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Recently I came across the article by Lumley and Yaglom which is cited below as [1]. I think it is new to me but quite possibly I will find it lurking in my filing system when at last I am able to visit my university office again. It is always good to get something gossipy and opinionated to read about turbulence as a welcome relief from all the worthy but demanding research papers! In any case, their Abstract is well worth quoting here:

*'This field does not appear to have a pyramidal structure, like the best of physics. We have very few great hypotheses. Most of our experiments are exploratory experiments. What does this mean?'*

They go on to answer their own question: *'We believe it means that, even after 100 years, turbulence studies are still in their infancy.'*

I'm not quite sure what is meant by the phrase *'pyramidal structure'*, but overall the general sense is clear; and really quite persuasive. Indeed, even after a further two decades, which have been marked by an explosive growth in research, this depressing view is still to a considerable extent justified. However, I think that it might be of interest to consider in what ways it is justified and in which ways the comparison with physics may be unfair.

There are of course the unresolved issues of fundamental turbulence theory, but what is more compelling in my view, is the bizarre and muddled nature of some key aspects of the subject. To begin with, there is the Kolmogorov spectrum. Nowadays it is probably well known that Kolmogorov worked in real space and derived the  $k^{-2/3}$  law, from which the  $k^{-5/3}$  spectrum of course follows by Fourier transformation. Yet

beginning with Batchelor's monograph [2], and for decades thereafter, discussion of the subject was entirely in terms of wavenumber space. A particularly egregious example arises in the book by Hinze [3]. After acknowledging [2], he writes: '*These considerations have led Kolmogoroff (sic) to make the following hypothesis.*' He then goes on to state the hypothesis (top of page 184 in the first edition) and expresses it in terms of wavenumber. As his statement of the hypothesis is in inverted commas, I assumed that it was a quotation from Kolmogorov's paper [4], but Kolmogorov nowhere uses the word 'wavenumber' in that paper!

This is not in itself a serious matter. But it is symptomatic, and the fact remains that various commentators rely on a real-space treatment to draw conclusions about spectra. For me, the truly astonishing fact is that I have been unable to find an exegesis of Kolmogorov's original paper anywhere. All treatments are brief and superficial, in contrast to his later paper [5] in which he derived the  $4/5$  law. This of course has been widely reviewed and discussed in detail. Which is perhaps not unconnected with the fact that it is very much easier to understand!

There are other schools of thought that one can point to, where the real problem is a failure to realise that the ideas being put forward are unphysical. For instance, the uncritical adoption of Onsager's pioneering work in which the viscosity is put equal to zero instead of taking the limit of zero viscosity. The result is the unphysical idea of dissipation taking place in the absence of viscosity, which of course it cannot. Absorption of energy by an infinite wavenumber space is not the same as viscous dissipation. At best it might be described as *pseudo dissipation*. Further discussion of this topic can be found in reference [6].

To round this off, there is Kolmogorov's 1962 paper, presenting what he described as '*a refinement of previous hypotheses*'. In fact, as is increasingly recognised, it is

nothing of the sort. It is instead the wholesale abandonment of previous hypotheses. But I have said that elsewhere. What concerns me here is that the theory is manifestly unphysical. The energy spectrum is (in thermodynamic terms) an intensive quantity. Thus the factor  $L^{\{\mu\}}$  which is now incorporated into the power-law form violates the requirement that it should not depend on the size of the system. In the limit of infinite system size, the energy spectrum must now go to zero if the exponent is negative and to infinity if it is positive. Curiously, no one seems to have commented on this.

Lumley and Yaglom were referring to the problem of achieving a fundamental understanding of turbulence and it is perhaps worth keeping in mind that the great success of physics is based on the happy accident of linearity. On purely taxonomic grounds, turbulence belongs to the class of many-body problems with strong coupling. These are just as intractable in nuclear physics, particle physics, and condensed matter physics as in fluid turbulence. The difference is that these activities are generally pursued in a more scholarly way, with a more collegial atmosphere among the participants. As a previous generation used to say: *verb. sap!*

[1] J. L. Lumley and A. M. Yaglom. A Century of Turbulence. Flow, Turbulence and Combustion, 66:241, 2001.

[2] G. K. Batchelor. The theory of homogeneous turbulence. Cambridge University Press, Cambridge, 1st edition, 1953.

[3] J. O. Hinze. Turbulence. McGraw-Hill, New York, 1st edition, 1959.

[4] A. N. Kolmogorov. The local structure of turbulence in incompressible viscous fluid for very large Reynolds numbers. C. R. Acad. Sci. URSS, 30:301, 1941.

[5] A. N. Kolmogorov. Dissipation of energy in locally isotropic turbulence. C. R. Acad. Sci. URSS, 32:16, 1941.

[6] W. D. McComb and S. R. Yoffe. The infinite Reynolds number limit and the quasi-dissipative anomaly. arXiv:2012.05614v2[physics.flu-dyn], 2021.