

Peer review: some further thoughts.

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Vacation post No 4. I will be out of the virtual office until Monday 31 August.

Peer review continues to cause concern, with widespread perceptions of unfairness. Although most of what I have noticed recently seems to be in the medical/public health communities, where one major gripe appears to be that established researchers have a significantly better chance of getting published. The current favourite response to this is to introduce double-blind refereeing, where you don't know who your referee is and they don't know who you are. Well, I can't see that working in turbulence and I doubt if there is any way that I could conceal my identity. In fact, that goes for anyone who publishes regularly in a field which does not have a lot of participants. So, in STEM subjects in general, that looks like a non-starter.

In any case, why shouldn't a researcher with a good track record of publication in their subject have a better chance of being published? Indeed, I would go further. I think that it should be part of the 'rules of the game' that there should be a presumption that a further publication on a topic should be published unless it is wrong in some way, or misleading, or quite definitely does not add anything to previous publications by that particular author. In other words, there should be an onus on the referee to demonstrate such faults.

I would actually go further and argue that, rather than introducing an additional layer of anonymity, we should remove the existing one. In my view, it would be helpful if referees had to put their name to their report. It should improve both fairness and (sometimes) courtesy. I should make it clear that I apply that opinion to everyone who referees and do not

exclude myself!

Naturally there will be those who will respond that if we remove anonymous refereeing, then the sky will fall in. I don't see why this should be. In my early years at Edinburgh, I did some work on turbulent diffusion in aerosol jets and this was published in the Journal of Aerosol Science. Their policy, at least at that time, was to have one referee who was expected to engage constructively with a submission and then to sign their report. My memory of it (rather vague now) was that it was a civilised and effective process. I also remember that the late Bob Kraichnan signed his referee reports and that was my experience on the few occasions that he refereed anything of mine.

And what about me? Well, I have dropped my anonymity on a number of occasions over the years, but only where I felt that it was particularly appropriate, for instance when my own work was being criticised. Apart from that, I have just been part of the flock! However, I seriously believe that the nature of refereeing in turbulence demands reform. My PhD supervisor described it as 'cut-throat' and at times it would be hard to disagree. Partly I think that this is due to the heterogeneous nature of the turbulence community, so that very often people are refereeing work that they are simply not able to understand.

I have yet further thoughts on this subject, which will be the subject of further posts. At the moment I am looking forward to a month's holiday from turbulence, so this is being written on the 30 July in order to be posted on the 27 August. On the 31 August I shall begin reading my email again.