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In 1985 I published a paper in JFM on laser-doppler measurements in drag-reducing fibre suspensions. This was the only paper on experimental work that I published in that journal and the refereeing process was not without interest. There was the usual iteration process and Referees A and B were fine, but Referee C was something else. His comments had a curious, slightly hysterical tinge, I felt. For instance, `Something is very far wrong here.' and `Conservation of energy is being violated here.' And others like that. Each attempt I made to reassure him, simply made matters worse. I should just mention in parenthesis that when you get a referee like this, they are impossible to reassure or satisfy. Editors need to be alive to this fact and in this case George Batchelor eventually said something to the effect `I'm afraid that C is being rather too suspicious and so I am going to disregard his reports.' In my view this was a perfect example of a good editor in action. He had ample evidence from A and B that the paper should be published and he took responsibility for having made an unlucky choice in C.

Some years later I was again having a paper reviewed by JFM and once again Referees A and B were fine, but this time C objected to the fact that the LET theory was being applied to isotropic turbulence. He said `there is far too much of this sort of work going on' and `the real problems are shear flows'. In response I argued that this work was physics and that, in comparison to condensed matter physics or particle physics, the amount of work on isotropic turbulence is very small and we really need a great deal more. Again, in parenthesis, this remains my opinion. Referee C responded by recommending rejection, and this time the editor (not

Batchelor!) said `well clearly C is an idiot and I'm going to ignore him'.

Actually this is all beginning to sound like it belongs in the story by the Canadian humourist Stephen Leacock `A, B and C: the human element in mathematics' in which he discusses problems in arithmetic of the type: `A, B and C are employed to dig a ditch. A can dig twice as fast as B and B can dig twice as fast C etc'. In his short story Leacock speculates about the three individuals and their interactions. He concludes that C always gets the dirty end of the stick and is a weak, undersized individual who dies young. Poor C!

So let us therefore turn to a bimodal form of refereeing, as practiced by the Physical Review. As I mentioned in my post of 25 June, when writing my book on HIT I found out that the coefficient E_2 in the Taylor expansion of the energy spectrum was identically zero. To my astonishment this appeared to be a new result, particularly in view of the ongoing controversy over `Saffman invariance *vs* Loitsianskii invariance'. After getting it independently checked, I wrote it up and submitted it to PRE. At the risk of spoiling the suspense, I should say that it was ultimately accepted for publication [1]. Nevertheless, the refereeing process had some remarkable features and raises some questions of interest.

First, Referees 1 and 2 replied. Referee 1 was positive and 2 was not. In fact their report was an incoherent rant which I found impossible to understand. I could manage to pick out phrases which I recognized as being points that are made about grid turbulence, but I was unable to discern anything relating to my paper. Moreover the entire report was in bold italic font, rather giving the impression of being what the police used to call `a green ink letter'.

So the Editor commissioned reports 3 and 4, one of which was favourable and the other was not. And then the Editor commissioned reports 5 and 6, one of which was favourable and the other was not. There was also a new development in that Referee 6 dragged in a recent disagreement between two different sets of investigators.

At this stage the Editor decided to reject my manuscript. This seemed to me to be `box ticking’ of the worst kind. Three for and three against, so let’s be on the safe side and reject it! Unlike in the two cases discussed above with JFM, there was no attempt to make a judgement of the relative quality of the referee reports. Naturally, I did not accept this. There followed a so-called arbitration, which was no such thing, and which I had no difficulty in shooting down. Then the Editor proposed a compromise. If I would add some material relating to the disagreement that Referee 6 had instanced, he would send it back to that referee. However, despite my adding material relating to that disagreement, Referee 6 did not change his extremely hostile attitude and recommended rejection. This time the Editor did what he should have done sooner and ignored this referee’s unbalanced report.

I should say that when I say Editor, I mean one of the associate editors of PRE at that time. Also, as PRE doesn’t come well out of this, I should mention a case where they did, and where (refreshingly!) the villains were not members of the turbulence community. I will keep this brief because I think this topic merits a post to itself. Basically I had done an analysis which showed that Galilean invariance did not suppress vertex renormalization in the NSE or similar equations which were of interest in soft condensed matter. Now unfortunately there was a substantial body of work in soft matter which relied very heavily on the supposition that it did, and not surprisingly my manuscript got a hostile reception. Any favourable reports were lukewarm (`might be of mild interest’) and the Editor turned the MS down.

I wrote to the Editor to say that I accepted his decision but wanted to point something out. If I was wrong, then not only were the `soft matter’ theorists better off as a result, but so also would I be, in that my LET theory would automatically be correct to fourth- rather than third-order in renormalized perturbation theory! The Editor suggested that I formally appeal against his decision, I did, and the arbitration was very much in my favour [2].

All four of these examples worked out satisfactorily, in my view, in that papers which should have been published were published. But they have worked out in different ways. In particular there is the question of should the editor pay attention to the quality of the reports? Let us bear in mind that editors are perhaps more reluctant to offend referees than authors. Also, when a number of referees are positive can that be cancelled out by a number being negative? I welcome comments on my posts and would particular welcome comments on these particular points.

[1] W. D. McComb. Infrared properties of the energy spectrum in freely decaying isotropic turbulence. Phys. Rev. E, 93:013103, 2016.
[2] W. D. McComb. Galilean invariance and vertex renormalization. Phys. Rev. E, 71:37301, 2005.