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An intriguing aspect of the Kolmogorov inertial range spectrum is that it was not actually derived by Kolmogorov. This fact was unknown to me when, as a new postgraduate student, I first encountered the $5/3$ spectrum in 1966. At that time, all work on the statistical theory of turbulence was in spectral or wavenumber (k) space, and the Kolmogorov form was seen as playing an important part in deciding between alternative theoretical approaches.

As is well known nowadays, in 1941 Kolmogorov derived power-law forms for the second- and third-order structure functions in r space. In the same year, it was Obukhov [1] who worked in k space, introducing the energy flux through wavenumber as the spectral realization of the Richardson-Kolmogorov cascade, and making the all-important identification of the scale-invariance of the energy flux as corresponding to the Kolmogorov picture for real space. It is usual nowadays to denote this quantity by $\Pi(k)$, and in this context scale-invariance means that it becomes a constant, independent of k . For stationary turbulence that constant is the dissipation rate. Obukhov did actually produce the $5/3$ law, but this involved additional hypotheses about the form of an effective viscosity, so it was left to Onsager in 1945 [2] to combine simple dimensional analysis with the assumption of scale-invariance of the flux to produce a spectral form on equal terms with Kolmogorov's $2/3$ law for $S_2(r)$. This work was discussed (and in effect) disseminated by Batchelor in 1947 [3], and later in his well-known monograph. Curiously

enough, in his book, Batchelor only discussed the spectral picture, having discussed only the real-space picture in [3]. This is something that we shall return to in later posts. But it seems that the effect was to establish the dominance of the spectral picture for many years.

In the early sixties, there was considerable excitement about the new statistical theories of turbulence, but when Grant, Stewart and Moilliet published their experimental results for spectra, which extended over many decades of wavenumber, it became clear beyond doubt that the Kolmogorov inertial-range form was valid and that the theories of Kraichnan and Edwards were not quite correct. We will write about this separately in other posts, but for me in 1966 the challenge was to produce an amended form of the Edwards theory which would be compatible with the ' $5/3$ ' spectrum. This, in other words, was a restatement of the turbulence closure problem. It is one that I have worked on ever since.

This is not an easy problem and progress has been slow. But there *has* been progress, culminating in McComb & Yoffe (2015): see #3 of my recent publications. However, over the years, beginning in the late 1970s, this work has increasingly received referee reports which are hostile to the very activity and which assert that the basic problem for closures is *not* to obtain $k^{-5/3}$ but rather to obtain a value for μ , where the exponent should be $-5/3 + \mu$, due to intermittency corrections. Unfortunately for this point of view, the so-called intermittency correction μ comes attached to a factor L , representing the physical size of the system. This means that the limit $L \rightarrow \infty$ does not exist, which is something of a snag for the modified Kolmogorov theory.

We shall enlarge on this elsewhere. For the moment it is interesting to note that the enthusiasm for intermittency corrections arose from the study of structure functions and in particular their behaviour with increasing order. This became

a very popular field of research throughout the 1980s/90s and threatened to establish a sort of *standard model*, from which no one was permitted to dissent. Fortunately, there has been a fight back over the last decade or two, and the importance of *finite Reynolds number effects* (or FRN) is becoming established. In particular, the group consisting of Antonia and co-workers has emphasised consistently (and in my view correctly) that the Kolmogorov result $S_3 \sim (4/5)r$ (which the *Intermittentists* regard as exact) is only correct in the limit of infinite Reynolds numbers. At finite viscosities there must be a correction, however small. A similar conclusion has been reached for the second-order structure function by McComb *et al* (2014), who used a method for reducing systematic errors to show that this exponent too tended to the canonical value in the limit of infinite Reynolds numbers. These facts have severe consequences for the way in which the *Intermittentists* analyse their data and draw their conclusions.

This leaves us with an interesting point about the difference between real space and wavenumber space. The above comments are true for structure functions, because in r -space everything is local. In contrast, the nonlinear energy transfers in k -space are highly nonlocal. The dominant feature in wavenumber space is the flux of energy through the modes, from low wavenumbers to high. The Kolmogorov picture involves the onset of scale invariance at a critical Reynolds number, and the increasing extent of the associated inertial range of wavenumbers as the Reynolds number increases. The infinite Reynolds number limit in k -space then corresponds to the inertial range being of infinite extent. At finite Reynolds numbers, it will be of merely finite extent, but there is no reason to believe that there is any other finite Reynolds number correction. I believe that this is more than just a conjecture.

[1]A. M. Obukhov. On the distribution of energy in the

spectrum of turbulent flow. C.R. Acad. Sci. U.R.S.S, 32:19, 1941.

[2] L. Onsager. The Distribution of Energy in Turbulence. Phys. Rev., 68:281, 1945.

[3] G. K. Batchelor. Kolmogorov's theory of locally isotropic turbulence. Proc. Camb. Philos. Soc., 43:533, 1947.