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In earlier years I used to get the occasional phone call from George Batchelor, at that time the editor of Journal of Fluid Mechanics, asking for suggestions of new referees on the statistical theory of turbulence. To avoid confusion I should point out that by this I mean the theoretical physics approach to the statistical closure problem, pioneered by Bob Kraichnan and Sam Edwards, and carried on by myself and others. For anyone interested, a review of this subject can be found in reference [1] below.

I didn't find this easy, as there were then (as now) very few people working on this topic. My suggestion that Sam Edwards, although no longer active in this area, could certainly referee papers, was met with little enthusiasm. He was seen as 'too kind' or even as 'soft-hearted'! I wasn't surprised by this, as Sam had explained his position on refereeing to me and it amounted to: 'Unless it is arrant nonsense, it should be published.' In contrast, the refereeing process of the JFM was notoriously tough and this has been generally true in turbulence research, and remains so to this day. Indeed this is the general perception in the subject, and to quote Sam again, he once referred to 'the cut-throat nature of refereeing in turbulence'. I suspect it was this perception which put him off continuing in the subject.

I find myself somewhere between the extremes, perhaps because this is a matter of culture and I have been both engineer and physicist. However, while I respect the professionalism of the engineering approach, at the same time I think it can be taken too far. A typical experience for me (and I believe also for many others) is that a technical discussion can be carried on between the authors and individual referees which is never

seen by others in the field. In my view these discussions should be published as an appendix to the paper (assuming of course that the paper is actually accepted for publication). I also think that where the authors have a track record there should be a presumption that the paper should be published. In other words, the onus should be on the referee to come up with definite and reasoned objections, as opposed to the vague prejudiced waffle which is so often the case!

Another problem that arises often in the turbulence community, is the desire of some referees to rewrite the paper. Or rather to force the author(s) to rewrite the paper to the referee's prescription. It is of course legitimate to point out aspects which are less clear than they might be, but it verges on arrogance to tell the author how to do it. Also, with electronic publication now universal the idea of saving paper/printing costs is no longer so relevant. Papers can easily be as long as they need to be.

I have been on the receiving end of this behaviour on occasion, but nothing compared to something I was told recently; where a leading member of the community was forced to modify his paper four times despite his own judgement that the changes were unnecessary and his making protests to that effect to the editor. Someone else I know, summed it up as 'lazy editors and biased referees'. He had come from particle physics, where his papers had generally been published 'as submitted', to fluid mechanics (in the context of climatology) where there was invariably a battle over changes being required by the referee. Of course I trust that it is clear that I am not referring to the minor changes that we should all be happy to make, but to major structural changes which may in the end be no more than one person's opinion against another's. For these two individuals it was the failure by the editors to intervene that caused the problems.

So, it really comes down to the editor in the end. It is their job to protect their referees from unfair attack, on the one

hand; and to protect their authors from unfair refereeing, on the other. As I have pointed out elsewhere, in practice what breaks this symmetry is that it is more difficult for the editor to get referees than it is to get prospective authors; who, after all, are queuing up to apply!

[1] W. D. McComb and S. R. Yoffe. A formal derivation of the local energy transfer (LET) theory of homogeneous turbulence. J. Phys. A: Math. Theor., 50:375501, 2017.