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Pyroclasts:
Nothing new, nothing new

John Shaw

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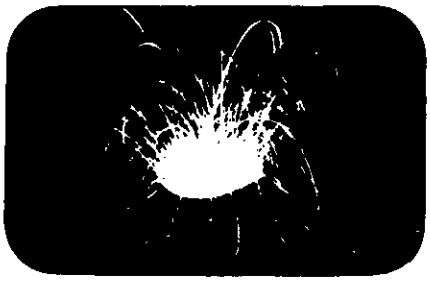

Pyroclasts

Nothing new, nothing new

John Shaw
Department of Geography
Queen's University
Kingston, Ontario K7L 3N6

He wrote four papers, they might be true.
Nothing new, nothing new.
Yelled the hordes of the peer review

Over the period of the recent summer Olympics I broke a personal record, zero for four on submitted manuscripts. It may be that they were simply of poor quality and, in years to come, I will be grateful to the referees. But I have a nagging suspicion that there were other considerations involved. Each paper challenges conventional wisdom on drumlin formation and was rejected by drumlin "experts" who have contributed to that wisdom. There is a potential for conflict of interest here, and I am convinced that it exists when the negative reviews, which call for outright rejection, are lacking in substance, insulting, and commonly sarcastic. It goes without saying that they are invariably anonymous. Other reviews have been open minded, but it is my impression that, for a number of reasons, editors play safe and are guided more by negative reviews than positive ones.

Barber (1981) quotes T.H. Huxley on this important issue of prejudiced referees:

"Merit alone is very little good; it must be backed by tact and knowledge to do very much. For instance, I know that the paper I have just sent in [to the Royal Society] is very original and of some importance, and I am equally sure that if it is referred to the judgement of my "particular friend" — that it will not be published. He won't be able to say a word against it, but he will pooh-pooh it to a dead certainty ... So I must manoeuvre a little to get my poor memoir kept out of his hands."

Huxley was relatively lucky; he had only one "particular friend" and room to manoeuvre. It appears I have a number of such friends and, since I cannot identify them by name, manoeuvre is somewhat difficult. I feel that sending one of my papers to a drumlin expert is like submitting a work by Daniel Ortega for Ronald Reagan's consideration or a pamphlet on birth control for the approval of the Pope.

Perhaps, it is necessary to outline my credentials to discount the possibility that these are the ravings of a crank, who could not pass a drug test, trying to market an absurd idea. I have a Ph.D. in glacial sedimentology, and have tenure and am a full-professor at a respectable university. I teach undergraduate students and have supervised a number of graduate students at the M.Sc. and Ph.D. levels. I have served on a very large number of thesis examination committees both as an internal and external examiner, and on three occasions I have been asked to serve as an external examiner abroad. I have externally reviewed several promotion and tenure cases and more manuscripts than I care to remember. I am an associate editor of the *Journal of Sedimentary Petrology*, and, on his recent retirement from the editorship, Norm Smith wrote:

"I've appreciated the conscientious efforts with which you've handled reviews as well as the thoughful and balanced recommendations you've passed on."

I have given short courses on glacial sedimentology organized by the Society of Economic Paleontologists and Mineralogists, the United States Environmental Protection Agency, the Geological Survey of Canada, and the Geological Society of America. The course evaluations suggest that the participants learned a lot and enjoyed my presentations. I have also served as a member of the NSERC Earth Sciences Grants Committee determining funding for Earth Scientists in Canada. I am invited to give more talks at other universities than I can possibly accept.

Sorry about all that trumpet blowing, no doubt many quit reading half-way through, and others think, "With all that on his plate, no wonder he writes lousy papers". However, I ask myself the question, "How can I, on the one hand, be given so much scholarly responsibility, but, on the other, fail to get even one out of four manuscripts published?"

I would like to illustrate my point by using an open letter to a reviewer of one of my papers. The review has the lack of substance and the vindictiveness that have become the hallmarks of one of my "particular friends". He may not have written it, it was of course anonymous, but someone with a similar mentality to his did. My reason for making this a public issue is to draw attention to the devastating effects of such people on the science community as a whole, and on young scientists in particular.

" *Dear Anonymous Reviewer,*

You recently reviewed a manuscript of mine on drumlins and subglacial deformation theory. We appear to have some fundamental differences in our philosophies. It is telling that you were perturbed because I chose to attack another hypothesis rather than defend my own, for here lies an important difference in our view of science. I think that, since proof is impossible in Earth Science, falsification is an extremely important route to progress. By falsification of working hypotheses, we may concentrate our efforts on the remaining, plausible ideas. To you, this approach involves the trivialization of conventional wisdom; to me, it involves exciting advance. You have every right to your views, but I wonder why you are so anxious to suppress mine.

As an example of our differences, you suggest that my refutation of the Smalley and Unwin theory lacks substance. I point out that the shear stress zonation central to their theory is contradicted by elementary theory on basal shear stresses. Their suggestion that there is no subglacial deformation in the marginal zone of glaciers is directly contradicted by the observations of Boulton and his co-workers. Finally, the theory contravenes the principle of conservation of mass. I wonder how substantial a criticism must be before it passes beyond what you call "unsupported opinion"?

You would also serve your purpose better if you read papers you review with care rather than distorting what is written and declaring your perceived version to be ill-conceived and fatuous. The use of these two derogatory terms brings me to the important message I wish to convey. It seems to me that as much care to avoid insult is called for in reviews as in published papers. I wrote that some scientific theory is a product of wishful thinking and subjective effects. You state that such thoughts verge on the insulting and have no place in the scientific literature. They are already in the literature and were attributed, in my paper, to I. Bernard Cohen, Victor S. Thomas Professor of the History of Science, Emeritus, Harvard University. Yet you consider such views to be quasi-philosophy. I hope that the courage of your convictions will cause you to publish your highly original views on the philosophy and history of science.

Given the tone of your review, I am genuinely perplexed at your concern to keep insult out of the literature. Your review *is* part of the scientific literature, albeit the unsigned scientific literature. Presumably, according to your concern for courtesy, you agree that a review should be as politely written as a paper. I wonder how you justify referring to me as: emotional, perverse, peevish, derisory, derogatory, inflammatory, and guilty of poor judgement and lack of balance? You describe my work as: badly written, quasi-scientific-cum-philosophical, poor, superficial, ill-conceived, fatuous, and insulting. All this from one who considers that insult has no place in the scientific literature!

My paper has my name at its head; I have never written an anonymous review, no matter how hard my criticism. But we differ here too. Why do I write this letter to you and others who spit invective from behind the cloak of anonymity? Because, sir or madam, I have some confidence in my ideas and can see through your game. But there will be other, younger and less established researchers with novel ideas and all the insecurities and self doubts that go along with exploration in uncharted waters. You and your fellows with your abuse might well devastate the heart and mind of one of them. Please think on this, it is not written lightly.

Yours sincerely,
John Shaw ❞

Well that's a lot off my chest, but I still have four unpublished papers on my hands. I would welcome a call from a sympathetic editor; winter's approaching and I don't have the zeal to stand on street corners handing out my tracts.

Reference

Barber, B., 1961, Resistance by scientists to scientific discovery: Science, v. 134, p. 596-602.

Accepted 16 November 1988