

Peer Review in Wonderland

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In 1974 I completed a task which had begun during my PhD days, and found a way of rendering the Edwards statistical closure compatible with the Kolmogorov spectrum. The basic idea was that the entire transfer spectrum $T(k)$ acted as a sink of energy at low wavenumbers and a source of energy at high wavenumbers. It could not, as in the Edwards theory, be divided into separate output and input terms that were valid at all k . This division, which is present in other theories, including DIA, would have seemed natural at the time, as it is characteristic of many equations of mathematical physics, such as the Boltzmann equation, the Pauli master equation, and the Chapman-Kolmogorov equation. But these equations are for Markov processes and turbulence transport in wavenumber is most emphatically not Markovian.

My key assumption was that the turbulence response was determined by a local (in wavenumber) energy balance and I called it the Local Energy Transfer (or LET) theory. It was limited to the single-time, stationary formulation, and a few years later I published a two-time extension of the theory which could be compared to Kraichnan's DIA. This analysis was somewhat heuristic and I have written in a previous post (see post for 16 July 2020) about its inadequacies and how they were cured over the years by phenomenological and heuristic methods. However, my long term ambition was to follow the self-consistent field methods of Sam Edwards and actually derive the LET for the two-time case from first principles.

It is a well-known truth that, as one gets older, it becomes more difficult to do mathematics. I'm not sure why this should be, but certainly as the years went on I was happy to entrust the detailed derivations to my students. Nevertheless, once I retired I felt that this was the moment to try. I had no commitments, apart from the visits to be made as part of my

Leverhulme travel fellowship, so I could proceed at a glacial pace to try to work out a theory. The result was a half-baked theory which I published in 2009, two and a half years after retirement. A lot more time passed, and in 2017 I published a paper which, although in some respects still a work in progress, does amount to a first-principles derivation of the LET. It is also a concise review of the topic and one of my colleagues said that I should have published it as a review article, in which case I would have escaped the hassle and also been paid some money. Well, if I had escaped the hassle, I wouldn't have had anything to write about in this post!

The first lot of hassle arose when I submitted it to JFM. This is where Sam published his 1964 paper and I thought it appropriate to do the same. But the JFM alas is not what it was when Sam published there nor indeed what it was over the years that I published a number of papers in it. Indeed, if memory serves, one of the referees said something to the effect that I had had more than my share of JFM papers and that appeared to be his main reason for rejection. For the moment I will pass over this episode. To do it justice I would have to publish the entire correspondence online. Whether or not I do that, there are some points to be made about Lagrangian versus Eulerian theories, so I will return to that topic in a later post.

The next step was that I rewrote the paper and submitted it to JPA [1], where it ultimately appeared. At the first hurdle there was the usual lukewarm result and, as JPA is a staff-edited journal, my manuscript was sent off to a member of the editorial board (EBM) for a decision. In passing, I should say that, while I also think that the time for anonymous refereeing has passed, I am strongly opposed to an EBM sheltering behind anonymity when giving a decision. It is in, my view, an impropriety.

Of course, it isn't necessarily very difficult to figure out who your anonymous EBM is; and naturally his field of interest

as well. In this case the EBM made a few vague remarks which indicated that he had probably not even troubled to read the paper. Then he said something like: 'I should have thought that a theory to explain the anomalous exponents of the higher-order moments was a more worthy problem.' That was apparently his grounds for rejecting the paper. I then appealed to the Editor-in-Chief, who made a careful assessment of the situation which was reflected in his detailed written statement in favour of publishing the manuscript. This was a scholarly decision which was fully justified by the subsequent interest in the paper. Within days of the paper being published, it had been downloaded several hundred times, and at time of writing the total number of downloads is over twelve hundred. That is a very large number of downloads when compared to recent papers on turbulence in JPA and perhaps when compared to any papers on turbulence.

So once again, we find ourselves in the *Alice in Wonderland* situation that is quite common in turbulence refereeing, where the normal rules don't seem to apply. The failing of my LET theory, so far as the EBM was concerned, is that it is compatible with the Kolmogorov spectrum and hence possibly incompatible with the existence of his particular problem. With regard to anomalous exponents, recent analysis using a standard technique of experimental physics to account for systematic error, indicates that this may be the main cause of so-called anomalous exponents [2]. I shall have more to say on this particular topic in a later post.

[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.

[2] W. D. McComb, S. R. Yoffe, M. F. Linkmann, and A. Berera. Spectral analysis of structure functions and their scaling exponents in forced isotropic turbulence. *Phys. Rev. E*, 90:053010, 2014.