

Intermittency corrections (sic) and the perversity of group think

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In *The Times* of 11 January this year, there was a report by their Science Editor which had the title *Expert's lonely 30-year quest for Alzheimer's cure offers new hope*. Senile dementia is the curse of the age (even if temporarily eclipsed by the Corona virus) and the article tells how in 1905 Alois Alzheimer made a post mortem examination of the brain of a woman who in her later years had become confused and forgetful. He found two pathological features: one consisted of clumps of plaques of a protein called beta amyloid and the other consisted of sticky tangles of a different protein, later identified by a Professor Claude Wischik as a protein called tau.

Now, with two possible causes, you might imagine that researchers in the field would be interested in both. But you would be wrong. It seems that the community targeted the beta amyloid cause and for many years neglected the other possibility. Now, after decades of failure, the major pharmaceutical companies are developing anti-tau drugs. Even if none of these proves to be the magic bullet, it seems a healthier situation that both symptoms (and the possible interaction between them) are being studied. The article ends on a note of moderate optimism, but the question remains: why was the research skewed towards just the one possibility? The article seems to suggest that this may have been because beta amyloid was already known and possibly implicated in another

pathology. As always, in applied research there is a temptation to go for the 'quick and dirty solution'!

The behaviour of the researchers pursuing the beta amyloid option (to the exclusion of the equally possible tau option) exhibits some of the characteristics of what psychologists call *group think*. A similar phenomenon has been part of fundamental research on turbulence for at least five decades. As is well known, it started with a remark by Landau about the Kolmogorov (1941) theory; or K41 for short. This criticism is based on the idea that intermittency of the dissipation rate has implications for the K41 theory, despite the fact that the physical basis of that theory is the inertial transfer rate, which is sometimes equal to the dissipation rate. This criticism, along with various others, is discussed in Chapters 4 and 6 of my 2014 book on turbulence and I will not consider it further here. All I wish to note is that there has been an ongoing body of work on so-called intermittency corrections, and the strange thing is that more obvious corrections have been largely neglected, until quite recent times. Let us now expand on that.

Essentially Kolmogorov used Richardson's concept of the cascade to argue that energy transfer would proceed by a stepwise process from large scales (production range) to small scales and this would result in a universal form for the structure functions in these small scales. Furthermore, for large Reynolds numbers, the effect of the viscosity would only be appreciable at very small scales, and there would be an intermediate subrange of scales where the local excitation would be controlled by inertial transfer into the subrange from the large scales and inertial transfer out of the subrange into the small scales where it would be dissipated by viscous effects.

At this point, I should enter a small caveat. I feel quite uncomfortable with what I have just written. The physical concept of the cascade is rather ill-defined in real space. I

would be much happier talking in terms of wavenumber space where the cascade is well defined and the key concept is scale-invariance of the inertial flux. This fact was recognized by Obukhov (1941), by Onsager (1945) and by Batchelor (1947), and after that very widely. It is rather as if Kolmogorov, in choosing to work in real space, had opted for Betamax rather than VHS!

However, ignoring my quibbles, in either space one point is clear: this is an approximate theory. Either $S_2 \sim \nu^{2/3} r^{2/3}$ or $E(k) \sim \nu^{2/3} k^{-5/3}$ is *only asymptotically valid in the limit of infinite Reynolds numbers*. Under all other circumstances, there must be corrections due to finite-Reynolds number (FRN) effects. These corrections may be small enough to ignore: bear in mind that on various measures an infinite Reynolds number is not all that large. There is certainly no need to worry about zero viscosity (*pace*) Onsager and his hagiographers! We shall return to this specific point in later posts.

The response of Kolmogorov to Landau's criticism was the somewhat *ad hoc* K62, in which the retention of the specific effect of the large scales of the system (in both structure functions and spectra), completely reversed the original assumption of the stepwise cascade leading to universal behaviour. For reasons that are far from clear to me, this sparked off a positive industry of intermittency corrections, anomalous exponents and various improvements (*sic*) on Kolmogorov, which lasts to this day. In contrast, from the late 1990s, increasing attention, both experimental and theoretical, has been given to FRN effects, and in particular the way in which they have been ignored in assessing the evidence for anomalous exponents and suchlike. We may highlight the situation in the field by contrasting two major papers, both published in leading learned journals within the last year.

The first of these is by Tang *et al* [1], who note in their abstract that K62 'has been embraced by an overwhelming majority of turbulence researchers.' This paper is one in a series in which this group has investigated the alternative effect of finite Reynolds number corrections. In addition to their own analysis, they also cite many papers from recent years which support their conclusion that the failure to account for FRN effects has 'almost invariably been mistaken for the intermittency effect'. In the main body of their paper, they express themselves even more forcibly. In contrast, the paper by Dubrulle [2], which is very much in the K62 camp, so to speak, cites not a single reference to FRN effects. Instead the author argues that small-scale intermittency is incompatible with homogeneity, and makes the radical proposal that the Karman-Howarth equation should be replaced by a weak form which takes account of singularities. At this point one takes leave of continuum mechanics and much else besides! If we consult Batchelor's book, we find that homogeneity is defined in terms of mean quantities and is therefore entirely compatible with intermittency of the velocity field, which is nowadays understood to be present at all scales.

I was tempted to say that it is difficult to imagine such a fundamental gulf in any subject other than turbulence, but then that's where we came in!

[1] S. Tang, R. A. Antonia, L. Djenidi, and Y. Zhou. *Phys .Rev. Fluids* **4**, 024607 (2019).

[2] B. Dubrulle. *J. Fluid Mech.* **867**, P1, (2019).