

MANN'S BEST FRIENDS

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Dear Benny,

I followed the link from Andrew Revkin to <<http://dotearth.blogs.nytimes.com/2009/10/05/climate-auditor-challenged-to-do-climate-science/>> and I am not sure that I did not read more into it than Andrew intended me to.

The blog entry certainly was trenchant, and the attached threads if anything more so. All very entertaining if that is the sort of thing that entertains one. In all it vividly recalled Bierce's definition: "CONTROVERSY, n. A battle in which spittle or ink replaces the injurious cannon-ball and the inconsiderate bayonet." Spittle had the upper hand in that blog, I should say.

However, the point I wish to make here hardly surfaced in the blog though it is the most important theme of this entire matter. If AGW or any other form of climate change begins to bite, the question of Humanity's continued survival eventually will assume a fascination all its own, but for the present there are more immediate concerns, in particular the question of the survival of integrity and sense in science, and the role of peer review.

Consider: "Michael Mann of Pennsylvania State University said that if Mr. McIntyre wants to be taken seriously he has to move more from blogging to publishing in the refereed literature..." and "Skepticism is essential for the functioning of science," Dr. Mann said. 'It yields an erratic path towards eventual truth. But legitimate scientific skepticism is exercised through formal scientific circles, in particular the peer review process... Those such as McIntyre who operate almost entirely outside of this system are not to be trusted.'

Such stern sententiousness, irrespective of its source, should shame the most unregenerate cavillers to kennel, except those who pause slitty-eyed, to reflect on what he actually meant. "Peer review process", hmmm...? Those operating almost entirely *inside* of this system *are* to be trusted, are they? The same system that passed all sorts of publications of the most assorted standards during the last century or so, not to mention certain particularly embarrassing examples very recently? Publications that led to blushes inversely proportional to how effectively and for how long the parties concerned could distract attention from them? The same peer review process that has served as the most powerful tool for intimidating, quashing, and crippling the slightest dissent from the approved line? For punishing anyone who breaks the ranks of the favoured? For emasculating or deferring publication of the research of upstarts? The most powerful weapon for delaying outsiders' discoveries to the point of loss of priority of publication, or even to fatal obscurity?

Surely not! Which is fortunate, because that is not the point that I had referred to. Plenty of abler critics have raised similar objections more biting than ever I could.

No, the peer review that I write to praise and not to bury is the peer review that for generations of scientists has been the sentinel and shield against erosion of standards. It has been a sheet anchor both of the elite and the merely workmanlike journal, the means of assuring the editorial staff that the work they publish is sound, non-trivial, constructive, an advance on preceding work, a stone in the edifice of growing human knowledge. It has been an aid to efficiency, speeding the selection and augmenting the quality of the product of the researchers' labour and ingenuity; and of course (though perish the thought of any such sordid considerations crossing the mind of the authors) enhancing the kudos appertaining to the publication of the item.

Good stuff. Very good indeed. And yet I cannot rid my mind of a framed engineering degree on the wall of the office of an erstwhile young colleague of mine. It was in a large company, employing many graduates, and yet he was the only one that I remember nailing his colours to

the er, wall in such a way. Any time the standard of his work or his good sense got challenged, he would point at his degree in rebuttal. Unanswerable of course.

And yet he did not last long, strangely.

Am I the only one to see this anecdote as relevant? Sorry. . .

Peer review as it should be used in a perfect world should not be a major concern of the author (except when a generous reviewer offers assistance or admonition, typically anonymous).

Peer review also should not be a major concern of the reader. If I read material dealing with a field I am so unfamiliar with that I cannot even follow the train of logic, then I act in bad faith and bad sense if I accept or condemn it on the grounds that it was or was not peer-reviewed. If however I can follow the logic, but without being able to challenge actual facts or observations, then I am able, with appropriate reservations, to accept, challenge, or reject the logic in good faith, but I still cannot justify my opinion by reliance on any peer review process. If I can claim to be fully conversant with the field, then I can accept, challenge, or reject any part, context or aspect of the work. If in doing so I need to defer to the dread dignity of peer reviewers, than how can I claim competence in the field at all? If I need to ask how it was reviewed before I consent to trust the work, then why am I reading such stuff, when there are plenty of Mills & Boone books to challenge my intellect?

Peer review or no review, it is for all readers to accept or reject research results according to what they find personally convincing. In good sense or good faith no research worker can justify a decision to accept or challenge work according to whether it had been peer reviewed.

That is not what peer review is for.

To criticise or praise a *journal* because of its eschewal or quality of peer review is reasonable in suitable contexts; even if one assumes that the editor is omniscient, it may be comforting to reflect that independent review guarantees lack of bias. However, to challenge the work of an *author* because it had not been favourably peer reviewed, is the most breathtakingly abject tactic I have seen, short of running to mummy because these nasty people had been disagreeing with ums. The more I contemplate it, the less it makes sense.

Consider what such justification for rejection amounts to: some third parties somewhere, who hadn't been asked to vet the work, but who might or might not have approved it if they had been asked, had not actually said anything about the work. Right? So because the work was not considered by those third parties, it thereby is errr. . . to be neglected without rebuttal by those in response to whose work it had been presented? Why should we respect authors who had been unable to assess the merits of criticism of their work or defend their work independently of peer reviewers? In the example under consideration, the criticism after all, did not involve novel work or novel techniques, but a critique of (peer reviewed) work. What role is peer review of the critique to play in such a case? What sort of peril would such peer review be intended to avert? Even in the top scientific journals, letters to the editor in response to peer reviewed articles are not in general peer reviewed. Right?

Never mind! Let's get back to the real world.

This much at least should be clear: science is passing through a most painful phase. (At least I hope that "passing" is not too optimistic a word!) As scientists we have a century or so of frequently (not invariably) inappropriate reliance on a cumbersome system. We have to deal with problems of ethics, politics, information explosion, population explosion, and technology explosion. In my opinion the peer review system *in its current form* has outlived its usefulness, in many respects even its viability. Whether the next generation is to rely on something totally new or on an amended review system, I cannot say, but what served for say the 1950s is hardly

likely to serve for the 2050s. Some developments apparently in process within some Internet publications, in which pre-publications are exposed to public execration or appreciation before the final editing, may point the way to the future, but whatever form it takes, something new is needed.

Whether it turns out to be in the interest of the editorial staff, the author, or the reader, the fact remains that peer review as she currently is spoke, notionally is primarily for the benefit of the editorial staff, only contingently for the benefit of the author, and usually irrelevant to the reader, whether friend or enemy. But those who appeal to the process for shelter from unwelcome assessments of their work, or their duties to their readers; for some reason recall to me two lines of Burns written in a slightly different context:

From Envy and Hatred your corps is exempt,

But where is your shield from the darts of Contempt!

In case that strikes you as insulting, I invite you to consider it in the perspective of the insult to the reader at whom certain helpful remarks were directed -- remarks of the form: "Those such as McIntyre who operate almost entirely outside of this system [of the peer review process] are not to be trusted." We readers apparently are seen as stupid enough to swallow the hockey stick without choking on the mediaeval optimum or little ice age, but too stupid to gag at the implications of the physics of photon absorption, the history of volcanic influences on the climate, the principles of sample significance, or the implication of withheld data -- and far, far too stupid to read a statistical argument?

Unless it is peer reviewed?

No wonder Mann's best friends turned on him and bit him.

Jon