

About Peer-review

Many people seem to have the impression that scientific papers are of the highest quality and credibility only if they have survived the peer-review process practiced by science journals. Such an impression belies the naivety of the person and the blind faith they have in a very flawed system.

Peer review was intended to be a "sanity check" so that journals didn't make fools of themselves publishing rubbish but even from the early days it was being used to promote some lines of thought and suppress others.

It has always been a method of "outsourcing" judgment as to the merits of a paper by people with insufficient skill or motivation to make those judgments for themselves.

The American Geophysical Union (AGU) publishes several scientific journals and among its information for authors has this to say¹:

Peer Review Process

The decision to accept or reject a paper lies with the scientific editor. This is the peer-review process, step-by-step:

1. Author submits manuscript. Authors provide the names and contact information of five experts in their area as possible reviewers.
 2. AGU Editor's Assistant performs quality checks to ensure author's manuscript meets journal submission requirements.
 3. Potential Reviewers are assigned and contacted.
 4. Reviewers review manuscript.
 5. Editor makes decision to accept, request revisions, or reject. The Editor may confer with Associate Editors and reviewers in making this decision.
 6. If decision is a revision, Author revises manuscript and submits revision.
 7. Editor makes new decision.
 8. Author is contacted with decision.
- Steps 6-8 are repeated as necessary until Editor makes final decision.

In my experience there's nothing unique about these comments from the AGU; other journals make very similar comments, or at least the processes are the same.

Steps 1, 4 and 5 are of particular relevance to understanding how the peer-review system operates in practice and these will be discussed below to show that peer review is not the "gold star" seal of approval that many believe it to be.

This essay is written from experience because at the time of writing this I am the author or co-author of three peer-reviewed papers and a fourth has been accepted by a journal. The comments below largely focus on the events surrounding one such paper published in July 2009.

¹ <http://authorguide.agu.org/submission-process/>

The first of the above step that is relevant to us is "1. Author submits manuscript. Authors provide the names and contact information of five experts in their area as possible reviewers" and in most, if not all instances, the journal's request that the author provide these names is accompanied by a statement such as "Please do not list colleagues who are close associates, collaborators or family members".

Authors who want their papers published (and isn't that all authors?) are hardly likely to provide the names of people who hold the opposite opinion to that expressed in the paper and are likely to be highly critical of it. In reality they are likely to nominate people whose views are aligned with the paper who will give the paper as easy a path as possible. This easy path might not be a conscious decision from reviewers and could be that the reviewer's understanding of the subject means that he fails to notice passages that are unclear and require better explanations for less knowledgeable readers.

When scientific fields are small, the number of experts is low and the interaction of researchers within that field is high, such as with climate modelling or the reconstruction of historical temperatures from tree-ring data, finding genuinely impartial experts to nominate is difficult.

Doubtless some authors of papers will genuinely try to play fair by nominating potential reviewers as people who they believe will act honestly and without bias, but the situation is very subjective and blatant abuses are nothing new.

One example of abuse can be seen in a Climategate email² sent on 5 August 2009 by Phil Jones, director of Climatic Research Unit at the University of East Anglia and incidentally the author of numerous peer-reviewed papers. In his email to his co-authors Jones mentioned several potential reviewers and went on to say:

"All of them know the sorts of things to say - about our comment and the awful original, without any prompting."

In other words these potential reviewers were being considered not for their expertise and impartiality but because of a level of confidence about what they would say.

The relevant step in the AGU's process as described above is "4. Reviewers review manuscript."

Three reviewers commented on the 2009 paper for which I was lead author. One wrote three paragraphs (10 sentences). Another wrote just four numbered sentences. The third wrote 1.5 pages with a summary, a general comment, a discussion of three major issues, a short list of minor issues and a suggested reference.

² See <http://www.ecowho.com/foia.php?file=3500.txt>

In response to our paper, a Grant Foster and eight other authors submitted a Comment (i.e. formal criticism) to the same journal. The comments from the three reviewers of the Comment can be found in another Climategate email³.

The first reviewer said that the Comment did an excellent job of refuting our paper then offered several suggestions for improvement and made it clear that they were suggestions. The second said the comment could be published as was and then proposed that a different word be used in a few places in the text before offering some advice on presentation. Both of these were in the order of 100 words.

The third began by saying "Accept pending major changes (in style not scientific comment). The real mystery here, of course is how the McLean et al paper ever made it into [the journal]". This was followed by two more sentences critical, not of the Comment, but the paper that the Comment referred to. This third review concluded saying "In general, the current paper is sloppy and needs tightening. I don't think the lead author needs 10 pages of text to make the main points."

As you may have gathered from these two examples, there is no standard approach to reviewing, no template, no checklist; it's all left to the reviewer to write as much or as little as he or she wishes, and it might seem from the third reviewer, on whatever top he wishes.

These reviews failed to identify several fundamental failings in the Comment.

(a) The abstract of the comment stated:

McLean et al. [2009] (henceforth MFC09) claim that the El Nino/Southern Oscillation (ENSO), as represented by the Southern Oscillation Index (SOI), accounts for as much as 72% of the global tropospheric temperature anomaly (GTТА) and an even higher 81% of this anomaly in the tropics. ...

But the opening sentence of the Introduction to the comment said:

They claimed that more than two thirds of the interseasonal and longer-term variability in global tropospheric temperature anomaly (GTТА) (72% using the 29-year-long MSU satellite record and 68% using the longer 50-year RATPAC-A record), and an even larger 81% of the variation in tropical (20±S-20±N) tropospheric temperatures, can be explained by the long-term variations in the Southern Oscillation Index (SOI).

The abstract is talking about values whereas the Introduction correctly says that our paper talked about variation in values. An analogy would be to discuss speed at one point but acceleration at another.

³ See <http://www.ecowho.com/foia.php?file=1254179301.txt>

- (b) Foster et al. claimed that we used a technique that filtered data, and then made our key statements on the basis of that filtered data. In fact, we used the filtering technique solely to establish that a 7-month time lag existed between changes in the ENSO and changes in global average lower tropospheric temperature, which was a non-controversial finding that accords with earlier research (e.g., by Phil Jones, who is one of the co-authors of the Foster et al. comment). Our substantive conclusions were then based on applying this time-lagged relationship to the raw data sets, for which purposes the methodology by which we had established the time lag are irrelevant.

In other words, the criticism focussed on the Analysis part of the paper in which established that a time-offset relationship existed in the data, not the Discussion and Conclusions both of which were based on that relationship. The Analysis section is always a preamble to the Discussion and Conclusions, which are the heart of any paper and where criticism should be focussed.

- (c) In their conclusion, the comment claimed that we asserted that the relationship between temperature and ENSO could not be simulated by climate models. We made no such statement, and referred only to the IPCC's Fourth Assessment Report of 2007 saying that models of the ENSO system can provide acceptable predictions only to about 12 months ahead.

These are three very basic faults with the comment. Competent reviewers would have reported these issues. A competent editor would have noticed them himself and taken action.

It's therefore not only the authors of a paper who can manipulate the system but also reviewers and even journal editors who might choose to turn a blind eye to flaws in a review or maybe didn't make any effort to check.

The final relevant step in the AGU's process described at the start of this document is "5. Editor makes decision to accept, request revisions, or reject."

In our own experience, and it's confirmed by a Climategate email, the journal editor sends an email that contains a preamble then gets down to the business of instructing authors to ...

Please carefully consider the Reviewers' recommendations for revisions, make the necessary changes, and respond to me with a point-by-point response of how you have addressed each concern. In your cover letter, please include a statement confirming that all authors listed on the manuscript concur with submission in its revised form.

It goes on to provide a deadline for the acceptance of the revised paper and that its non-arrival by that date will be interpreted as the paper being withdrawn.

There is no explicit statement to the effect that the paper requires revision, no indication of a range of responses that would be acceptable and, if reviewers express conflicting views, how these should be dealt with. Authors are left to interpret the comments and make their own deductions.

In the case of our paper we were requested to provide a statistical indicator that neither the three of us or anyone that we knew could explain. We took a best guess at what was required and the reviewer approved the inclusion; we still don't know if we provided exactly what was requested.

When it came to the Comment we were explicitly invited to respond and told that the journal intended publishing the Comment and our Response together.

Things got off to a bad start with that response because I exchanged several emails with the editor – actually a new editor, the original one having been replaced – about the fact that the Foster criticism had already been published on the website operated by the lead author Grant Foster under the pseudonym "Tamino".

I quoted the journal's notes to authors from its web pages about how it would not accept material that had already been published. I also pointed out that it had been shown in the journal's format when one web page, said in contradiction to the other, that the "unformatted papers" may be posted on their websites.

The editor's initial response was that being published on the Internet wasn't in fact being published. After several emails he acknowledged that the Comment had in fact been published but then when confronted with my email about its formatting I received no further correspondence from him.

Readers are invited to compare our editor's stance to that described in to another Climategate email⁴ that says:

Thanks. Bad news today. Nature Geosciences wont publish this because the Real Climate Blog mentions (more vaguely) the basic content of what we have written. That is indeed the reason Nature Geosciences have given. It seems blogs can now prevent publication! ...

We wrote and duly submitted the Response, mentioning in particular the flaws in the Comment as described above.

We probably made life difficult for the journal because if it published our response – and to do otherwise is almost unheard of in scientific publishing – would mean that we would be showing the flaws in the Comment and by extension the incompetence of the Comment reviewers and the journal editor for not acting on those flaws.

⁴ See <http://www.ecowho.com/foia.php?file=1231279297.txt>

The editor sent our Response to reviewers although gave no prior indication that he would do this and it seems contrary to the journal's online instructions to authors. Those instructions only state that authors of Responses should address the issues raised in any Comments. To my mind that should be the full extent of the checking given that the authors had already submitted a paper on the subject and other reviewers had approved its publishing.

The review comments show the travesty of the process.

Reviewer 1 said " ...they certainly did not go out of their way to make it clear to the reader that their conclusions and interpretations applied only to these derivatives and not to the unfiltered SOI and temperature series, and by omission of these reminders, they implicitly and inappropriately invited the reader to interpret their results as applying to these unfiltered series."

This was the reviewer's only point. We felt the paper was plain enough and in our response reiterated the separation of findings based on derivatives (i.e. processed data) and raw data, which means that the reviewer could only have been – and wrongly - referring to our original paper.

Reviewer 2 began by acknowledging that we had clarified the use of derivative data and raw data (which contradicts Reviewer 1 above) but then focussed on statistical trends, mainly from our original paper, and failed to take into account our mention of irregular disruptions to the pattern that our finding was based on. Although better expressed this criticism was therefore also flawed.

Reviewer 3 appeared to take no notice whatsoever of our Response because his comments seemed to refer to our original paper. He opened with the statements "REJECT! The reply by McLean et al to [Comment by Foster et al] is not worth publishing. The techniques they use and conclusion drawn from them are largely bogus. How in the heck did the original dog of a paper ever get through the review process. Please check it out and reprimand the appropriate editor."

His emphasis is also shown in his concluding sentences " Finally for the Editor... My job would have been a lot easier if you had sent me their original paper and the comments by [Foster et al]. Please make a note."

A competent editor would have recognised that these comments didn't apply to our Response and refused to accept them.

It should also be noted that reviewers are anonymous and could really be anyone, even the journal editor wearing a different hat. That said, given that the Comment by Foster was co-written by several people with a reputation for climate alarmism and they discussed potential reviewers on the basis that "All of them know the sort of things to say" (see above), the reviewers of our response could well have been those nominated by Foster et al or at least those invited to comment on the Comment by Foster et al.

All up the peer-review process is like a lottery that the participants – authors, reviewers and editors – can try to manipulate to their advantage. The reviewers are anonymous and therefore it's a process that can be used to support the findings of a paper or to suppress them

and because of the anonymity of reviewers the authors cannot challenge the review comments on the grounds of bias or incompetence.

The process is informal with no template or checklist. Reviewers can write as much or as little as they wish, they are not required to provide evidence of their thoroughness and their failure to identify and report issues is easily overlooked by editors who can range from the very diligent to the opposite.

Peer-review in its current format simply cannot be regarded as a gold seal of approval.

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