

Wavenumber Murder and other grisly tales

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When I was first at Edinburgh, I worked on developing a theory of turbulent drag reduction by additives. But, instead of considering polymers, I studied the much less well-known phenomenon involving macroscopic fibres. This was because it seemed to me that the fibres were probably of a length which was comparable to the size of the smallest turbulent eddies. It also seemed to me that the interaction between fibre and eddy would be two-dimensional and that it might be possible to formulate an explanation of turbulent drag reduction on mainly geometrical grounds. In particular, I had in mind that two-dimensional eddies could have a reverse cascade, with the energy being transferred from high wavenumbers to small. That is, the reverse (but *not* inverse) of the usual process. In this way drag might be reduced.

I derived a simple model for this process, and a letter describing it was published by *Nature Physical Science* in 1974. So far so good. Then I set to work writing the theory up in more detail and submitted it to the *JFM*. The results were not so good this time, and I had three referees' reports to consider. At least, George Batchelor did not feel the need to suppress any of the reports on the grounds of it being too offensive (someone I knew actually had this experience). But still, they were pretty bad.

No doubt this was salutary. I didn't dissent from the view that the paper should be rejected. In fact I dismantled it into several much better papers and got them published

elsewhere. But what sticks in my mind even yet is the referee who wrote: *'The author commits the usual wavenumber murder. Who knows what unphysical assumptions are being made under the cover of wavenumber space?'*

Well, that's for me to know and you to find out, perhaps! Of course, now that I am older (a lot) and wiser (a little), I realise that I could have played it better. I could have written up the use of Fourier methods, quoted Batchelor's book extensively, and thus made it very difficult for the referee to respond in that rather childish way. But why would that even occur to me? I was used at that stage to turbulence theorists who moved straight into wavenumber space without seeing any need to justify it. This is a cultural factor. Theoretical physicists are used to operating in momentum space which, give or take Planck's constant, is just wavenumber space in disguise. Anyway, at the time I was surprised and disappointed that the editor did not at least intervene on this particular point.

I actually found that referee's reaction quite shocking, but in one form or another I was to encounter it occasionally over the years, until at last it seemed to die out. Partially this could be attributed I would guess to the growth of DNS, with its dependence on spectral methods. Also, I think it could be due to better educated individuals becoming attracted to the study of turbulence.

Anyway, a few years ago, and just when I thought it was safe to mention spectral methods again, I made a big mistake. I had written (with three co-authors) a paper in which we used spectral methods to evaluate the exponents associated with real-space structure functions. It had been increasingly believed that the inertial-range exponents departed from the Kolmogorov (1941) forms, increasingly with both order and with Reynolds number, although it was actually realised that this could be attributed to systematic experimental error. So we had used a standard method of experimental physics to reduce

systematic error and found that the exponent for the second-order structure function in fact tended to the Kolmogorov canonical form, as the Reynolds number was increased. This is precisely the sort of result that merits a short communication and accordingly we submitted it as such. One of the referees was contumacious (and I may come back to him in later blog), the other was broadly favourable but seemed rather nervous about various points. However, when we had responded to his various points, he wanted one or two more changes and then he would recommend it for publication. At the same time, he commented that he really did wish that we hadn't used spectral methods.

This was where I made my big mistake. Overcome by kindly feelings towards this ref, and obeying my pedagogical instincts, I tried to re-assure him. I pointed out that he was quite happy with the pseudo-spectral method of DNS, in which the convolution sums in wavenumber space are evaluated more economically in real space and then transformed back into wavenumber. Now, I said, we are employing the same technique, but the other way round. We are evaluating the convolutions determining the structure function in real space, by going into wavenumber space. The response had a petulant tone. We were, he said, talking nonsense. The structure functions did not involve convolution integrals and he was rejecting the paper as mathematically unsound!

Later on we wrote up a longer version of the work and it was published: see #7 in the list of recent papers on this site. Appendix A is the place to look for the maths which bewildered the poor benighted referee. While accepting that this degree of detail was not given in the short communication, what is one to make of a referee who is unaware that a structure function can be expressed in terms of a correlation function and that the latter is a convolution integral?

Both referees were frightened of Fourier methods and between them almost seem to have bookended my career. But referees who

are comprehensively out of their depth have not been a rare phenomenon over the years. The forms which this inadequacy takes have been many and varied and I shall probably be dipping into my extensive rogues' gallery in future posts. There is also the question of the editor's role in finding referees who are actually qualified to referee a specific manuscript, and this too seems a fit subject for further enquiry. However, I should finish by pointing out that being on the receiving end of inadequate refereeing is not exclusively my problem.

In the first half of 1999, the Isaac Newton Institute held a workshop on turbulence. During the opening week, we saw famous name after famous name go up to the podium to give a talk, which almost invariably ended with *'and so I sent it off to Physica D instead'*. This last was received with understanding nods and smiles by an audience who were clearly familiar with the idea. This quite cheered me up, it seemed that I was not alone. At the same time, the sheer waste of time and energy involved seemed quite shocking. It prompted the thought: is it the turbulence community that is the problem, rather than the turbulence? That is something to consider further in future posts.