

Six comments and rejoinder on:

Grazia letto-Gillies, "[A XXI-century alternative to XX-century peer review](http://www.paecon.net/PAERReview/issue45/lettoGillies45.pdf)",

issue no. 45, 15 March 2008, pp. 10-22, <http://www.paecon.net/PAERReview/issue45/lettoGillies45.pdf>

Comment by Donald W Braben (University College London)

If ever there was an elephant in the academic room it is peer review. This ubiquitous process now dominates every facet and phase of everyday academic life. It was not always so. Until about 1970, tenured academics usually had access to modest resources which they could use as they pleased without reference to anyone. For most people, therefore, peer review only became an important issue when they had results to publish.

Grazia letto-Gillies focuses her attention on that latter process. She outlines the well-known weaknesses of peer review¹, and proposes a new system - "ex-post bottom-up peer comments" – that takes advantage of the new communication technologies. In her system, papers for publication are submitted to an appropriate open-access site where they would be screened to weed out "crankish" papers, for example, and then published by being posted on the site. Those who wish to comment on them are free to do so, subject to similar screening. Thus, papers would be published quickly, and those who have comments can also gain credit as they would be posted on a linked site.

Taken in isolation, her proposals have the merit of formalizing peer review at its informal best. Thus, in the time-honoured way, one may meet a colleague over coffee say to discuss new results or an idea, the colleague may make comments, and one's position is modified or not as a result. But letto Gillies' proposals cannot be taken in isolation. The post ~ 1970 world is now too complicated. Indeed, the very process of peer review is no longer uniquely defined.

Since about 1970, the practice of giving modest funds to tenured academics as of right has virtually disappeared. Now, for the first time in science's long history, researchers have little alternative but to submit their proposals in writing to an appropriate agency. The agency then subjects them to third-party assessments of quality, relevance, and potential deliverables from which it selects the best. This process is also called peer review. It should, of course, have been given another name. I have suggested *peer preview*^{2,3} but whatever its name, it is arguably more important than the processes applied to completed work. Without peer-review - peer-preview - approval, there are no new projects, and the issue of peer review – the old sort – does not arise.

¹ My favourite description of peer review was given by Richard Horton, the editor of *The Lancet*, in an editorial for the *Medical Journal of Australia* (MJA 2000:172: 148-9). "The mistake, of course, is to have thought that peer review was any more than a crude means of discovering the acceptability -- not the validity -- of a new finding. 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."

² Donald W Braben, *Pioneering Research: A Risk worth Taking*, Wiley, 2004.

³ Donald W Braben, *Scientific Freedom: The Elixir of Civilization*, Wiley 2008.

In addition, there is the question of who might read Letto-Gillies style publications. Articles published in peer-reviewed journals are on the agenda, so to speak. Researchers preparing submissions to funding organisations must demonstrate, among other things, that they are aware of progress in their fields. They *must* keep up to date, therefore. But Letto-Gillies' publications might initially lack the status of "progress", especially if they are controversial. Their reading might not yet be mandatory, therefore. In more sensible times, that would not be such a problem but academics today do not have as much time for reflection as they once had. Tenured academics are almost constantly preoccupied with the task of seeking new funds. Failure can lead to loss of team members or even to a group's disbandment. For similar reasons, those such as post-docs on soft money are almost constantly preoccupied with their future employment, and whether they will be able to pay for such mundane things as rent at the end of the year. In addition, teaching loads are increasing as student numbers rise. In these circumstances, "fire-fighting", and doing what must be done to survive take precedence. There is little time for anything else.

Letto-Gillies mentions the growth of the audit and control culture, and asks, rhetorically, whether this type of culture encourages academic endeavours. Peer review and its alter ego peer preview are among the most important pillars of that culture. Before about 1970, academics with an individualistic turn of mind could choose to ignore their peers' opinions. They might have been confident, for example, that they would eventually prove that their unique view of the world was correct. Unfortunately, however, we now ignore peers' opinions at our peril, and the short-term becomes virtually all that matters. "Eventually" does not get its chance.

The post-1970 developments are strangling research enterprise. Spontaneity has been lost and bureaucracy rules. It is tragic that, like children given something whose value they do not appreciate, the leadership in many countries today seems to believe that there is nothing special about academic endeavours. The university should therefore be subject to the same indiscriminate processes of optimization and performance assessment other institutions must endure. But the university has long been a valuable source of independent advice and new insight. Exposing its virtually every action to peer preview scrutiny undermines those vital functions. New insight, for example, usually challenges consensus.

I have discussed these problems in more depth elsewhere^{2,3}. However, the urgent need is for every researcher to find the time to stimulate a wider discussion of these issues, and to press for actions that will prevent academic life from drowning in the seas of mundanity.

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Comment by Stevan Harnad (Universite du Quebec and University of Southampton)

Flight-test before you fly

(1) There is nothing wrong with classical Peer Review (PR) that a supplementary Open Access (OA) system will not fix -- but OA is an "**ex post**" *supplement* to PR publication, not an "**ex ante**" *substitute* for it.

(2) OA means immediate free webwide access to post-PR journal articles ("postprints") immediately upon acceptance for publication, plus, in cases where the authors desire it, free access also to their pre-PR "preprints" **even earlier, for pre-PR commentary.**

(3) This solves most of the problems cited by Grazia Ietto-Gillies in "A XXI-century alternative to XX-century peer review": access, speed, scope, corrective feedback.

(4a) **Classical** PR is also (a) an *answerable* mechanism, with the referees, optionally anonymous, privately answerable to the editor, as is the author, for producing a paper that, once accepted, has been revised to meet the known and trusted quality standards of the journal in question; the editor is in turn publicly answerable to the journal's usership with its reputation **for quality.**

(4b) PR is also a (necessarily "ex ante") filter for users, so that they need not waste their **limited** reading time trying to peer review raw drafts for themselves, **nor** risk their **scarce and precious** research time trying to build on unsound results that have not met a known and trusted quality standard.

(5) "Ex-post" open commentary is neither answerable to an editor who answers for maintaining the journal's quality standards, nor is the author of an unrefereed draft answerable, having the option of revising or not revising to meet arbitrary self-appointed commentators' recommendations.

(6) Most serious referees and users do not have the time or the desire to work their way through raw unrefereed drafts, **neither** to referee them, **nor**, worse, to risk using them, **unrefereed.**

Systems like the one proposed by Ietto-Gillies have been proposed many times. What is needed is to test them, to demonstrate that they are capable of generating at least the same standards of quality and useability that we have now in each field -- **and also that they are sustainable and scalable. (Everything new works at first, for a while.)**

Until they are thus tested and proven, these are just evidence-free conjectures -- and conjectures that go against the actual experience of editors, which is: (i) that qualified referees (who often want and need the option of anonymity) are a **scarce, overused resource** that is already hard to mobilize when **referees** know that authors are answerable to editors **to ensure that they take** their referee reports seriously, hence even **less likely** to donate their time and attention to unfiltered and unanswerable raw drafts; (ii) that authors, (who often want and need the option of not making their unrefereed drafts public) seek qualified feedback from referees answerable to a qualified editor of a journal with known quality standards, so that their own article too can be certified as having met those known quality

standards; (iii) that users need research that is filtered and certified to have met known and trustworthy quality standards in advance (i.e., "ex ante").

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Comment by Roland Fox (Salford Business School, UK)

Criticism of the peer review system can all too easily sound like sour grapes. Yet, remembering that the whole Impressionist Movement sprang out of the “Gallerie des Refusées”, the review system has had notable failures in all fields. Surely Marxist literature should have realized that the system was failing before the collapse of Communism? Maybe there were such articles that were being regularly rejected but I doubt it. Self-censorship by authors and a belief that the process is biased leaves little room for true disagreement. Adding a twig to a branch is more the style, creating an ever increasing network - a little criticism to ease along the debate, seems to be the most that one finds. In my own area, I recall an eminent professor saying that a particular innovation in accounting had “come to its conclusions too early”: surely this is good? When a highly critical paper is published, and it does happen, it can all too easily be stillborn and left to gather bytes in some remote file that is forever forgotten. I can think of a notable author who demonstrated the inadequacy of traditional investment appraisal even for a simple replacement decision – it was ignored. Indeed, the relationship between research and reality is, to say the least, suspect. An investment appraisal technique much favoured by economists and written about endlessly in finance for which there are, according to my search engines, no case studies showing the technique actually aiding decisions. As for educational research and the UK government’s naïve belief that the results demonstrated breakthroughs, the less said the better. What is happening here? Letto-Gillies refers to a socialization process but in my opinion she rather understates the problem; it is wider than just social interaction between the editor, reviewer and author (as suggested by Bedeian (2004)). Controlling the “gate” by using rarefied techniques, theories and terminology, a self referencing, self-sustaining coterie of academics can carry each other into ever greener fields.

As Letto-Gillies points out, the whole of the academic career structure rests on research publications. Where the criteria are relatively clear one may argue along with Letto-Gillies that an open access system might be helpful in allowing ground-breaking articles to come through. But in the social sciences and, I suspect, the arts and indeed many areas of the sciences, the notion of ground-breaking is unclear. It is arguable in the Popperian sense that knowledge is not being developed as there is no real falsification process. Review has more in common with initiation rites – is the article *well* referenced, is the methodology *sound*, are the results *interesting* – shall we admit this member to our order? Some academics like the “eminent” professor quoted earlier (and obviously myself) are cynical about this process, but the outcome would be the same even for those with the best intentions. As noted by Letto-Gillies, there is a preference for statistical significance. I would submit that this is an honest attempt to develop falsifiable statements. Alas, the term is a misnomer as noted in my primary textbook in the 1970’s (Wonnacott and Wonnacott p. 188); the term should be “statistically discernable”. In addition the “size effect” – how much of the variation in the predicted variable (reading age, the price of a share, student scores etc) is being explained, multicollinearity – the close correlation of explanatory variables and the experimental effect (called the Hawthorne effect in business similar to the placebo effect in medicine), all make statistically significant findings highly dubious. Even in the sciences, epidemiological studies send out a stream of conflicting advice about the causes of cancer based on the same flawed statistical methodology. Unfortunately, the alternatives (e.g. grounded theory) simply offer different flaws. Nevertheless one worries that the cynics might be right. The gatekeepers might just try to keep out any real cure – not wanting to come to any conclusion. Remember that Dr Marshall actually had to give himself stomach ulcers before the disbelieving brotherhood would ordain this heretic. The Gavescon debate is only the most recent example of interests

distorting truths. But there is also a good side: the cold fusion debate and Creationism did not make it through the review system and anti global warming papers are sidelined. My point is that although there are examples of appalling review decisions and a failure to ask inconvenient questions, the root cause is not the review process but the problems of developing uncertain knowledge and the high stakes in terms of career advancement. Seen in this light, an open access system might do no more than give increased access to the process of coalition building. Would papers previously accepted be shot down in such a system? I suggest not, the loss of anonymity as suggested by letto-Gillies could well make any form of criticism career threatening. After all, why do we have anonymity? Would review be better for a kind of popular “bottom up” process leading to acceptance of papers that would have otherwise been rejected? I am not convinced, this is after all the role of conferences and maybe they should take this role more seriously. But the worry is that open access with minimal editing might merely give a platform to new low standards of debate. Authors could well be unwilling to make as much of an effort as the rewards will be less apparent and hastily written comments may be given undue prominence.

Clearly, there is a need to rethink the journal process for the XXI century as the process has undoubtedly been in part shaped by a technology that is no longer extant. Journals are, as letto-Gillies observes, beginning to use the techniques of the web with more developed comments sections – though this was always a rationale for hardcopy versions. But should a traditional review process precede web comments? My answer is an unfortunate, yes. At least it is a well defined “currency” in a very ill defined scenario. The key to my mind is to have an independent and anonymously reviewed comments section as a way of reviewing the editorial process – a kind of independent role to the whole process, as with a non executive director. Then perhaps the more abstruse contributions of certain “brotherhoods” would be shown for what they are. Although peer review is far from perfect, the big problems, as I have indicated, lie elsewhere. And should there be a “Gallerie des Refusés” under the control of the comments editor? Why not, as long as it is bytes, not books.

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Comment by Marco Gillies (Goldsmiths College London)

Peer Review and interdisciplinarity

In a recent (9th May 2008) talk at University College London, David Delpy, the chief executive of the UK Engineering and Physical Sciences Research Council (EPSRC, the main UK governmental research funding body in engineering and physical sciences), gave some interesting comments on peer review and its shortcomings. The UK research funding councils are increasingly trying to encourage more ambitious and interdisciplinary research projects, but are having great trouble doing so. This is largely due to the peer review process being disproportionately hard on interdisciplinary projects, with the effect that most of these projects being rejected, despite the research councils desire to fund such projects. Interdisciplinary work seems to be particularly problematic for the review process as it inevitably requires reviewers to judge work that is, at least, partially outside their expertise. The result is often that reviewers judge a proposal too narrowly from within their discipline. Prof. Delpy gave the example of bio-engineering projects being found wanting by biologists because they lack a clearly formulated scientific hypothesis, despite that not being suitable for an engineering project. The opposite extreme, that Prof. Delpy also found to be common, is that reviewers of interdisciplinary projects tend to comment, often negatively, about elements of the project outside their field, despite their lack of knowledge. This problem is potentially even more problematic than the first as it essentially means that projects can be rejected based on non-expert reviews.

Social Authorship

Bedeian, in his article on peer review (Bedeian, 2004) identifies the social authorship inherent in peer review. This paper stresses the negative aspects of social. I would disagree on this matter, because social authorship and discussion can produce better work, by bringing new insights of which the original author was not aware, as well as correcting errors.. So what is the problem in the case of PR? One issue that Letto-Gillies identifies is the power relation involved. But another point that she does not stress is the lack of visibility and attribution of the social authorship. A process of revision, that can change the opinions expressed in the paper to ones different from the authors, and addition of elements by the reviewers, occurs completely invisibly from the final readers. The post-hoc comments system would make this process much more visible and turn it into a proper debate. This should be supported by a system that allows authors (completely at their own discretion) to progressively refine papers in light of comments. Ideally there would be a history system where previous versions are retained so that readers can get a full picture of the debate. One possible result is that authors might tend to upload work in progress (e. g. pilot studies) even earlier in order to get comments and start debate (and probably to get an early priority). This would mean that the positive aspects of social authorship could go much deeper, influencing the research while it progresses, without so many of the negatives.

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Comment by Paul Ormerod (Volterra Consulting, UK)

A recent experience

I thought the following experience I have recently had with the refereeing process might be of interest in the context of Grazia Letto-Gillies' paper "A XXI-century alternative to XX-century peer review"

I have written a technical paper looking at whether there are cascades across countries which lead to global recessions. In other words, what are the chances that a recession which starts in one particular country will spread to others.

I use data in 17 countries from 1870 to 2006. I discover empirical features of recessions which are not in the standard economics literature:

- the statistical distribution of duration of recessions within individual countries,
- the statistical distribution of 'wait times' between recessions within individual countries,
- the statistical distribution of the proportion of developed countries in recession in any given year.

I have a simple theoretical model, based on cascades across a network, which is consistent with all three of these key empirical findings

We might think that both these empirical findings and the aim of the paper itself would, especially in current circumstances, be of interest to economists.

I sent my paper to the *Quarterly Journal of Economics*, which has carried articles on cascades of various kinds in the past. Within 24 hours (!) I received the following from neo-classical Nobel prize winner Robert Barro:

'I am sorry to report that we will not be able to publish your paper, MS 14032, entitled "GLOBAL RECESSIONS AS A CASCADE PHENOMENON WITH HETEROGENEOUS, INTERACTING AGENTS." I have concluded that there is not enough value added for a general economics audience in the present paper to warrant publication in the *Quarterly Journal of Economics*'

So establishing new empirical results on the structure of recessions is not, in Barro's view, adding enough value for a general economics audience. And this is in April 2008, when there are worries everywhere about whether a US recession will spread!

I cannot help but note that real business cycle models will be unable to replicate these key features of recessions in the capitalist economies which I have discovered.

I'm always prepared to take comments and criticism on papers I write. I don't mind papers being rejected on quality, but not to even send such a paper to referees when the question of a global recession is a key current issue seems to me very odd.

Anyone who would like a copy, please email me at pormerod@volterra.co.uk (my website is currently being re-designed and only a very old version is accessible)

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Comment by Menakhem Ben-Yami (Israel)

“Right” books, “right” periodicals, “right” paradigm

Mainstream science and economics are as sluggish in changing their course as a 500,000-mt tanker. The inertia is tremendous. Fishery science is a good example. More and more independent scientists and even some scientific bodies are recognizing the shortcomings of the old models and methodologies, hence, the low reliability of stock assessments. Nevertheless, those models and methodologies are still being used throughout most of the world's fisheries. One of the mainstays of the prevailing paradigm is the way scientific articles and papers are peer-reviewed. In most cases they are sent to “peers” of the same discipline and the same educational background, who usually only check whether the consensual methodology has been followed and no mistakes made. The reviewers don't question the basics, because they wouldn't admit that they had for years practiced inadequate science.

Similarly, there's a he problem with some economists; not that they are not qualified professionals, or that their statistical methods are wrong. The problem, in my view, is rooted in their very pretense that economics is an objective discipline and, hence, that the prevailing economic school can produce, using certain methodology, impartial recommendations for “efficient” policies and (among others) fishery management that would be beneficial to society and nation. This prevailing economic school dominates also the “peer-review” system. Reviewed by the “right” peers, reports and papers would be published in the “right” books and periodicals, as long as they stick to the “right” paradigm.

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Rejoinder by Grazia Ietto-Gillies (London South Bank University, UK)

I would like to start by thanking very warmly all the people who have taken the time and trouble to respond to my article. Taking all the contributions together, I can see some agreement, some criticisms and some novel points. They are all very welcome. I will not try here to respond to each separately but I will summarize the main points of my proposal as related to the overall comments.

I would first like to stress that a new Open Access system for putting papers into the public domain is already with us or underway. Most researchers now post their papers on their own web sites prior to publication in journals. Moreover, the move towards assessment of research output via metrics is having an effect on this process. Some very prestigious universities – including Harvard and University College London - are organizing web sites of all the research papers – published, unpublished, current and past – by their staff. The aim is to have an institutional e-archive in which their academics' works become easily accessible and other researchers throughout the world can access and cite them. The reasoning and purpose behind this initiative is obvious: if what matters is citation, then let us make citation easier and this means making one's works more accessible⁴. So the move towards an Open Access system is well underway. It could, indeed, be claimed that what I propose is too conservative and that people do not see a need for an overseeing editorial process at all: they can just put their papers on the web and it is for others to decide which to read and cite. It is already happening: many of us cite papers published on the web rather than in journals. Whether we like it or not, the process is unstoppable.

My proposal is for a more managed process, one in which there are light-touch editors in charge; editors who would also encourage and channel comments and debates which I consider essential to the process. Why do I want an OA system for putting research results into the public domain? The answer in one word is: *efficiency*. There are several respects in which the proposed system is more efficient than the current PR system. It would put fewer obstacles for ground-breaking, unusual works to find their way quickly into the public domain. It would greatly lower the costs of having works put into the public domain: here the savings are seen both in terms of financial costs and in terms of opportunity cost of all the time that editors and referees of journals put into the process. It would ensure a speedier system for getting papers into the public domain. It would encourage a culture of open debate in which the community of researchers will not shy away from making critical comments or adding new points to somebody else's paper because they know they will get attribution. It would create sites of specialized research contributions similar to the current system in journals. A further advantage of my proposal is that it would make access to research works more democratic because it would be equally accessible by researchers in rich as well as in poor countries: all the researcher needs is a computer. Currently many researchers in developing countries are cut off by the high costs of journals in relation to the resources of their libraries.

Now for the comments. We have all received and continue to receive rejections to papers by journals' editors and their referees; they are always hurtful and the more so if the process is perceived to have been unfair. Some of us have inflicted rejections on others – hopefully not unfairly - and continue to do so. Yet, hurt feelings and fair or unfair referees' reports are not the reason for my proposal; my main reason is just efficiency made possible by the new technologies. After all, would it be less hurtful to have strong criticisms published

⁴ A long term effect of the spread of e-archives will be savings on journals' subscriptions by libraries. This, of course, will undermine the viability of many publishers.

on an open site? Some may respond that signed criticism would be less strong. Well, researchers and academics in general do not shy away from strong critiques when writing book reviews, why should they do so when commenting on a research paper?

Let us now look at the main objections to the proposed system. There will be a lot of worthless papers put into the public domain; yes, undoubtedly, but there already are on the web and in journals; but, at least, putting them on the web will be less costly for the research community world wide than it is at present. Moreover, the OA system envisages a bottom-up evaluation process via comments from peers.

Yes, what I propose is not fully tried yet and there will be problems when we try it; but this is true of any new system and the problems cannot be fully experienced and faced until we start embracing the new system. In fact, I see a major problem with a period of transition between the old PR and the new one: as long as the PR system is seen as the gold standard of quality assurance in publications, researchers may become reluctant to move away from it for fear of damaging their career prospects or chances in grants applications. However, here the introduction of metrics/citation indices into the assessment of research may act as a spur towards a new system; the establishment of institutional e-archives as mentioned above is a good example of this development. Last, but not least important, we should not underestimate the resistance to the new system by the publishers of journals.

Nonetheless, it is worth reiterating that access to unpublished papers on the web is already with us and that the process is unstoppable. What I propose is a system in which fairly open entry into the public domain is combined with encouragement of comments by peers in a process that furthers the social nature of research.

A few months ago when Edward Fullbrook offered me to publish my paper in *Real-World Economics Review* I had already committed it for a special issue of a journal to be edited by a friendly American colleague. I withdrew it from the latter and I still hope that the colleague remains friendly and understanding. The reason for opting to publish in the current *Review* is partly consistency with the nature of the paper and partly to give the paper a chance to be read widely and commented on; given the subject I thought that this latter element was important. My instinct was right and I thank Edward Fullbrook for his offer. We have had a good number of comments and the overall experience in *test flying*, minuscule though it is, shows the following.

- People do contribute to debates particularly when encouraged.
- Researchers are prepared to give both supporting and critical comments.
- Attribution of comments may have encouraged some novel points to the debate.
- The process has led to self selection; it is people interested in the issue who have contributed to the comments, whatever the reasons for their interest.

On the whole I think that the research community is ready for a new system that takes full advantage of the available technologies for the benefit of the community itself. When it comes to evaluation and dissemination of research results all I am saying is: let's give technology and efficiency a chance.

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