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In the previous post we discussed the introduction of Kraichnan’s DIA, based on a combination of a mean-field assumption and a new kind of perturbation theory, and how it was supported by Wyld’s formalism, itself based on a conventional perturbation expansion of the NSE. This was not too surprising, as Kraichnan’s mean field assumption involved his infinitesimal response function which the Wyld comparison showed was the same as the viscous response function, and hence not a random variable. By 1961 it was known that the asymptotic solution of DIA was incorrect, with implications for both the Wyld formalism (and the MSR formalism later on: see previous post).

The next step forward was the theory of Edwards [1] in 1964, which was restricted to the more limited single-time covariance and also to the stationary case. This took as its starting point the Liouville equation for P, the probability distribution functional of the velocity field, and went beyond the mean-field case to calculate corrections to it self-consistently. That is, Edwards made the substitution $P\equiv P_0 + (P –P_0)$ and then expanded in powers of the correction term $\Delta P = P – P_0$. Then, taking P_0 to be Gaussian, and exploiting the symmetries of the system, Edwards gave a highly intuitive treatment of the problem, in which he drew strongly on an analogy with the theory of Brownian motion. It turned out that the resulting theory was closely related to the DIA and, like it, did not agree with the Kolmogorov spectrum.

The following year Herring [2], using formal methods of many-body theory, produced a self-consistent field theory which was much more abstract than the Edwards one, but yielded the same energy equation. Then, in 1966 he generalised this theory to the two-time case [3]. All three theories [1-3] led to the same energy equation as DIA, but all differed in the form of the response equation.

Now, it is in the introduction of the response equation that the renormalization takes place, and it is in the form of the response equation that the deviation from Kolmogorov lies, so this difference between these response equations raises fundamental questions about all these theories. Various interpretations were offered at the time, but these were all phenomenological in character. It was much later that a uniform, fundamental diagnosis was offered and I will come on to that presently. But this was the situation when I began post-graduate research with Sam Edwards in October 1966. The exciting developments of the previous decade seemed to be leading to a dead end, and my first task was to choose the response function of the Edwards theory in a new way, such that it maximised the turbulent entropy [4].

On the basis of the Edwards analysis, his theory had failed under the extreme circumstances of an infinite Reynolds number limit, in which the input was modelled by a delta-function at the origin in k-space and the dissipation was represented by a delta-function at $k=\infty$. Edwards argued that under these circumstances the Kolmogorov spectrum would apply at all wavenumbers, and in his original theory this led to an infra-red divergence in the integral for the response function. (Note: Kraichnan used the scale-invariance of the inertial flux Π as his criterion for the inertial range, but the two methods are mathematically equivalent.) The `maximum entropy' theory [4] certainly achieved the result of eliminating the infra-red divergence, but that was about as much as one could say for it. It became clearer to me later that it was not a very sound approach.

It is a truism in statistical physics that a system is either dominated by entropy or energy. If we consider a system made of many microscopic magnets on a lattice then the entropy will determine the distribution. However if we switch on a powerful external magnetic field, all the little magnets will line up with it and (small fluctuations aside) entropy has no say in the matter! It is just like that in turbulence. The system is dominated by a symmetry breaking current of energy through the modes, running from small to large wavenumbers, where it is dissipated by viscosity. There is no real reason to assume that the entropy determines the turbulence response.

When I was in my first post-doctoral job, I gave a talk to some theorists. I explained my early ideas on how energy transfer might determine the turbulence response. They heard me out politely, and then I made the mistake of mentioning the maximum entropy work. Immediately they became enthusiastic. ‘Tell us about that’, they said. The impression they gave was ‘now that’s a real theory!’ I was in awe of them as they were much older and more experienced than me, and talked so authoritatively about all aspects of theoretical physics. Nevertheless, this was my first inkling of conventional thinking. The implication seemed to be: it was a text-book method, so it must be good.

Over the next few years I developed the local energy transfer (LET) theory [5, 6], and also offered a unified explanation of the failure of first-generation renormalized perturbation theories. The further extension of this work to the two-time case has had a rather chequered history and will be the subject of further posts.

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