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When I was first at Edinburgh, in the early 1970s, a number of samizdat-like documents, of entirely mysterious provenance, were being passed around. One that came my way, was a paper by Lumley which contained some rather interesting ideas for treating the problem of turbulent diffusion. I expect that it is still in my filing system; but, with the Covid-19 lockdown, I am cut off from my university office and unable to refresh my memory. Later on I encountered the paper by Proudman which criticised Kraichnan’s theory of turbulence – the Direct-Interaction Approximation – and by that time I presumably had heard about the meeting held in Marseille in 1961. Of course my ignorance is not all that surprising, in that the meeting, which was the source of these papers, took place five years before I began my postgraduate research. In any case, I must have known about it by the late 1980s, as these papers are correctly referenced in my 1990 book on the physics of turbulence.

An interesting and informal account of this meeting is given by Moffatt in his review [1], which is essentially an appreciation of the life and work of G. K. Batchelor, and accordingly the meeting is seen, as it were, through this prism. Having told the story of how Batchelor discovered the work of Kolmogorov, while searching through the literature of turbulence in the library of the Cambridge Philosophical Society; and how he had expanded the short and rather cryptic papers of Kolmogorov into what was to become a seminal work on the subject [2], Moffatt sees the Marseille meeting as a ‘watershed’ in the study of turbulence. In support of this, he highlights two contributions to the meeting.

First, there is the report by Stewart of experimental measurements of energy spectra carried out in the channel between Vancouver Island and the mainland. This investigation achieved values of the Taylor-Reynolds number up to about 3000, and several decades of power-law behaviour, which appeared to support the Kolmogorov $-5/3$ spectrum. This work was published the following year [3].

Secondly, there was a lecture by Kolmogorov, also published in the following year [4], in which he outlined a refinement (*sic*) of his 1941 theory in response to a criticism by Landau. His conclusion was that the power of $-5/3$ should be subject to a small correction μ; but he was unable to obtain a value for μ.
There is an element of contradiction here, but that could possibly be resolved quite trivially if one were to find out that the two agreed within experimental error. So that in itself is not a paradox. The paradox that I have in mind arises in a different way.

Moffatt discusses the fact that Batchelor essentially gave up turbulence as his main research interest after this meeting. His argument appears to be that Batchelor was already becoming discouraged by the difficulties of the subject. And, given that a major part of his own research had been the interpretation and dissemination of the Kolmogorov (1941) theory, he may have found that Kolmogorov's lecture at this meeting came as the last straw!

Another possibility, that Moffatt doesn't mention, is that Batcheleor may have found the new wave of theoretical physics approaches, as initiated by Kraichnan, not only complicated but also part of an alien culture, to the extent that this too was discouraging. I have a personal note that I can add here. I only met Batchelor once; in 1967 when he examined my Master's thesis. At one point he had some difficulty with the units, where I was giving a quantum physics analogy, and I pointed out that there would be a Planck's constant involved, but that I was working in units where Planck's constant was unity. At another stage *he* pointed out that he was, at the risk of being accused of cynicism, no more optimistic about these new quantum-inspired approaches, than about anything else. And, that was with Sam Edwards, who had published a theory of turbulence in JFM three years earlier, also in the room! I am quite sure that forty (or more) years on, there would be many in turbulence research who would eagerly say that he had proved to be right. But, following one's prejudices, rather than engaging with a subject, is the abnegation of scholarship. Sometimes the truth lies deep.

However, another major discouragement took place at this meeting. Kraichnan was predicting an inertial-range spectrum with an exponent of $-3/2$. Even if the results of Grant *et al.* [3] were compatible with a small correction to $5/3$, they were certainly good enough to convincingly rule out Kraichnan's rival $3/2$ exponent. As a result, Kraichnan had to look at his theory again, and over a period of several years he became convinced that the problem was insoluble in Eulerian coordinates, and that there was a need to change to a mixed coordinate system which he called Lagrangian-History coordinates. The result was an immensely complicated theory, which not only had to be abridged in order to permit computation, but also depended on the way in which the theory was formulated. This has left a legacy of other workers who employ a more conventional Lagrangian system.

This, then, is the paradox that I had in mind. The outcome of the meeting, put in very broad brush terms, is that Batchelor changed his mind because Kolmogorov (1941) was wrong and Kraichnan changed his mind because it was correct. It cannot be said that progress in turbulence is ever smooth.

[1] H. K. Moffatt. G. K. Batchelor and the Homogenization of Turbulence. Ann. Rev. Fluid Mech., 34:19-35, 2002.
[2] G. K. Batchelor. Kolmogorov's theory of locally isotropic turbulence. Proc. Camb. Philos. Soc., 43:533, 1947.
[3] H. L. Grant, R. W. Stewart, and A. Moilliet. Turbulence

spectra from a tidal channel. J. Fluid Mech., 12:241-268, 1962.
[4] A. N. Kolmogorov. A refinement of previous hypotheses concerning the local structure of turbulence in a viscous incompressible fluid at high Reynolds number. J. Fluid Mech., 13:82-85, 1962.