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Academic Knowledge Production and Scholarship: Hostage to Peer Review

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Development, University of Fort Hare*

Introduction

This article will concentrate mainly on the issue of the relationship between scholarship and peer review. Examining this relationship is important because of the need for broadening the conceptualisation and scope of engaged knowledge production especially in ways that connect with the knowledges and experiences of the working class and rural communities of the university. These communities continue to be socio-culturally, economically and politically marginalised. Academic knowledge has to accept some responsibility for that reality. Elsewhere¹ I argue that academic conceptions of knowledge impede attempts by communities from genuinely participating in the co-construction of scientific knowledge, consequently reducing its potential and usefulness by proscribing the boundaries of knowledge production through the conventions by which academic knowledge is validated in ways that exclude the possibilities for wider 'community' engagement.



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28 August 2014

Introduction

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¹ Motala E. 22 August 2014, Public scholarship, democracy and scholarly engagement. Nelson Mandela Institute – Fort Hare University, emotala@lantic.net

There I argue that academic conventions seriously limit the possibilities for engaged knowledge which can be usefully developed by the processes of co-construction with the university's non-academic communities without violating the requirements of critical knowledge production. The prevailing academic conventions about knowledge constrain intellectual endeavour; academics constitute themselves as a self-regulating and privileged caste and produce knowledge that is often no more than 'academic'.

Implied in my examination of the process of peer review is an argument that suggests that the criteria for the validation of academic knowledge need to be examined in relation to the public purposes of intellectual endeavour. Especially important is the question of how the present process of knowledge validation in academia forecloses the possibilities for genuine engagement with the wider communities of the university – and especially those communities which have no access to the academic forms of scholarship. Peer review, as I will argue, despite all its ostensible rigors, is in fact deeply flawed in many respects and has to be reconsidered quite fundamentally if academic knowledge is to achieve genuine social meaning and serve useful social and public purposes².

In this regard I am aware of the present discussions about 'engaged scholarship' also taking place in South African universities largely in relation to what is accredited as such³. This discussion is, in part a consequence of the intellectual commitments of academics whose work 'transcends' the boundaries of conventional research and who seek to bring into reckoning intellectual work of applicative value, social critique and service orientation and the methods that inform such work. Their claims seek to validate intellectual effort beyond the prohibitive conventions of validation that prevail. They seek also to open spaces for non-academics who make claims to contributing to the body of scholarly knowledge – by implication seeking also to widen the definition of scholarship in relation to intellectual work. This preoccupation is often associated with ideas about the inseparability of intellectual work from thoughtful, critically oriented and dialogically committed social activism.

² I do not, in this sense share the interpretations of academic rigor ascribed to Michael Polanyi's 'Republic of Science' nor indeed aspects of his reflections on this issue. But this is not the place for discussion about this issue.

³ A recent Conference at Rhodes University in June 2014 on the subject of universities and social engagement is a case in point.

The implication of the debates in academia is that the measure of academic outputs (such as a fixed number of approved journal publications for a year) is inadequate to evaluate or understand the work of academics who are often engaged in a wide array of scholarly activities viewed on a continuum between consultations with policy makers to directly commissioned 'expert' consultancy advice; to dialogue and public engagement activities through intellectual debate and social critique; to the publication⁴ and dissemination of research activities related to these activities, and to the various types of writings associated with this work.

Scholarship and Peer-Review

Scholarship symbolises the activity of conceiving important ideas for creative research and the production of new ways of thinking and finding explanations; it provides new interpretations of old ideas, adding to the body of human understanding, expanding the horizons of such understanding and explaining phenomena more clearly. It includes, especially in the natural sciences – but not only in them, the modes and methods of enquiry used in the process of conscientious and careful study through observation, deductive reasoning, hypothesis formation and experimentation - what Carl Hempel called hypothetico-deductive reasoning,⁵ according to which:

*Theories are essentially constructs or models of how the world works. We work within the strictly theoretically world by deducing what consequences must follow from the model's assumptions and premises, we then test the validity of the model by comparing its predictions against the real world But then when the model fails to predict reality correctly, we alter the model accordingly or search for a better one. Science, in other words, is a feedback process: it learns from its own mistakes. Indeed it behaves in a genuinely Darwinian fashion: only successful theories survive.*⁶

⁴ The issue of journal accredited is itself contentious. It has importance because the selection of journals has effects on the formula for research funding and indeed according to the ASSAf report 'on the development of local journals, the behaviour of individuals, the financial sustainability of learned societies that produced the journals and the institutions that received the "output" subsidy' See ASSAf 2006. Preface.

⁵ Dunbar R, 1996, *The Trouble with Science*, Faber and Faber, London. In regard to the physical sciences in particular, Newton attempted to reconcile the demands of two opposing trends in the 17th century – the empirical, inductive method of Bacon and the rational, deductive method of Descartes. Newton emphasized that 'neither experiments without systematic interpretation nor deduction from first principles without experimental evidence will lead to a reliable theory'. This went beyond both Bacon and Descartes 'systematic experimentation' and 'mathematical analysis' respectively and advanced the methodology on which the natural sciences have been based since then. Capra: 1988:64

⁶ Dunbar: op cit 25

Scholarship refers more broadly to the activities of intellectuals both in and outside research institutions⁷ and speaks to the value, purposes and modes of scientific enquiry. The dissemination of ideas and publication through peer review would be an important element of that though not its *only* attribute.

Within academia there are a range of approaches and conceptions of scholarship. A brief excursion into some of the writings on this issue shows that there are competing ideas and debates about it. For instance although it may be agreed that the idea of scholarship is conventionally 'described in terms of the trio of teaching, research, and service functions' in practice it is largely about research and publication. Both Feldman and Atkinson⁸ take issue with the dominance of this interpretation of scholarship arguing for the important role for the scholarship of teaching.

The scholarship of teaching is a concept with multiple ramifications. It is at the core of the current transformation of higher education. The scholarship of teaching challenges the existing stratification system within the academy. The scholarship of teaching and learning is a much larger enterprise, a movement that can transform the nature of academia.⁹

For Boyer (1990)¹⁰ the *scholarship of teaching* was about the creation of knowledge through the process of debate and 'discourse' and was a 'continuous process' of re-examining knowledge associated with the idea of 'discovery'. The *scholarship of discovery* was the 'process of intellectual excitement' and not about the 'outcomes of knowledge', while the *scholarship of application* was about 'professional activity and service' – subject to the same rigorous criteria as teaching and research while the *scholarship of integration* was about connecting various disciplinary knowledges. All scholarly work though 'could be appraised by qualitative standards that needed to be explicitly articulated'. Even these broader characteristics, defined by Boyer were viewed critically because, we are told, they lacked any orientation to the 'socio-economic contexts and historical purposes' of universities.¹¹

⁷ For a discussion of the concept and history of 'intellectuals' see Mondrou Robert (1978) From Humanism to Science 1480-1700 Volume III Pelican Books and especially page 27 et seq. See also Motala E, Occasional Paper, 2011 **Academics, Intellectuals and Scholarship**, emotala@lantic.net

⁸ Feldman Kenneth A 1995 and Atkinson M P 2001.

⁹ Atkinson M.P. 2001. page 1

¹⁰ Referred to in Bitzer E M 2006. Page 374.

¹¹ Bitzer E M 2006. page 374

Yet academic scholarship is largely hostage to the measure of academic outputs such as a fixed number of publications in accredited journals for a year.

Publication in high status refereed journals has become a major criterion of academic success in the competitive environment of global higher education. Appearing in internationally circulated journals published in English is especially prestigious. Universities are engaged in a global arms race of publication; and academics are the shock troops of the struggle¹².

This is despite the fact that the majority of universities globally are mainly teaching universities and have a limited profile as research universities and only a few of the 18000 of the world's universities 'appear anywhere in the international rankings.'¹³ Indeed as Paulsen and Feldman argue

Everyone agrees with the contention that creation of new knowledge through research and publication is an essential dimension of scholarship. But this conventional conception of scholarship has been criticized as too narrow and restrictive..... Today an increasing number of faculty and administrators support an enlarged view of scholarship that encompasses and encourages the full range of diverse, creative talents of faculty, allows for different disciplinary perspectives and provides a framework for the development of mission statements expressing more distinctive and differential priorities.¹⁴

In fact there are similar criticisms against the privileging of research in various other writings and its continued dominance in perspectives about higher education. This dominance is now fostered even more by the very system of rankings that has become so prominent in the discourse about universities and their attributes.

Limits of research output approach

The measure of published research outputs is, for many of its critics, inadequate to evaluate or understand the work of academics who are often engaged in a wide array of scholarly activities viewed on a continuum between discussions with policy makers to directly commissioned 'expert' consultancy advice; to dialogue and public engagement activities through intellectual debate and social critique; to

¹² Altbach P.G 2014 July 18, What counts for academic productivity in Research Universities, World University News, No 329

¹³ *ibid*

¹⁴ Paulsen M.B and Feldman K.A. 1995, Toward a Reconceptualisation of Scholarship, The Journal of Higher Education, Vol. 66(6) 615-640 at: 615

the publication¹⁵ and dissemination of research activities related to these activities, and to the various types of writings associated with this work.

According to its critics peer-review has an exceedingly dominant [if not exclusivist] role in the evaluation of the scholarly research output, raising issues about how scholarship is defined in the first place and what the balance is between various forms of scholarship. Peter Vale¹⁶ talks about:

This is an age in which the academic journal occupies a status way beyond its humble nineteenth-century ambition. The discovery of the academic journal itself should be a cause of some celebration. After all, the Enlightenment ideal of replacing fear and superstition with understanding, consent and order looked forward to the rise of a form of commonwealth that was based on reason and natural law. The spread of ideas through the broadcast of research findings helped to create a global society of scholars, a process that was literally to change the world. Today, however, the scholarly journal seems valued, not as an authoritative carrier of Enlightenment ideas, in which the currency of peer review was central, but seems, rather, to have become an instrument for material accumulation, like much else in a market-centred world.

Although definitional issues are very much a part of the problematic of determining what scholarship is about, there can be little argument that peer-review has a compelling grip on the determination of what succeeds or fails in the scholarly enterprise outside the realm of master's and doctoral studies, and now, increasingly even in that regard¹⁷.

From the perspective of planners and bureaucrats who are responsible for the evaluation and funding of the work of scholars, peer review has the extraordinary attribute of solving the problem of what is regarded either as creditable for recognition *or* unworthy of support as scholarship. Very little else needs to be done to establish the value of a particular scientific output in whatever discipline other than the assent of 'peers'. Their judgement is both critical and absolute subject only to the interrogation of other 'peers'. There are persuasive reasons why this has been and

¹⁵ The issue of journal accredited is itself contentious. It has importance because the selection of journals has effects on the formula for research funding and indeed according to the ASSAf report 'on the development of local journals, the behaviour of individuals, the financial sustainability of learned societies that produced the journals and the institutions that received the "output" subsidy' See ASSAf 2006. Preface.

¹⁶ Vale P, and Prinsloo Estelle H, The New South Africa at Twenty: Critical Perspectives, UKZN Press, Durban: page 3 ,

¹⁷ The University of Pretoria for instance requires some categories of doctoral candidates to publish their work as it progresses.

continues to be so, not least, the fear of the 'political' interference in the shaping of research results, in which regard the Lysenko affair in the Soviet Union is exemplary. It speaks to the integrity of the scientific enterprise and its right to pursue its goals without fear, especially of dirigisme.

A common reason for the justification of peer review is that while an individual author or research team might not be able to identify the weaknesses in a piece of work, these might be more apparent to someone with special expertise especially in an esoteric field. It would be the best way to meet the requirement of publishers and funders regarding the 'novelty' and substantive claims of the research. The process of review in this instance strengthens the quality of the work under review.

The literature about this seems to adopt an uncritical view of the concept of 'expert' ascribing to her/him the quality of expertise attained through training, educational attainment, the application of skill, professional experience and a record of publication. These attributes required for scholarly judgement are taken as given and is itself established by the consensus of 'peers'.¹⁸ These 'peers' moreover can only be members of the academic community and outsiders have no right to 'peership'. This too raises the question whether all academics regard the concept as applying to their academic colleagues alone; or whether they recognise the possibility of other and wider interpretations?¹⁹

Peer review is also favoured for its value as 'objective', 'disinterested' and 'independent' criticism and review unfettered by cronyism and preferential treatment.²⁰ That is why the preferred practice is one that avoids close colleagues of the writer who should ordinarily be disqualified from acting in this role because of the possibilities of conflicting interest. Although anonymity of review is the preferred mode of operation this is sometimes not possible because the scarcity of specialists in particular

¹⁸ <http://en.wikipedia.org/wiki/expert> (20.0807) has a useful discussion of the 'two broad' approaches to the study of expertise, the one reliant on its 'social construction', while the other is reliant on expertise as a characteristic of individuals 'and is a consequence of the human capacity for extensive adaptation to physical and social environments.' See also the reference to the computational models developed to explain the development of novice to expert and the critique of 'expert systems literature.

¹⁹ I am indebted to Judy Favish for drawing my attention to this and several other issues.

²⁰ Yunus Ballim, Vice-Chancellor of the University of Witwatersrand, comments as follows in an undated personal note to me: 'Much of this critique has parallel with the use of the examination as a means of assessment. It is a brutish system that often does not measure what users think it does. However, society accepts the system on the belief that the RATE of change is positive – even if the QUANTUM of change may be unknown. This belief includes the sense that the system is self-regulating: mediocrity that gets through because the measurement instrument is too blunt, will wither and die on the vine.'

fields and the credentials and standing of particular reviewers make it difficult to support the pretence of anonymity in such cases.

The system of peer review is intended therefore to provide the necessary evaluative process ostensibly sifting serious scientific work from that which is not. It is the mechanism for producing credibility in the 'republic of science' - since it would otherwise be impossible to decide what knowledge is authentic and meaningful. It attests to the plausibility of explanations, the claims derived from hypothetico-deductive, inductive and other methods of inquiry, the development of theory, the propounding of physical and other 'laws' and about novelty in knowledge. Its cumulative effect is to pronounce on what should be regarded as a serious contribution to the body of scientific outputs. How else, in the academic enterprise could the development of science take place with integrity?

All this notwithstanding there is little doubt, as will be shown later, that the approach adopted by reviewers differs widely in the level of scrutiny, rigorousness and the stringency they bring to the processes of review. In fact there is much agreement that the demands of academic work make serious evaluation by reviewers difficult. The availability of time and the subjective predilections of reviewers intrude heavily on their styles of review and the characteristics they bring to it even though the outcomes of the review are intended to adhere to the requirements of clear purpose, accurate representation of data, cogency of argument, good presentation and the like.

Peer Review – a brief history

Although peer review has incomparable ascendancy and is dominant in defining the rigorous attributes of scientific output and has been greatly enhanced and systematised as *the* mode of regulation mainly over the last half-century, its provenance can be traced (in the English speaking world at least) to the work of the Royal Society founded in 1660, and especially to its scientific journal the *Philosophical Transactions of the Royal Society*.²¹ At the heart of the Royal Society's institutionalised procedures lay the "witnessing" or "attesting" to experimental outcomes, established as an absolute requirement for validating results. According to Lisa Jardine this remains a feature of the process of putting scientific experimental findings

²¹ See Bronowski and Mazlish 1963 at Chapter 10 for a fuller discussion of the evolution of the Royal Society. The Royal Society publishes seven, high quality peer-reviewed journals covering: biological and physical sciences; history and philosophy of science; and cross-disciplinary research at the interface between the physical and life sciences. Also "http://en.wikipedia.org/wiki/Royal_Society"

on the record.²² These procedures, it should be said, were developed in the context where the idea of shared knowledge was not genuinely accepted. In the 17th century, a condition for the sharing and dissemination of knowledge was that although reproducibility was a fundamental criterion, it was nonetheless taken for granted by the “virtuosi” that those enthusiasts with mere “ordinary” curiosity had in the end to be excluded from full participation.²³

Ann Weller’s useful study which examines other published studies of peer review provides a broad history of the ‘modern evolution of editorial peer review, the adoption of the practice by scientific and scholarly journals during the past century.’²⁴ Her book is ‘for anyone interested in the scholarly communication process’ since ‘almost all scholarly and research articles published in the journal literature undergo the editorial peer review process prior to publication.’²⁵ Weller also raises the important issue of the effects of the rapidly expanding electronic environment for the process of peer-review as it is presently constituted, arguing that it is likely that its effects would be to ‘transform dramatically’ the ‘scholarly publication process.’²⁶

According to her several established journals especially in the medical sciences began using the process early in the 20th century but it was only after WW II that the process became widely accepted even though this did not mean that every journal was fully peer reviewed since editorial prerogative remains a characteristic of the process. We know that not all peer reviewed journals are accredited although all accredited journals are

22 Jardine Lisa 1999. page 316

23 Jardine L 1999 page 317: The writer provides a telling example of this on the pages that follow.

24 Weller A C 2001, Page 3

25 Weller A C 2001: Preface: xii. The book covers studies in the English language for the period from 1945 to 1997 based on over 1500 citations and drawing on an annotated bibliography published in 1993. (Referring to Speck, B.W. (1993), *Publication Peer Review; an annotated bibliography* (Vol. 7). Westport, Connecticut: Greenwood Press. It also covers the categories relating to rejection rates, studies of editors, authors and reviewers, statistical review, the electronic environment and draws conclusions overall. It does not study the peer review processes used by finding institutions or of monograph publication.

26 Ibid: xii. The Journal *Behavioral and Brain Sciences* employs a not so unique practice called “Open Peer Commentary”: An article of major significance is published, a large number of fellow scholars comment on it, and then the original author responds to all of them. The approach has many virtues, one of which being that it lets you see where a community of scholars and thinkers stand with respect to a controversial or provocative scientific idea. See also on the issue of open access Jefferson et al 2002, Harnad Stevan 1998 and Riisgard Hans Ulrik 2000. Peek R and Newby G Eds, 1996. and <http://oalibrarian.blogspot.com/2008/02/oopen-peer-reviewed-open-access-books.html>

peer reviewed. For instance in an article in *Research Africa*, Pippa Smart²⁷ points to the fact that of the approximately 1 200 journals published in Sub-Saharan Africa only 27 are cited in the ISI/ISSI Index. We are told by Weller that it was only in 1948 that for the first time a list of editorial board members was published – this in the *Journal of Paediatrics*; that it took some 200 years for the process of peer review to be ‘universally accepted’; that the process has ‘never taken a standardized form’ and continues to evince variation; that ‘most, if not all, journals contain some material’ that is not subject to review, such as news items, editorials, symposia and conference proceedings, solicited manuscripts, commentaries and letters; that in the case of some journals preference is given to the material presented at the annual conference of disciplinary societies.²⁸

Other studies about peer review are also referred to by Weller. She refers to Bailar and Patterson²⁹ who suggest that studies in the peer review process have not been the subject of high level interest or funding and consequently

The work we found was often poorly conceived, methodologically weak, based on small samples, undertaken by persons without ... policy³⁰

Most studies in editorial peer review are in the fields of medicine, social sciences and psychology with the latter two concentrating on studies in reviewer bias and rejection rates in the social sciences. Weller informs us that there is widespread scepticism amongst some researchers about the proven benefits of editorial peer review. Referring to work by Fletcher and Fletcher (1997), she points to several efforts within professional associations that endeavour to understand the value of editorial peer review leading to the formulation of policies and practices for peer review. They argue the case for more studies on the subject thus:

*Few studies have put to the test, through scientific research, beliefs (hypotheses) that what peer reviewers and editors do to and for the manuscripts improve the outcome. This kind of research is needed to establish a strong basis for editorial policies ... It is possible to do hypothesis-testing research on peer review and editing practices. There are a growing number of examples, though not a well-developed body of information.*³¹

²⁷ Sourced: *Research Africa* (www.research-africa.net) on 6 October 2008

²⁸ Weller A C 2001: pages 5 et seq.

²⁹ Bailar J C. and Patterson K 1985.

³⁰ Weller A C 2001 page 11 (no page reference for original given)

³¹ Weller A C 2001 page 13 (referring to Fletcher and Fletcher page 38) She also provides a useful summary of how a ‘peer-reviewed journal’ has been defined and the distinctions made by journals in

A 'peer-reviewed journal' she suggests, *a la* Pettigrew and Nicholls, is an ambiguous term since:

*it covers a continuum of peer-controlled quality assessment that reaches its most strict definition with double-blind peer review by several scholars working in the research area, and minimal ability of the editor to override clear decisions by the peer reviewers on which articles are appropriate for a given journal.*³²

Definitional issues have also been raised about the term 'peer'.³³ There are questions about how 'peer' is understood other than by reference to someone who is the editor or co-editor.

Peer review has become a stock term, but is the reviewer of a manuscript ... always a peer: a person who has equal standing with another, as in rank, class or age.'³⁴

Weller argues that editors are hugely influential, even if idiosyncratic about the processes and procedures used by them,³⁵ and suggests that greater clarity about the outcomes and purposes are required. The strengths of the system she believes refer to the publication of valuable work and the rejection of 'non-valuable' work, the 'goodwill' that reviewers bring, the educational value of review processes, open communication and the sharing of knowledge and confidentiality prior to publication, accepting the acknowledged 'bias' of reviewers.³⁶

In South Africa, Christina Scott,³⁷ referring to the investigation conducted by six members of the Academy of Science of South Africa, in a piece titled "Publish or Perish" to "Publish and Vanish," reminds us that:

For an ambitious academic, journals offer a chance for their work to be recognised by the brightest minds in the field, and an opportunity to snare invitations to overseas conferences. Despite their tiny print runs, journals can also -- over time -- influence governments and shape economies.... Or at least, that's the way the process is supposed to work.

this regard to suggest *a la* Pettigrew and Nicholls that the term is ambiguous since 'it covers a continuum of or peer-controlled quality assessment that reaches its most strict definition

³² Weller A C 2001 page 13

³³ The idea of a peer review suggests 'an assessment by experts (peers) of material submitted for publication in scientific and technical periodicals" Weller A C 2001: page 15 See also Harnad S 1986

³⁴Weller A C 2001 page 16 (referring to DeBakey, 1990: page 347)

³⁵ Discussed at pages 17-27 in Weller 2001

³⁶ Weller A C 2001 page 308.

³⁷ See for this Peters D. P (2006 12 May), Cape Town [IPS], Digital Imaging South Africa, University of KwaZulu Natal, e-mail: petersd@ukzn.ac.za <http://disa.nu.ac.za>

Yet the report refers to a number of 'deep flaws' in the prevailing journal system in South Africa referring to its report titled 'A Strategic Approach to Research Publishing in South Africa'. The report refers to the powerful influence of financial incentives in the publication of journals 'rather than the desire to give an airing to important research findings';³⁸ the paucity of local journals that merit international recognition; that of those that are recognised few are 'worth the paper they are printed on';³⁹ that the financial incentive trumps considerations of intellectual worth or competence; whether the journal is read widely or referred to outside South Africa; and other criticisms.

Tellingly, Scott argues that:

By drawing attention to the limited readership of many South African publications, the academy's report also highlights the difficulties of making scientific inquiry benefit the broader community. If scientists aren't even communicating with researchers in other institutions through the accepted channel of academic journals, there is probably little hope that their work on malaria, tuberculosis or education is being taken up by policymakers in government⁴⁰.

Critical views about peer review

In addition to some of these difficulties surrounding peer review referred to above, there are a number of quite direct and far reaching criticisms of the value of peer review, Drummond Rennie, Deputy Editor of Journal of the American Medical Association and an organizer of the International Congress on Peer Review and Biomedical Publication, had this to say:

There seems to be no study too fragmented, no hypothesis too trivial, no literature too biased or too egotistical, no design too warped, no methodology too bungled, no presentation of results too inaccurate, too obscure, and too contradictory, no analysis too self-serving, no argument too circular, no conclusions too trifling or too unjustified, and no grammar and syntax too offensive for a paper to end up in print.⁴¹

³⁸ *ibid*

³⁹ According to says Dr Anastassios Pouris, director of the Institute for Technological Innovation at the University of Pretoria, and one of the authors of the report.

⁴⁰ *Ibid*: The Report moreover recognizes the changing environment engendered by the role of Internet and its power to radically alter the role of peer review.

⁴¹ http://en.wikipedia.org/wiki/peer_review

And none other than the Editor of the acclaimed and prestigious *Lancet*, Richard Horton, delivers this stinging rebuke:

Editors and scientists alike insist on the pivotal importance of peer review. We portray peer review to the public as a quasi-sacred process that helps to make science our most objective truth teller. But we know that the system of peer review is biased, unjust, *unaccountable, incomplete, easily fixed, often insulting, usually ignorant, occasionally foolish, and frequently wrong*. (My italics)⁴²

It seems obvious that while peer review is a widely accepted practice it is hardly uncontroversial. A number of criticisms and cautions have been raised about its usage and applicability. Gitanjali refers to critics who describe the process as ‘subjective, prejudicial, crude, offensive, secretive’ and as ‘the informed prejudices of old men’ and complains that ‘the readability of articles at publication remains poor’; that ‘editors may be acting in a high-handed fashion and arbitrarily rejecting manuscripts’; that ‘many peer reviewers find it difficult to accept new ideas and hypotheses that are totally in contrast to the prevailing ideas at the time’... And he asks, ‘how often do independent referees agree about the quality of a paper or abstract?’⁴³ The argument is that since quality is related to originality, the use of appropriate methods and analytical conclusions that speak to the data, there ought not to be such variances in the ‘reproducibility of peer review’, raising questions about the integrity of this mechanism. Similar strictures are made against the potential effects of the peer review system in ‘blocking the flow of innovation and corrupting public support for science’ – serious criticisms indeed.⁴⁴

Even more unflattering for peer review is Horrobin’s references to a decision made by a US Supreme Court, in which the acceptance and reliability of scientific evidence was in issue, provoking the Court to provide guidelines in this regard. In an important caveat it ruled that peer review was not adequate as unequivocal evidence of the validity or otherwise of a scientific finding because of the ‘alarming lack of correlation between reviewers’.⁴⁵ This was an important judgement since it ruled against a ‘sacred pillar of the scientific edifice’ and was a reprimand against the prevailing canons about the reliability of peer review. It supported critics who are viewed with suspicion and even as

⁴² *ibid*

⁴³ Gitanjali B 2001 [cited 2007 August 31]; 47:210-4 Available from <http://www.ipgmonline.com/text.asp?2001/47/3/210/189>

⁴⁴ Horrobin D F: 2007:1 See also Horrobin 1996 suppression of dissent.

⁴⁵ *Ibid*: 1

'mavericks' who are 'driven by bitterness'.⁴⁶ Complaining about the unwillingness of the 'scientific establishment' to accept the need for an evaluation of the process of peer-review Horrobin accuses them of:

... failing to understand that if a process that is as central to the scientific endeavour as peer review has no validated experimental base, and if it consistently refuses open scrutiny, it is not surprising that the public is increasingly sceptical about the agenda and the conclusions of science'.⁴⁷

While he does not argue that peer review is invalid, his main claim is that there is lack of good evidence about the usefulness or otherwise of peer review. He uses the work done by Rothwell and Martyn to support his perspective about the negative and ascendant characteristics of peer review since 'they have provided solid evidence of something truly rotten at the core of science'.⁴⁸ According to Horrobin, Rothwell and Martyn's research substantiate the prior criticisms of peer review to the effect that it was not to be relied on. And most damningly he avers that:

The core system by which the scientific community allots prestige (in terms of oral presentations at major meetings and publication in major journals) and funding is a non-validated charade whose processes generate results little better than does chance. Given the fact that most reviewers are likely to be mainstream and broadly supportive of the existing organization of the scientific enterprise, it would not be surprising if the likelihood of support for truly innovative research was considerably less than that provided by chance.⁴⁹

The relationship between peer review and ranking is not dealt with here although inevitably ranking and publication are related. There is now a wide ranging literature on this issue in relation to the question of citation.⁵⁰ Suffice it to say that even in this regard (ranking) there are questions about its reliability on the grounds that the criteria for ranking are 'not objective'; that citation rankings are highly dependent on the vagaries of the samples used producing idiosyncratic results. Hence the argument by Rothwell and Martyn that:

It follows that any career decisions based on rankings are dominated by chance and do not reflect research quality. Instead of propagating a ranking

⁴⁶ Ibid: 1

⁴⁷ Ibid: 2

⁴⁸ Horrobin D F 1990 page 2

⁴⁹ Horrobin D F 1990 page 2 His own emphasis. See also Horrobin 1981-2

⁵⁰ See, for instance, the extensive treatment of this issue in Peters M A 2006

*based on board membership as the gold standard, we suggest that committees make use of this quality indicator to find members who, in turn, evaluate the research quality of individual scholars.*⁵¹

Similarly, a number of criticisms relate to the influence of peer reviewed publishing on the funding of research and the selective dissemination of new ideas. This criticism is expressed simultaneously with concerns about the quality of work that is funded and published since, it is argued, 'there is no objective standard of quality of a scientific report or grant application against which the sensitivity or specificity of peer review can be assessed.'⁵² Even more disconcerting is their argument that it is likely that peer-review processes in academia and even in industry 'destroy' rather than promote innovation.

A number of other writers make similar criticisms about peer review – relating in particular to its negative consequences for the stimulation of scientific innovation. Berezin talks about how 'genuine innovation is suppressed while mediocrity and production of trivial data are implicitly endorsed' and how peer-review 'almost invariably favours research along well-established lines and discourages real innovation and risk-taking' which 'leads to a proliferation of conservatism and cronyism, and the overconcentration of research funds in the hands of elitarian (*sic*) and overfunded control groups' and the marginalization of 'truly exploratory research'⁵³ Hagley talks about the attempt to reconcile 'science and rationality' and uses Feyerabend (and Kuhn) to argue the proposition that 'in many instances, scientific progress occurred only because of the irrationality of individual scientists who clung to their theories despite strong evidence against them – progress in science requires competition among contradictory theories'.⁵⁴

Weller herself complains that one of the 'outstanding weakness is that errors of judgment, either unintentional or intentional, are sometimes made. Asking someone to volunteer personal time evaluating the work of another, possibly a competitor, by its very nature invites a host of potential problems, anywhere from holding a manuscript and not reviewing it, to a careless review to fraudulent behaviour.'⁵⁵ And some

⁵¹ Rothwell and Martyn 2002: page 123 (Frey B S and Rost K, 2008 October)

⁵² Rothwell and Martyn 2002: page 123

⁵³ Bezerin A A1996. page 1

⁵⁴ Hagley M T 1990:3143. The opposition of the British astrophysicist Arthur Stanley Eddington to Chandrasekhar's proposition about what came to be known as the Chandrasekhar Limit is a remarkable case in point.

⁵⁵ Weller A C 2001. page 308

relate to the lack of guidelines and standards, the idiosyncratic nature of review with no accepted standard and definition of peer review.⁵⁶

Osmond refers to the 'inextricable link between peer-review and the 'decapitation principle' which results in any attempt at rejecting the latter being regarded as a simultaneous rejection of the peer-review' 'since most reputable scientists favour the principle of peer review, they also feel trapped by the present system'. In a strong attack on how funding applications are treated he complains about the phenomenon of 'grantsmanship' - 'uncreative research' being rewarded because it is proposed by someone who knows how to 'play the game'; proposers doing work that has already been done; the refusal to fund less experienced researchers and 'technicians'; the unsophisticated nature of the system of review which is unable to distinguish between *good* and *best* proposals; the exclusion of 'all local knowledge of the applicant' by the reviewer; 'assassinations by the hundreds every year without defence' and the lack of an appeals or review system; the problem of plagiarism which reviewers themselves are susceptible to since they could unwittingly (or deliberately) 'absorb' the ideas reviewed and other such criticisms.⁵⁷

In another article Horrobin goes even further - arguing that the assumption that peer review is about quality control is misguided because it is 'inadequate'. Referring in particular to the field of medicine he suggests that the purpose of peer-review must itself be strongly associated with the purposes of the discipline in which it is applied; in the case of medicine, its purpose to 'cure sometimes, to relieve often, and to comfort always'.⁵⁸ This approach raises the critical question about the purposes of research itself and the argument between those who espouse the right to 'curiosity' as fundamental to academic life and freedom and as a criticism of the intrusions of state or other agencies - intent on sublimating these 'curiosities' to social and 'national' purposes - an issue I will deal with elsewhere.

Even more disquieting are the aspersions against peer review which relate to issues of power and the 'suppression of dissent'. Martin⁵⁹, probes the issue of the suppression of dissent in science by reference to academic writing in the areas of pesticides, fluoridation, and nuclear power. He

⁵⁶ See her discussion at page 308 et seq. See also Harnad's review of this subject in Harnad S. 1982 and Harnad S. 1986

⁵⁷ Osmond D H 1983. pages 96 et seq

⁵⁸ Horrobin D F 1990. And 1996 Horrobin, D.F. 1996. Peer review of grant applications: A harbinger for mediocrity in clinical research? *Lancet* 348:1293-1295.

⁵⁹ Martin B 1999.

argues that while there is no systematic literature on 'cases of attacks on dissident scientists', and about the 'the possibilities and difficulties in drawing links between suppression and corporate, professional and state power, respectively.'⁶⁰ There is yet a need for a thorough study of this issue since, in his view, the evidence is both unsatisfactory and contradictory. In addition to methodological issues in the study of suppression of dissent in science, criteria for assessing the alleged cases of suppression that are 'hushed up' are required. In his view:

*studying suppression has the potential to reveal much about the dynamics of expertise, power, and legitimacy in contemporary society, but this type of investigation is bound to remain controversial both because of definitional and methodological challenges and because it draws attention to an exercise of power that those exercising it would rather pass unnoticed.'*⁶¹

Another issue relates to the 'self-referential,' 'positivistic' and 'conservative' nature of peer-review. Self-referential scholarship's lack of openness to external evaluation means that it is only available to 'insiders' relying on an 'internal history' and removing the 'possibility of engaging with history as a dialectic about the past, present and the future rather than as the conservation of the present and a determination to uphold what appears to have usefulness *now*'⁶².

The assumption that new knowledge is validated by a self-referential system also poses the conundrum since what is new is often not recognisable as such by 'peers' in the absence of a conceptual framework or direct experience or awareness of it. They have no way of knowing about ideas arising from outside the paradigmatic scaffolding of their own research and writing.⁶³ This also suggests the possibility that direct self-interest plays a role in the validation and rejection of new ideas and approaches and removes the possibility of critique and engagement. In the field of law the 'rule of self-recognition' is the direct expression of this approach and removes the possibility of democratic practice through open discourse, debate and dialogue both in the community of practice and outside it. Self-referential approaches are wont to abjure any possibilities for thinking of issues from outside the particularities insisted on by disciplinary boundaries. Although such boundaries may be critical for deeper understanding they may be less than adequate where broader knowledge derived from approaches which are multi, cross and

⁶⁰ Ibid 105

⁶¹ Ibid 105

⁶² I am indebted to Dr. Andre Keet, with whom I had a discussion about this issue on the 23 July 2008

⁶³ Although differences may be taken as given amongst peers within a paradigmatic system

transdisciplinary, necessitated by the particular problematic under enquiry. This would be the case, for instance in matters relating to the resolution of complex physical and social challenges in areas as varied as water reticulation systems for rural dwellers, environmental degradation, socio-medical syndromes, space travel and even cosmology.⁶⁴

Jurisprudential questions best exemplify the positivism inherent in self referential approaches since they are predicated on a strict separation between the law and moral authority 'by changing the perception of the law to that of centralised, malleable legislation, by making the legal profession servile towards the politicians'.⁶⁵ The effect of these approaches is to confound commitments to the rule of law and constitutionalism. Self-referential approaches to knowledge construction moreover are dependent on the nature of the hypotheses informing such knowledge resulting in the rejection of new ideas simply because they have no currency in contemporary theories. Referring to Feyerabend's criticisms of this phenomenon Dunbar talks about the need to consider 'any alternatives that occur to us, however outlandish they might seem at first sight'.⁶⁶

⁶⁴ Although this also raises questions about the nature of multi-cross and transdisciplinary knowledge, its virtues and limitations, I do not digress here into that discussion. For instance Nabudere: 2005 argues strongly for what he calls 'trans-disciplinarity and criticises approaches which imply a role for multi-disciplinarity while continuing to cast knowledge as essentially disciplinary. There are many countervailing arguments to this approach too. These insist on the retention of disciplinary boundaries for deeper understanding.

⁶⁵ Berggren: 2006 at 217. For a critique of John Austin's positivism see entry in Stanford Encyclopaedia of Philosophy – insertion of Feb 17, 2005. <http://plato.stanford.edu/entries/austin-john/>

⁶⁶ Dunbar: *ibid*: 29

Conclusion

The criticisms of peer review notwithstanding, the justifications for peer-review are clearly not apocryphal since its most ardent critics have no agreed alternatives. The justifications for peer review range from the pragmatic (since there is no alternative we should accept the process of peer-review as otherwise we would open the floodgates of poor scholarship), to those that seek to strengthen its processes to avoid 'throwing out the baby with the bath water.' For instance, even though Osmond⁶⁷ questions the ostensible direct benefits of the system – since we cannot know what would have been the case if an alternate approach were in use – he argues the case for strengthening aspects of the peer review system because it is the system acceptable to most scientists and 'evaluation by one's equals is both essential and unavoidable wherever reputable science is done'.⁶⁸ This, despite his complaint about the inordinate power wielded by reviewers of funding application since they 'set themselves above other experts – on their own 'say-so' having no need to account. As he says 'no scientist can legitimately exercise so much influence; yet this is done all the time'.⁶⁹ Unsurprisingly he is less sanguine about the virtues of competitive bids for funding because of the lack of definition of its purposes and conditions; is critical of processes that 'eliminate' scientists and cause damage to their 'promising work'; and points to the contending interests of 'rivals' as reviewers. Arguing the need for change in the present system he makes a case for changing the 'destructively competitive' aspects of the present system and of the 'decapitation principle' it implies. His approach to the purpose of peer review is based on the view that:

Essentially all scientists of any worth are already highly motivated by the inner drives of scientific curiosity, humanitarianism, joy of discovery, ambition, and the desire for recognition by one's peers. We need no other monkeys on our backs or guillotine blades upon our necks'.⁷⁰

These perspectives about peer review should give us pause to consider what exactly the uses of peer review are beyond the requirements of funding and the idiosyncratic processes for validating intellectual endeavour. These considerations should reflect more purposefully on the uses of science and enquiry and how knowledge could be developed as

⁶⁷ Weller A C 2001 and Osmond D H:1983

⁶⁸ Osmond D H 1983: 96

⁶⁹ Osmond D H: 97

⁷⁰ Osmond: 1983: 107

part of the agenda of its public purposes no less given to ‘public reasoning’ and critical scrutiny. Academics have rightly pointed to the profoundly disquieting impact of knowledge commodification; its subjugation to the agenda of global corporate interests through commodification and the infractions against it following the demands of managerialist approaches to education. There is widespread agreement that the process of knowledge production should never be allowed to capitulate to the crass purposes of corporate profit-making and the abandoning of its public purposes and humanising social goals. Nor should knowledge production ever be in an uncritical and obsequious relationship with a democratic state. These scholarly orientations about the value of knowledge should similarly constitute an imprimatur against the exclusivist and self-referential rituals and hierarchical processes of academic enclosure which have no place in the search for democratic citizenship.

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