My life in wavenumber space

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In September 1966, when I began work on my PhD, I almost immediately began to dwell in wavenumber space. After a brief nod to the real-space equations, I had to learn about Fourier transformation of the velocity field, with the wave-vector $\mathbf{k}$ replacing the position vector $\mathbf{x}$, and the Navier-Stokes equations being changed from real space to wavenumber space. In addition, it was usual in those days to begin with the velocity field in a cubic box and use Fourier series. Then at some stage one would let the box size tend to infinity, and replace summations by integrals. At the same time, the periodic boundary conditions would be replaced by good behaviour at infinity. So far as theoretical work was concerned, I was not to emerge from wavenumber space until around 2006, when I began to take an interest in the phenomenology of turbulence.

This narrowness was not unusual and indeed did not seem particularly narrow at the time. There had been an incursion of theoretical physicists into turbulence from the 1950s onwards; and, for theorists of the time, wavenumber space was just momentum space with Planck’s constant set equal to unity. So everyone working on the statistical theory of turbulence was quite at home in wavenumber space, and it fitted in with what was almost a tradition in turbulence theory, which had begun with Taylor’s introduction of spectral methods in the 1930s and had been carried on in the 1950s by Batchelor’s book in particular. Problems only arose when one’s papers were refereed by those who were not part of this grouping, and who were hostile to spectral methods. But I have written about that in other blogs and it is not what concerns me here, which is something rather more subtle.

The other day I was trying to work something out and was sure that I had done it previously. I’m not keen on doing anything that I, or indeed anyone else, has already done. Hence I was checking back in my notebooks and found what I was looking for dated May 1993. So, that was satisfactory, but it reminded me of why I had done the work originally. During the 1970s/80s, I became increasingly aware of referees who felt that theories predicting the Kolmogorov $-5/3$ law should not be published, because ‘intermittency corrections meant that it wasn’t correct’. It seemed to me that the very structure of renormalization theories was evidence for the correctness of the $-5/3$ law. But as such theories were very largely inaccessible to fluid dynamicists (especially, of course, when they were refereeing them!) I had wondered how one could extract the basic ideas without the full level of complication.

The essential feature, it seemed to me, was the occurrence of scale invariance, in which the inertial flux through wavenumber became constant independent of wavenumber. Beginning with the velocity field in k-space, one could exploit its complex nature to separate out amplitude and phase effects. Then, in the context of the energy balance equation (nowadays increasingly referred to as the Lin equation), one could determine the energy spectrum by power counting; with its prefactor being determined by an average over the phases.

I wrote this up and submitted it to *PRL* sometime in 1993. The response was interesting. It was rejected with a report that spoke approvingly of how it was written and presented but regretted that the energy-balance equation had already been used to derive the so-called ‘$4/5$’ law for the third-order structure function by Kolmogorov. I of course was happily ignorant of this. It was something done in real space. Which demonstrates the disadvantages of taking too limited or narrow an approach.

In 2006 I retired and began to take an interest in various phenomenological questions. This meant that at last I crossed over into real space and worked with the Kármán-Howarth equation as well as with the Lin equation. When working on the scale-invariance paradox, I decided to revisit my 1993 theory and this was published as [1] below. I was now able to point out that it answered the Landau criticism of Kolmogorov’s theory (as reinterpreted by Kraichnan [2]), in that its prefactor also depended on an average to the two-thirds power. If the original referee had been more familiar with spectral methods, he might have realised that my paper was a derivation of the inertial-range energy spectrum from the equations of motion, not the Fourier transform of the third-order structure function. So it was very much a different result from the Kolmogorov ‘$4/5$’ law. It also occurs to me as I write this, that the relationship between prefactors in the real-space and wavenumber-space formulations might be worth looking at.

Is there a moral in all this? I think there is. Basing my opinion on long experience of papers, discussions and referee reports, I believe that those fluid dynamicists who are uncomfortable with spectral methods understand less about the basic physics of turbulence than they otherwise might… and the New Year is a time for resolutions!

[1] David McComb. Scale-invariance and the inertial-range spectrum in three-dimensional stationary, isotropic turbulence. J. Phys. A: Math. Theor., 42:125501, 2009.
[2] R. H. Kraichnan. On Kolmogorov’s inertial-range theories. J. Fluid Mech., 62:305, 1974.