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In 1969 I published my first paper [1], jointly with my supervisor Sam Edwards, in which we maximised the turbulent entropy, defined in terms of the information content, in order to obtain a prescription for $\omega(k)$, the renormalized decay time for the energy contained in the mode with wavenumber k. Of course, in statistical mechanics, one associates the maximum of the entropy with thermal equilibrium. So, in the circumstances, we were very frank about possible problems with this approach, having actually stated in the title that our system was ‘far from equilibrium’. Before we examine this aspect further, it may be of interest to look at the background to the work.

By the mid-nineteen sixties, there had been a number of related theories of turbulence, but the most important were probably Kraichnan’s direct-interaction approximation (DIA) in 1959 and the Edwards self-consistent field theory in 1964. At this time there seems to have been a mixture of excitement and frustration. It had become clear from experiment that the Kolmogorov $-5/3$ power law (or something very close to it) was the correct inertial-range form, and none of the various theories was compatible with it. Kraichnan ultimately concluded that he needed to change to a so-called Lagrangian-history coordinatate system, but otherwise could retain all the features of the DIA; whereas Edwards concluded that he needed to find a different way of choosing the response function, which in his case depended on $\omega(k)$. In my view, and irrespective of the merits or otherwise of the ‘maximum entropy’ method, Edwards made the right decision.

When I began my PhD research in 1966, my first job was to work out the turbulent entropy, using Shannon’s definition, in terms of the turbulent probability distribution; and then carry out a functional differentiation with respect to $\omega(k)$, in order to establish the presence of a maximum. What I didn't know, was that Sam had himself carried out this calculation but had got stuck. In order to take the limit of infinite Reynolds numbers, he had to show that his theory was well behaved at three particular points in wavenumber space: $k=0$, $k=\infty$ and $|\mathbf{k}+\mathbf{j}|=0$, where j is a dummy wavenumber. He had been able to show the first two, but not the third. Not knowing that there was a problem, I soon discovered it, but by means of a trick involving dividing up the range of integration, I managed to show that it was well behaved. However, the prediction of the value of the Kolmogorov constant was not good, and this was not encouraging.

In later years, when I had a lot more experience of both turbulence and statistical physics, I thought more critically about this way of treating turbulence. The maximum entropy method is the canonical way of solving problems in thermal equilibrium where there are only either weak or very local interactions. If we take the para-ferromagnetic transition as an example, we can think of the temperature being reduced and an assembly of molecular magnets (i.e. spins on a lattice) tending to line up as the effective coupling increases. However, this process would be swamped by the imposition of a powerful external magnetic field. Similarly, the molecular diffusion process can be swamped by vigorous stirring. In the case of turbulence, it is possible to study absolute equilibrium ensembles by considering an initially stirred inviscid fluid in a finite system. If we replace the Euler equation by the Navier-Stokes equation, then the effect of the viscosity is symmetry-breaking and the system is dominated by a flow of energy through the modes.

This, of course is a truism of statistical physics: a system is either controlled by entropy or energy conservation. In the case of turbulence, it is always the latter. Turbulence is always a *driven* phenomenon. So while perhaps entropy is actually a maximum with respect to variation of $\omega(k)$, it may be too broad a maximum allow an accurate determination of $\omega(k)$. Also, it is worth bearing in mind, that it is not precisely turbulence but the statistical theory we are approximating it by, which needs to show the requisite behaviour.

In any case, in 1974 I published my local energy transfer theory of turbulence [2], which is in good accord with the basic physics of the turbulent cascade.

[1] S. F. Edwards and W. D. McComb. Statistical mechanics far from equilibrium. J.Phys.A, 2:157, 1969.
[2] W. D. McComb. A local energy transfer theory of isotropic turbulence. J.Phys.A, 7(5):632, 1974.