning and preparing lessons sourced from reading and research, promoting dialogue, motivating and educating students) that are themselves difficult to observe. Obtaining students' evaluation of teaching receives even better support, as it may embrace most of the preparatory activities on the one hand, and complements an academic's efforts in the classroom with a feedback from the recipient of such efforts, on the other. Similarly, the periodic research records of academics (either individually or as a group) may provide an output-based signal of their commitment to learning. Another reason for focusing on a selected number of performance indicators, mostly output-based measures, is the measurability of these selected indicators. From policymakers' perspective there is little use in agreeing on indicators that involve considerable controversy in measuring (for example, one's ability to think). Disagreements related to measuring a particular indicator can initiate an endless debate threatening the achievement of any anticipated outcomes. Difficulty of measuring is also problematic from a motivational point of view, as things that are extremely difficult to measure may fail to provide adequate motivation. The progress one makes in writing a thesis, for instance, is difficult to measure reliably during the research and writing process, but adding some measures such as a completed draft of a chapter may be very helpful to motivate the candidate. Seeing a completed chapter and getting some comments (including compliments) may be essential not to lose heart when engaged in tasks that span several years. Related to the issue of measurability are two other reasons that add strong justifications for selecting only a few indicators. These

are the comparability and the practicality of administering the selected indicators. One of the reasons for measuring anything is motivated by a need to compare that measured item across people, time, institutions, and so on. Indicators of academic performance are no exception. They are used to comparing the performance of an individual or groups of academics, or as is happening under the PBRF system, to compare academic institutions. Therefore, selecting measures that are comparable across individuals, divisions and institutions makes some sense.

Placing a heavy emphasis on comparability however is fraught with considerable risks, which can undermine the anticipated value of the selected indicators. In order to make comparisons one may be forced to search for journal ranking (for example, ABDC 2002 journal ratings list) and citation counts that are related to academic research. These signals may be driven not merely by scholarship-related factors, but also by political and egoistic factors. Nonetheless, the meeting of the twin requirements, measurability and comparability, becomes critical to make administering the academic performance matters a possibility. Indeed, it may not be too much to say that the selection of a measure first and foremost has to pass the administrative feasibility test before they get tested against any other criteria. When placed in the hands of administrators any agreed criteria tends to find a hierarchy of importance, thus leading to communicating both intended and unintended messages to those involved in the process. To illustrate, evidence of research output may be what is intended, but different types of output exists and they need categorising, especially if the research measurement process involves high stakes. A good example of this categorisation is the ranking of peer-reviewed journal articles higher than authored books, and then ordering of peer-reviewed journals into categories such as A-journals, B-journals and so on (see Gray et al. 2002; Locke and Lowe 2002; Milne 2002; Milne et al. 1999). As opposed to remaining true to substantial issues, academics and their institutions may tend to move their focus to citation counts and ranking of journals to distinguish their work from those of others.



Accordingly, a significant part of energy is exerted in defending the quality of academic work by referring to some widely used yet strongly contested criteria of quality (such as citation counts, impact factors and so on). The Research Assessment Exercise (RAE) in the United Kingdom (UK), for example, attracted criticism as to whether the exercise encouraged high-quality research or continued to protect existing research (Humphrey et al. 1995). Arguably, this is a waste of energy as it concerns *showing* as opposed to *doing* one's job. The enormous amount of time spent by academics and their institutions on preparing evidence portfolios required by the PBRF to show what academics have achieved is a case in point. (Incidentally this raises an ethical issue as to whether or not decision makers read the actual work of those academics they judge as research active or inactive.)

The quality and reliability, parsimony, measurability, comparability and practicality arguments, however, must be balanced with concerns of much greater importance; namely, (1) the issues of justice and (2) the issues of scholarship. Before moving on to a discussion on these two issues, a note is in order to clarify the position of this paper on (1) the freedom to research and write, and (2) the importance of researching and writing. It must be emphasised that the criticisms presented do not in any way intend to imply that academics have little freedom either to do research or to write. For example, many university academics in New Zealand now work under the semester system, where each of the two main semesters involves 13 weeks. Although some universities have three or four semesters a year, many universities require academics to teach during the two main semesters only. On this basis, teaching-related work accounts for 26 weeks. If we allow three weeks per semester to allow for planning, organizing, examining, marking and course advising, then the total number of weeks devoted to teaching is 32 [(13 weeks + 3 weeks) X 2] – leaving another five weeks for annual leave and unexpected absences from work – this still leaves 15 weeks of non-teaching time to account for (52 weeks less 32 weeks less five weeks). That is a reasonable amount of time to engage in

learning-related activities, and hence, it is assumed that academics have time to research and write. No doubt this time is limited, and it can present challenges when coupled with large student numbers we see in business schools. The availability of 15 weeks, however, provides some justification that there is freedom to research and write or to sustain one's commitment to learning.

Furthermore, challenges raised against PBRF must not be construed as attempts to undervalue research as a learning-related activity. From the view point of sustained commitment to learning, conducting research and writing are vital activities that all academics must engage in. In this respect, the following comments on research by two well-known accounting academics are strongly endorsed:

Research has an exercise dimension – it surely stresses our cognitive capacities; it forces us to be rigorous in our thinking, and to apply the principle of science and logic. We seek to instil these intellectual processes in our students. (Demski and Zimmerman 2000, p. 346).

This brings us to the main contention of this paper; that is, while research is vital, evidence of publications as they are looked at presently to ascertain research productivity encompass issues of justice, and this can also interfere with the authenticity of the knowledge creation process. These two issues are discussed next.

### **Issues of justice**

To illustrate, consider the learning-related activities of researching and writing along with a commonly used indicator – evidence of publications in journals – to assess the performance of these activities. Several assumptions are made when selecting publications in journals as an indicator of research performance:

1. Academics have freedom to publish their work
2. Different publication outlets can be neatly ranked so that different ranks reveal differences in quality of published work

3. All academics have equal opportunities to get their work into any outlet
4. The form of publication (a book chapter, a book, a conference paper, a journal article, etc.) is itself a quality indicator

Focusing on the first assumption, to be responsible for something one must have the freedom to do or to not do that thing. Freedom must precede responsibility, otherwise some injustice is inevitable. For simplicity let us ignore any issues concerning funding <sup>2</sup>. Yet, the fact that academics have freedom to write does not mean that they also have freedom to publish or publish in a specific outlet. Indeed, it can be argued that academics have little freedom when it comes to turning their work into refereed journal publications (Moizer 2009; Mathews 2007; see Gray et al. 2002 on different forms of disseminating scholarship). First, there are a limited number of outlets and this limited number gets further decreased depending on the type of research, academics' geographical location, the type of data used, academics' prior affiliations, currency and topicality of issues researched, and so on. According to Jones and Roberts (2005), of all the articles published in the US and the UK accounting and finance journals between 1996 and 2000, 79% and 95% of the articles in the top journals in the two countries respectively come from academics working in five countries – Australia, Canada, Hong Kong, the UK, and the US. The US top journals during the same period have published only 13% of work from foreign authors. Second, freedom to publish is further restricted by the pressure to publish in so-called high-quality journals, as administrators do not reward all publications equally (see Gray et al. 2002; Parker et al. 1998) <sup>3</sup>. The top US accounting journals such as *The Accounting Review* and *Journal of Accounting Research* publish mainly studies that use US data (both ranked as A\* in the ABDC's list of accounting and finance journals). For example, of all publications between 1996 and 2000 in these two journals, 96.9% and 91.4% of published articles respectively used US data (Jones and Roberts 2005, p.1129). Moizer (2009) shows that there is a problem in accounting journals, for the acceptance rate of some have decreased to as low as 10 percent, while time from submission to publication has increased markedly.

Third, although many reviewers and editors work hard to improve manuscripts submitted to their journals, the reasons for rejection by a journal may include reviewers' and editors' bias towards a particular paradigm, method, or perspective or other reasons (Campanario 1998a, 1998b) <sup>4</sup>. Based on an extensive review of literature, Campanario (1998b) shows four likely, yet unworthy, reasons for getting one's work rejected: reviewer bias, reviewer negligence, favouritism and self-interest. Additionally, there are major difficulties in publishing articles that replicate previous studies (Campanario 1998a). According to a review of 4270 published articles in business-related disciplines (published during 1970 to 1991 in 18 leading journals), replications and extensions accounted for only 266 articles or 6.2% (Hubbard and Vetter 1996). This paints a bleak picture of academics' ability to publish certain types of work. The articles that get published in journals may have the backing of the peer-review process to ensure that what gets published is quality tested. However, the upshot of journal publications – the presence of a peer-review process – appears to involve a lot more problems than many academics want to admit. After becoming the editor of *Administrative Science Quarterly*, William Starbuck observed that reviews were highly inconsistent (Starbuck 2003). He found that the chance of getting two reviewers to accept a paper was only 6%, while the chance of getting two rejects was 25% (Starbuck 2003, p. 346). In a recent study, Adler and Liyanarachchi (2011) surveyed the authors who had a paper rejected by *The Accounting Review* in 2004 or 2005. Here are some participants' comments (who remained anonymous):

[Shows] a noticeable reluctance to publish anything that does not follow the excessively mathematical style.

The paper was rejected by the editor because the topic was deemed to be 'not accounting-related'. I think that accounting journal editors tend to take too narrow a view of what constitutes 'accounting'. As a result, papers with a public policy focus (even with an accounting theme) are rejected and the authors are referred to economics journals.

The most common reason for rejection is the claim that the topic is NOT accounting. I suspect what is 'accounting' is in the eye of the beholder.

These comments resonate with the concerns expressed by Anthony Hopwood, the editor of the *Accounting, Organizations and Society*, that serious challenges to new ideas are blocked by reviewers and editors of *The Accounting Review* who seem to know what accounting is (Hopwood 2007).

These issues are not confined to social sciences (McCutchen 1991; see Starbuck2003, p. 347 for a review of these problems in medical sciences). In hard sciences too the peer-review process seems to be posing serious problems. Charles W. McCutchen, who earned a PhD in nuclear physics from the University of Cambridge in 1957, and who has experienced peer-reviews both as a reviewer and an author, shows that the process is less than objective:

Unfortunately, the power of referees, usually anonymous, permits self-interest, jealousy, revenge, and other unworthy motives to influence decisions. Dozens, probably hundreds, of letters to the editor over the years show that nastiness in reviewing contributes to a general unpleasantness in the publication process and in science as a whole. (McCutchen 1991, p.33).

He shows evidence that some of the most influential work had, at least, received one rejection in the publication process. According to McCutchen (1991) the main problem in the peer-review process is its lack of transparency.

Peer review resists investigation. Only insiders know the details of each decision. They may not tell the truth, and the technical background needed to extract the facts is hard for outsiders to learn. (McCutchen 1991, p.30).

He questions the value of time spent in publishing: 'The time and energy spent fighting to be published are lost forever' (McCutchen 1991, p.33). This may be a little extreme, as in many cases reviewers and editors tend to assist authors to improve the quality of their work by providing constructive comments and suggestions. But authors need freedom to disagree and even withdraw their work if serious disagreements occur in the peer-review process without experiencing

substantial disadvantage. Forcing academics to publish in certain journals surely restricts that freedom.

And finally, although some papers get through the review-process successfully, they can still get rejected. Consider for example the following comment by an academic who has served on 23 editorial review boards and who was the editor of a journal for 17 years:

...despite being rated 5 out of 5 by all three reviewers, the section editor rejected the piece because it differed from his own paper. [Anonymous participant in the survey conducted by Adler and Liyanarachchi (2011).]

At least in some cases, conducting research, writing up papers and responding to reviewers' comments still may not result in publications. Therefore, if this position is accepted; that is, there is little freedom when it comes to publishing – then some injustice occurs when publications are used as an important (often the most important) indicator to gauge academics' involvement in learning.

The likelihood of some injustice occurring becomes even greater when we move beyond publishing into publishing in the right journals. For publications to receive credit they must be in highly reputed journals. As Jones and Roberts (2005, p.1107) observe:

The publication of articles in highly-rated journals is crucial for accounting and finance academics, especially in the Anglo-Saxon world. In countries, such as the UK and the US, promotion, peer esteem and academic reputation all hinge upon publication in a few highly rated, peer-reviewed journals.

Judging by the ratings given in the previous PBRF rounds, the New Zealand experience is not very different. The ranking of journals is based on the argument that journals vary in terms of quality. However, the dimensions of quality and how these vary across different journals in management and accounting have not been widely researched. The limited data available on this issue suggests that one agreeable dimension which distinguishes journals is the academics'

perception of journal quality (see Hull and Wright 1990; Lowe and Locke 2005; Lowe and Locke 2006). Examinations of dimensions of quality related to the editorial review processes however provide results that contradict the perceived quality-based rankings of accounting and finance journals (Adler and Liyanarachchi 2011; Starbuck 2005; on the reliability of prestigious economics journals see Oswald 2007). Starbuck, who attempts to identify quality differences in journals, shows that articles published in them tend to overlap widely between low-prestige and high-prestige journals. According to him, their classification into different quality stratum is fraught with serious problems:

Highly prestigious journals publish quite a few low-value articles, low-prestige journals publish some excellent articles, and excellent manuscripts may receive successive rejections from several journals. Evaluating articles based primarily on which journals published them is more likely than not to yield incorrect assessments of articles' values. (Starbuck 2005, p.196).

Therefore it is difficult to *assume* that journals can be neatly ranked so that their ranks indicate quality differences among the journals. Practicality and expedience notwithstanding, this raises important questions of justice when such rankings are allowed to strongly influence decisions that promote some academics and demote others, all in the name of promoting knowledge creation in the academy.

The type of research that is of interest to different journals varies significantly, thus creating issues about equality of freedom to publish. Some journals are quantitative, while others are qualitative, and even critical, hence leading to claims on the merits of different types of research and research methods. The end result of this is that authors adopting a particular type of research or perspective, or method, tend to receive more favourable treatment in certain outlets than in others. This becomes a very serious problem if certain methods are favoured by journals that also happen to enjoy the status of high-quality journals, thus strengthening a self-fulfilling prophecy of the superior status of certain methods and journals. As Starbuck (2005, p.180) states: '...emphasis on

A journals is widespread, and widespread practices almost always benefit someone'. The assessment of research quality in social science is highly subjective (Macdonald and Kam 2007; Singh et al. 2007) and many criticisms that stand in the way of getting one's work published are not amenable to reason (or even to evidence in some cases). So-called highly reputed journals tend to ignore critical and innovative work (Hopwood 2007; Shapiro 2006), and thus hinder, rather than promote wider scrutiny of existing knowledge.

Judging the quality of research work using the form of their publication – journal articles, books and so on – is also problematic. According to Starbuck (2005, p.180):

The most influential writings have included books and chapters in books, and distinguished social scientists have told me they refused to submit their manuscripts to journals.

It is also not difficult to find instances where authors of journal articles have borrowed support for their ideas, arguments and sometimes even evidence from authored books. It therefore makes little sense to argue that books are in some way inferior to journal articles. This situation gets worse as we move to textbooks or contributions to textbooks, which tend to receive no recognition in the research assessment exercises such as the PBRF. If there is little value in communicating with students, which textbooks permit academics to do, then academics' ability to contribute to education is restricted, and perhaps more significantly, only a handful of authors (with the help of major publishers and with little competition from their peers) are allowed to shape the world of accounting in the minds of emerging professionals. Indeed, after reviewing a number of UK accounting books, Sikka et al. (2007) show that accounting textbooks over-emphasise the technical aspects of accounting and make little contribution to developing socially reflective professional accountants.

If academics are to remain free to perform their learning-related activities, they must be in a position to refuse to engage in power-struggles that seems to be



present in publishing in so-called prestigious refereed journals. This requires freedom to seek alternative ways of disseminating research findings and ideas (see Gray et al. 2002; Mathews 2007) so that a meaningful contribution to knowledge creation could be achieved. In some cases seeking alternative ways to disseminate knowledge may also be the most ethical thing to do (for example, where space restrictions of journals do not permit authors to maintain the breadth of their work). Treating one's work differently simply on the basis of its form of publication restricts this freedom.

### **Issues of scholarship**

Obviously there is more to disseminating knowledge than scoring points. This is particularly relevant when we look at publishing from a knowledge creation perspective. What is the link between knowledge creation and publication? First, it is expected that publication is necessary to promote wider scrutiny of research work, discoveries and new ideas. Second, publications and wider scrutiny of emerging knowledge broadens the scope of knowledge creation. An opportunity to scrutinise emerging knowledge is created when academics' work is publicly available. Therefore, arguably, neither the perceived quality of journals nor some arbitrary segregation of research into journal articles, books and book chapters are vitally important. In contrast, it is the availability of research in the public domain that becomes critical because it increases the chances of authenticating emerging knowledge. Indeed, the cooperative process of knowledge creation demands that emerging knowledge is exposed to wider criticism. Knowledge and power go hand in hand, and this gives all the reasons to promote wider scrutiny of emerging knowledge. Any attempt to create, either intentionally or unintentionally, unwanted barriers to exposing academics' work to wider scrutiny therefore must be challenged (see Sikka et al. 1995; Willmott 1995). McCutchen (1991, p.40) suggests a solution, that although some journals in public circulation can choose to reject manuscripts, specialist journals need not:

Specialist journals should never reject. If scientists are worth paying, they are worth hearing from....If no-fault publication results in a flood of garbage, it shows that scientists are creating garbage. Better we learn about that than conceal it.

Following this, is it too much to say that if academics are worth paying they are worth hearing from? The use of in-house publications, as opposed to putting pressure to publish in external journals, is more likely to reveal what academics do. This will also leave room for others to determine the quality of academic work by accessing the work and making judgments themselves.

Turning to the second issue related to scholarship, unlike in hard sciences such as physics, knowledge creation in management and accounting must be considered in the light of strong criticisms levelled against social sciences in general and business-related disciplines in particular (see von Hayek 1989; Ghoshal 2005). In his Nobel Memorial Lecture (Economics) in 1974, Friedrich August von Hayek delivered a devastating blow to the apparent scientism in economics, which is also increasingly becoming relevant to accounting today:

Unlike the position that exists in the physical sciences, in economics and other disciplines that deal with essentially complex phenomena, the aspects of the events to be accounted for about which we can get quantitative data are necessarily limited and may not include the important ones. ...there may thus well exist better 'scientific' evidence for a false theory, which will be accepted because it is more 'scientific', than for a valid explanation, which is rejected because there is no sufficient quantitative evidence for it. (von Hayek 1989, p.3).

Ghoshal (2005) challenges the wisdom of mathematical modelling in management and the inevitable loss of understanding due to our inability to grapple with modelling issues that stem from the complexity of social phenomena. Consider his remarks on corporate governance problems:

Why don't we actually acknowledge in our theories that companies survive and prosper when they simultaneously pay attention to the interests of customers, employees, shareholders, and perhaps even communities in which they operate? ... The honest answer is because such a perspective

cannot be elegantly modelled – the math does not exist. (Ghoshal 2005, p.81).

Societies and governments are influenced and moved (towards progress or otherwise) by ideas. Uncritical acceptance of ideas and theories tend to do more harm than good to people and their societies. The new ideas and their bases therefore must always receive proper scrutiny (Sikka et al. 1995; Willmott 1995). If the opportunities involved in critical examination of emerging ideas are curtailed, then our aims of learning and its ability to move society forward are similarly restricted. Ghoshal (2005) shows how certain ideologies have narrowed our capacity to imagine a world of business beyond a handful of theories that have originated largely as a result of such ideologies. For example, Ghoshal (2005, p.79) challenges the basis for the widely accepted view that profit must be maximised:

...few managers today can publicly question, that their job is to maximize shareholder value. Where did the enormous certainty that this assertion seems to carry come from?

He goes on to challenge the unquestionable superiority of maximising shareholder wealth and argues that the reason for its acceptance is a result of a mistaken view of scholarship:

If the value creation is achieved by combining the resources of both employees and shareholders, why should the value distribution favour only the latter? Why must the mainstream of our theory be premised on maximizing the returns to just one of these various contributors? The answer – the only answer that is really valid – is that this assumption helps in structuring and solving nice mathematical models. (Ghoshal 2005, p.80).

It is not necessary to bring in a theory of conspiracy, our unwillingness to adequately question a theory is all that is needed for it to flourish. These concerns call for academics to be modest about the state of affairs, of their theories, and knowledge. Indeed, Sikka et al. (1995, p.131) urge accounting academics to celebrate amateurism:

...it is necessary for those positioned as intellectuals or 'experts' to problematize the authority or 'expert knowledge', a process that involves an ironic celebration of amateurism in the sense that professionalism is not equated with, or reduced to, technical expertise.

As a way forward, academics may embrace the merits of methodological pluralism in the pursuit of knowledge. Indeed, gurus on empirical research methods have long recognised that there are flaws in any given research method, leading them to advocate that truth claims in social sciences must be considered with caution (Campbell 1988), and the need to scrutinise such truth claims using various methodological perspectives (McGrath 1982). Yet many journals, especially the top journals in accounting, tend to attract criticism due to their less than enthusiastic treatment towards research that challenges the orthodoxy (Hopwood 2007, 2008; Demski 2007). Joel Demski captures the status quo of major accounting journals as follows:

Our research has also become patterned. We are overrun with variations on pricing anomalies and cost of capital effects, just as we are overrun with multiple mutations of LENS-style model. Innovation is close to nonexistent. This, in fact, is the basis for the current angst about the 'diversity' of our major publications. Deeper, though, is the mindset and factory-like mentality that is driving this visible clustering in the journals. (Demski 2007, p.153).

Viewed from a knowledge creation perspective, these remarks challenge our ability to remain open to new ideas, critically examine existing as well as new ideas, and willingness to recognise limits of our knowledge, all of which suggest a strong tendency to respect mediocrity in the accounting academy.

### **In-house Publications – an Alternative**

The availability of an alternative to external publications (or publications in journals) – that is, in-house publication – was already alluded to in the previous sections of this paper. This was done with an aim to challenge the widely held view that publishing in journals is essential both to reveal the fact that academics are actively involved in research and such research is quality assured. Both

these requirements could be met through in-house publications. For example, the peer-review process, the main strength of external publications, can be used in in-house publications, thus providing an assurance of quality. Previously, Mathews (2007, p.234) advocated a similar approach – an online publication subsequent to external reviews by a *national panel of referees* who would recommend the successful papers as *worthy of publication in a learned journal* (for discussions of this approach see Bline 2007; Craig 2007; Hussey 2007; Smith 2007; St Pierre 2007; Stainbank 2007; Wells 2007).

The peer-review process has some limitations. As previously discussed, these may include reviewers' and editors' bias towards a particular paradigm, method or perspective, and inconsistent recommendations made by reviewers. One additional criticism relevant to the use of peer-review process for in-house publications may be its vulnerability to internal manipulation by selecting reviewers who are likely to provide favourable reviews due to institutional and scholarly loyalties. This can easily be overcome by revealing the names of reviewers. Moreover, the work that challenges ideas emerging in one business school can also be revealed by introducing criticisms as part of the in-house publications.

The use of in-house publication may facilitate a more intelligent form of accountability than is possible under the PBRF-type exercises. More significantly, it undercuts the legitimacy of excuses made by academics who do not actively engage in research activities by removing issues related to freedom to publish. For example, it betrays any sense of fairness to find fault with an academic who has researched and written, but has failed to publish in highly reputed external journals. Yet, questions about lack of research activities can be raised fairly if academics fail to submit work for in-house publications. A much wider variety of work can find a home through in-house publications than is feasible with external publication outlets that tend to place emphasis on page limits, certain methods or some arbitrary criteria of quality. If in-house publications are promoted, over time,

academics' and their institutions' research performance will be self-revealing, allowing academics to select their places of work, policymakers to reward those who want to perform beyond the call of duty, and students to select institutions they want to attend to pursue knowledge.

## **Conclusion**

This paper has taken a critical look at the assumptions underlying research assessment exercises such as the PBRF in New Zealand. It shows that there are serious problems related to accepting journal rankings of some sort as a primary indicator of research quality on the one hand, and assuming that all academics have freedom to find a suitable outlet for their research on the other. In so far as there are concerns related to freedom of publication, assessments of research productivity based on publications and the rewarding of academics who publish in the so-called high-quality journals raise issues of justice. This injustice of holding academics responsible for things they lack freedom to do, is a major drawback of government-initiated research assessment exercises. In addition, promoting publications in external outlets, and particularly forcing to publish in so-called high-quality journals, tend to hinder as opposed to promote both the creation of knowledge and the authenticity to truth claims that results from such knowledge.

Notwithstanding claims to increased quality through promoting external publications, the issues of quality are unlikely to disappear. For one thing, these have haunted journals, editors, reviewers, authors and the academic community even amid the presence of the best quality assuring processes. The quality of academic work will be tested only by the passage of time. The challenge is to make the time required to weed out poor scholarship shorter and this task fairer. The organising of academic work into some sort of reputation-based hierarchy to assess the quality of such work will only extend the shelf-life of poor ideas and theories, and make it more difficult to bring challenges. In contrast, exposing them to wider scrutiny through comparatively less prestigious modes of

publications such as in-house publications by different schools may reduce the possibility of what Friedrich von Hayek termed *the pretence of knowledge* (von Hayek 1989, p.3).

Creation and dissemination of knowledge have gained the status of *valued activities* due to an underlying assumption concerning their motives. That is, academics engage in research activities on the basis of broadly construed intellectual and social merits of such activities and *not* on the basis of narrowly selected criteria to gain short-term benefits to themselves and their institutions. As stated eloquently by Sikka et al. (1995, p.118), if accounting research is to remain a valued activity, accounting academics must continue to re-examine the motives underlying their research:

In short, intellectuals encounter the weight of convention and 'normality' as they strive to travel beyond the bounds of what is taken for granted, or seek to retrieve what has been 'swept under the rug'. ...what can one make of accounting intellectuals/academics? Are we concerned with giving visibility to what may have been marginalized in public debates? Or are we simply technicians whose labour is available only to the most powerful?

Also, consider the advice of one leading accounting academic on the reasons for actively engaging in research:

We are talking about responsibility to the academy... Our responsibility is not to prosper in this culture or to do *well*; it is to do *good*. ...some young people come to academia for the joy of learning, relatively untainted by the vocational virus. I urge those students to nurture their taste for learning, to follow their joy. That is the path of scholarship, and it is the only one with any possibility of turning us back toward the academy. Don't play the game. Redefine the game. (Demski 2007, p.156).

But how do we redefine the game? One sure way is to genuinely strive to sustain our commitment to learning, and more specifically, commitment to learning to advance knowledge for the wellbeing of society. Evidence of research and writing will follow naturally, albeit slowly, and our peers, students and society will notice the difference; the interests of the public will be better served. In contrast, by

failing to re-connect with the ideals from which academics gain respectability and much valued academic freedom, they will continue to add justifications to the need for exercises such as the PBRF. Attempts to respond to research performance measurement exercises in a game-like manner too could strengthen the need for further measures (government and/or university initiated) to interfere with academic affairs.

## Notes

- 1 According to the 2004 review of the PBRF: Of the 45 PBRF-eligible TEOs, 22 participated in the 2003 Quality Evaluation. The 22 comprised eight universities, two polytechnics, four colleges of education, one wānanga [publicly-owned Maori tertiary education organisations], and seven private training establishments' (TEC 2004, p.4). There were 41 designated subject areas and 12 peer-review panels (TEC 2004). In 2007, the estimated amount of funding allocated based on the 2006 PBRF scores was \$231 million. This included all of the former degree-top-up funding and an additional \$67 million from the government (Cinlar and Dowse 2008). The final score of an institution is obtained by summing up three component scores – the quality of scores of eligible staff (worth 60%); the number of completed postgraduate research degrees (worth 25%); external research income received by the university (worth 15%). The quality scores for eligible staff are based on the evidence portfolios that reward staff members' achievement in three areas: (1) Research Output – RO (70%); (2) Peer Esteem – PE (15%); (3) Contributions to the Research Environment – CRE (15%). As the exercise is mainly about identifying and rewarding research excellence, there is an emphasis on research quality. Some indicators of quality are publications in prestigious journals and data about impact of published work. In the field of accounting and finance there are several attempts to rank journals based on their influence or prestige. Among these the Australian Business Deans Council (ABDC)'s (2002) journal ranking list is likely to play an important part in distinguishing prestigious journals from the rest.
- 2 As stated in the New Zealand Vice-Chancellor's Committee (NZVCC) website, the overall success rate of the applications to the Marsden Fund in 2008 was 11%. '...in a media release commenting on this year's Marsden allocation, the NZVCC said fund growth must continue and while this year's result was good, it was still not good enough' (<http://www.nzvcc.ac.nz/node/306>).
- 3 It is acknowledged that as a result of the peer-review process, often the quality of a manuscript could be substantially enhanced. Many reviewers



- and editors provide constructive comments, detect errors and shortcomings, all of which contribute towards making the published work a much better article than the author(s)' initial submission. The problems highlighted in the peer-review process therefore are not meant to trivialise the good work done by many academics, and more importantly, a service performed free of charge. What is at issue is that research assessment exercises such as the PBRF have raised the stakes of publishing (especially in the reputed journals) very high, thus making the peer review process more susceptible to some of its precarious possibilities.
4. Oswald (2007) shows data that challenges the reliability of prestigious economics journals, especially when the influence of academic work published in journals are looked at over 25 years. Specifically, a very good article published in a relatively less prestigious journal can receive more citations than a poor article published in a highly prestigious journal. Therefore, Oswald (2007, p.28) points out the danger of concluding that an article published in one journal is 'more important' than one published in another.

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