Local Energy Transfer (LET): a curate’s egg theory?

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The LET theory began well as a modification to the Edwards theory [1,2], which was a single-time theory, and then underwent a rather heuristic extension to two-time form to become in effect a modification of Kraichnan’s DIA theory [3]. It was successfully computed for freely decaying turbulence in subsequent years and in one of these papers its derivation was put on a better footing [4]. This work was later formalised [5], and more recently the theory has been formally derived by applying the Edwards self-consistent field method to the full two-time pdf [6]. As the resulting set of equations for the two-time correlation and response functions is a fully Eulerian theory which gives good results, both quantitative and qualitative, I thought there might be some interest in a simple outline of the twists and turns in its evolution!

In 1966 when I began my postgraduate studies, the problem with both the Edwards theory and DIA was that they were incompatible with the observed $k^{-5/3}$ energy spectrum. It was 1974 before I saw what was wrong with the Edwards theory (and by extension DIA) was that the inertial transfer spectrum (usually denoted by $T(k)$ in the notation of the Lin equation) was divided into two parts, a diffusive term and a dissipative term which was proportional to the amount of energy in mode k. Now this is a form which crops up in physics, for example the Boltzmann equation, the Fermi master equation, and the Fokker-Planck equation, so is must have seemed quite natural. However, the first measurements of $T(k)$ were reported in 1963, and after that it became obvious that the entire term $T(k)$ was either input or output, depending on the value of the labelling wavenumber k. This was what I finally managed to see in 1974 and so I proposed that the turbulent response in the Edwards case was determined by a local (in wavenumber) energy balance involving the whole of $T(k)$ [1,2].

Extending this idea to Kraichnan’s two-time theory presented a far from trivial problem. My intuitive feeling was that the idea of determining the system response in terms of the relationship between stirring forces and the resulting velocity field should be abandoned and instead I decided to base my approach on the introduction of a velocity field propagator. I argued that in perturbation theory we would have at zero order a relationship: \begin{equation} u^0(k,t) = R^0(k,t-s) u^0(k,s). \end{equation} Note that this is in an updated notation, with R standing for response function, and that it is simplified with tensor indices being omitted, and we have assumed stationarity. Corresponding to some renormalization of the perturbation series I then proposed the introduction of an exact propagator R, such that: \begin{equation} u(k,t) = R(k,t-s) u(k,s). \end{equation} This allowed me to derive equations for the correlation function $C(k;t-t’)$ and the response function $R(k;t-t’)$. These were identical to those of Kraichnan’s DIA apart from the presence of an additional term in the response equation. This additional term had, of course, the crucial effect of making the response equation compatible with the $-5/3$ spectrum.

When the paper was submitted for publication it ran into trouble with the referees. One of them was worried by the fact that sometimes R was treated as if statistically sharp and at others as if it were not. I couldn’t understand that, but I added a footnote to say that the response function was statistically sharp. The other referee conceded that LET should do better than DIA at high Reynolds number, but reckoned that DIA would be better at low Reynolds numbers and so publication should await numerical calculations! I was quite fascinated by this report. It put me in mind of the comedy routine of early films where some luckless person tries to pack an overfull suitcase. He pushes in a shirt collar at one corner and snaps the lid closed, only to notice that a tie is peeping out at another corner. So he struggles to push that in, again snaps the suitcase closed only to see that a sock is sticking out at another corner. And so it goes on. Perhaps that was `the packing a suitcase’ method of assessing a theory?

A few years later, we published the numerical calculations and it turned out that the LET was actually better than DIA at all Reynolds numbers. It also turned out that DIA was not as bad at high Reynolds numbers as had been expected. The referees for the paper were Jack Herring and Bob Kraichnan, and I remember Batchelor telling me that I had `stirred them up quite a bit’ and that they would like to contact me directly. I recall that we had some very interesting and amicable discussions by letter: email was still in its infancy!

Equation (2) is open to some serious criticism and we should now consider what is wrong with it. Essentially it implies a fixed phase relationship between two realisations of the velocity field at different times, when there is no reason to suppose that such a relationship can exist in a mixing system like fluid turbulence. Another way to look at this is to rewrite (2) such that R is defined as the ratio of the two velocities, and we immediately see that we should have $\hat{R}$: a random variable. Now to replace $\hat{R}$ by R would be a mean-field approximation (there is an equivalent step in the derivation of DIA) but that can only be done in the context of some averaging operation. This was introduced in [4] where the basic hypothesis underlying LET was taken to be: \begin{equation} C(k;t,t’) = R(k;t,t’)C(k;t’,t’) \, \mbox{for} \, t’\leq t. \end{equation} Equation (3) is just the fluctuation relaxation relationship (FRR) which has been derived in dynamical systems theory for systems with a Gaussian initial distribution. Incidentally, the *fluctuation dissipation theorem* is a special case of the FRR which applies to small fluctuations about equilibrium in microscopic systems.

The FRR applied to turbulence has now been derived by a self-consistent method in which the base distribution is Gaussian at all times [6]. This reference gives a review of the topic as well as that derivation. It should perhaps be noted that the zero-order Gaussian pdf in this theory is an approximation to the exact pdf which is chosen to give the correct value of the covariance. It should be distinguished from the zero-order pdf which is obtained from the viscous response function applied to Gaussian stirring forces.

To sum up, equation (1) is a bad equation which yet provides a heuristic derivation of a useful set of equations: the LET theory. I think that it is analogous to a `bad proof’ as discussed in my post of 19th March 2020. Hence, LET was a curate’s egg theory. I think that it might now be described as just a theory.

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