Should turbulence researchers dare to be dull?

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I recently read a book review in *The Times* which was headed `Scientists must dare to be dull'. Well, that was attention grabbing, because most of the general population probably think that we already are. The author of the review then went further in a subheading: `We should listen to this warning about how neophilia and hype is ruining research.' Now that does sound a bit exaggerated; and he seeks to make his case by quoting examples from *Science Fictions: Exposing Fraud, Bias, Negligence and Hype in Science by Stuart Ritchie.*

Now I'm not sure if `neophilia' is a neologism or not (my spell-checker doesn't seem to like it), but clearly it is intended to mean `love of the new'. And this, along with `hype', has been a feature of academic research since the early 1980s. Before that, academic research was a gentlemanly pursuit, which in theory academics were supposed to do. However, when I took up my lectureship at Edinburgh in 1971, the teaching and administration were divided up equally, and once these chores were out of the way, one was free to do some research or some other activity. Alternative activities pursued by certain colleagues ranged from collecting antiques, through small-boat sailing, to (and this was rather extreme) one colleague who seemed to be turning himself into a market gardener in his spare time.

This all changed around the early 1980s, with the introduction of research assessment exercises, in which the government turned a beady eye on the research output of academics, presumably to divert attention from its own inadequacies. From then on, everything had to be newer, bigger and more `hype worthy'. Then of course, in time, research had to have impact! But we shall say no more about that. Instead let us turn to what the effect of this has been on research in turbulence.

We should begin by observing that turbulence, like all the rest of fluid dynamics, is dominated by research on practical problems. So my observations, as always, concern the relatively small amount of fundamental work; and even here there has for a long time been an excessive concentration on newness. Given that the problems we still need to solve are really quite old, a concentration on newness seems likely to be counter-productive. My own experience over the years has been of one particular referee who invariably says of my manuscript `there is nothing very new here’ and then turns it down!

To be more specific, I would say that direct numerical simulation of the equations of motion to represent isotropic turbulence is the most obvious example of the desire for the new, where in this case the desirable `new’ is a higher Reynolds number. This undoubtedly leads to a feeling of competition, with the achievement of a large Reynolds number seen as an end in itself. I believe this to be detrimental to scholarship, particularly when other desirable features of the DNS may have been sacrificed in order to achieve it.

A particular example of this arose in 2010 when we submitted a short paper in which we showed that the so-called Taylor dissipation surrogate was more likely a surrogate for the inertial transfer [1]. This was based on theoretical arguments and on some simulations of freely decaying turbulence, for various Reynolds numbers up to about $R_{\lambda}\simeq 60$, which showed the onset of asymptotic behaviour. One referee was favourable but the other recommended rejection on the grounds that our simulation was very much smaller that his one. This seems to have echoes of the behaviour of small boys in the school playground, but it has nothing to do with scholarship. Fortunately the editor was easily persuaded of this fact, and the paper was published.

A coda to this story is that we developed our simulations over the next few years, and also introduced a theory based on an asymptotic expansion in inverse powers of the Reynolds number, which was exact in the limit of infinite Reynolds numbers. For Reynolds numbers up to $R_{\lambda}\simeq 435$ in forced turbulence, we were able to verify our predicted $1/R$ decay law and measure the asymptotic value of the normalised dissipation rate as: $C_{\varepsilon,\infty} = 0.468 \pm 0.006$. Apart from supporting our results at lower Reynolds numbers, this work drew attention to the fact that certain high-Reynolds simulations merely provide a few outlier points on our systematic treatment of the subject [2]. How much better if they had started with low values of the Reynolds number and worked up!

Turbulence is essentially an asymptotic phenomenon; a fact that was realised by early workers in the subject who measured mean velocity profiles in duct flows (and indeed other shear flows) for huge ranges of Reynolds numbers, and clearly demonstrated its asymptotic behaviour. This is what we need today. Turbulence theory is like a jigsaw, in which not only are many pieces missing, but many of those we have are unclear. In effect, we're not quite sure which part of the picture they represent. In my view, what is needed is a big collaboration to carry out simulations which we can all access and have our questions answered. But the simulation is the easy part of that: I believe that there are databases for high-Re simulations, but what about all the low Reynolds numbers which allow us to move up an asymptotic curve and actually see what is going on?

The author of the above book review sees the need for `boring, plodding research that merely provides a sound basis for the continued progress of the Enlightenment'. I don't buy that description, and presumably he is being ironic, but I do accept that that is what we need. In the case of turbulence, we would also need a sea change to more open-mindedness on the part of many members of the community of researchers. I don’t think that is going to happen any time soon.

[1] W. David McComb, Arjun Berera, Matthew Salewski, and Sam R. Yoffe. Taylor’s (1935) dissipation surrogate reinterpreted. Phys. Fluids, 22:61704, 2010.
[2] W. D. McComb, A. Berera, S. R. Yoffe, and M. F. Linkmann. Energy transfer and dissipation in forced isotropic turbulence. Phys. Rev. E, 91:043013, 2015.

The modified Lin equation.

The modified Lin equation.

In my post of 27 February I discussed the importance of being aware of the full form of the Lin equation as this reveals the existence of a cascade in wavenumber space. In this post I want to take this a bit further, using my resolution of the *scale-invariance paradox* [1].

For me this topic first arose during a meeting in 1991 at MSRI, Berkeley. When I had finished my talk, Bob Kraichnan came up to me with a copy of my recently published book and pointed out Figure 2.5, which was a plot of the terms in the Lin equation for freely decaying turbulence. He commented on the fact that the transfer spectrum $T(k)$ was shown as zero for an extended range of values of k. He commented that people used to think that was the case, because it would be expected from the scale-invariance of the flux, but that in practice it was never observed. There was always a single zero-crossing. I was able to reassure him that figure was based on a computation of the LET theory; that there had been an error which had now been rectified; and that the revised figure would show a single zero-crossing and would appear in the paperback edition of the book to be published later that year.

However, I was left with a nagging feeling that there was an unresolved problem with this result. The first measurements of $T(k)$ had been published by Uberoi [2] in 1963, and this author had said that the single zero-crossing was probably due to the low Reynolds number and indicated that he would expect $T(k)=0$ over an extended range of k to develop with increasing Reynolds number. Although this does not seem to have been a matter of widespread concern, over the 1970s/80s/90s various ad hoc methods were used to cope with this behaviour in numerical calculations: for some references to this work, see [1]. As a matter of interest, I include both versions of Figure 2.5 below.

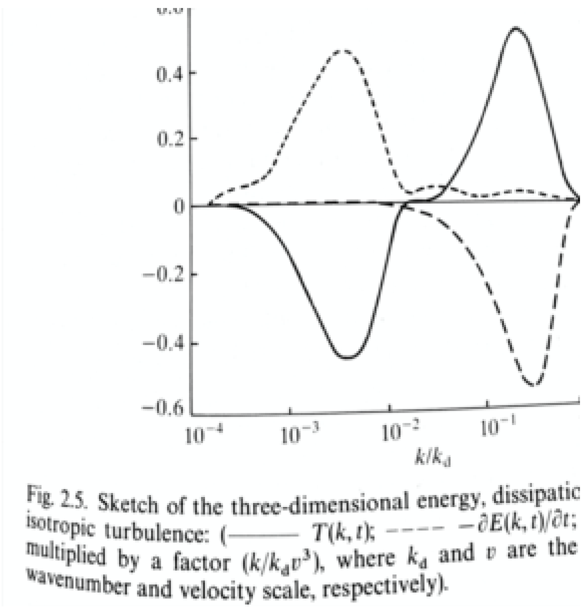


Figure 2.5 from Physics of Fluid Turbulence 1990

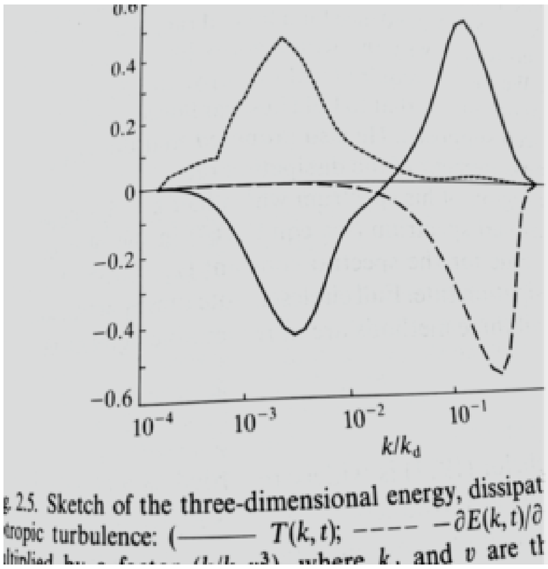


Figure 2.5 from Physics of Fluid Turbulence 1991

The Lin equation (see reference [3]) takes the form: \begin{equation} \left(\frac{d}{dt} + 2 \nu k^2 \right) E(k,t) = T(k,t)\label{enbalt}\end{equation} where $E(k,t)$ is the energy spectrum, $T(k,t)$ is the energy transfer spectrum and ν is the kinematic viscosity. Now let us integrate each term of (\ref{enbalt}) with respect to wavenumber, from zero up to some arbitrarily chosen wavenumber κ: \begin{equation} \frac{d}{dt}\int_{0}^{\kappa} dk\, E(k,t) = \int^{\kappa}_{0} dk\, T(k,t)-2 \nu\int_{0}^{\kappa} dk\, k^2 E(k,t). \label{fluxbalt1} \end{equation} The energy transfer spectrum may be written as \begin{equation} T(k,t) = \int^{\infty}_{0} dj\, S(k,j;t), \label{ts}\end{equation} where, as is well known, $S(k,j;t)$ can be expressed in terms of the triple moment. Its antisymmetry under interchange of k and j guarantees energy conservation in the form: \begin{equation}\int^{\infty}_{0} dk\, T(k,t) =0. \label{encon} \end{equation}

With some use of the antisymmetry of S, along with equation (\ref{encon}), equation (\ref{fluxbalt1}) may be written as \begin{equation}\frac{d}{dt}\int_{0}^{\kappa} dk\, E(k,t) = – \int^{\infty}_{\kappa} dk\,\int^{\kappa}_{0} dj\, S(k,j;t)-2 \nu\int_{0}^{\kappa} dk\, k^2 E(k,t).\label{fluxbalt2}\end{equation} the integral of the transfer term is readily interpreted as the net flux of energy from wavenumbers less than κ to those greater than κ, at any time t.

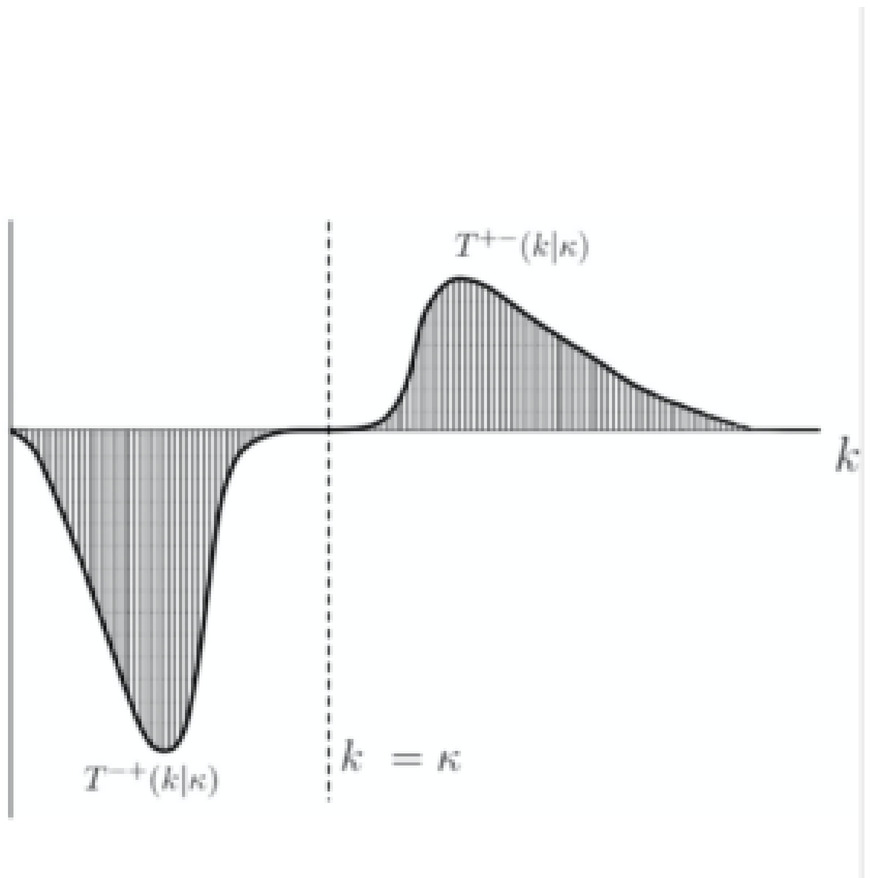
It is convenient to introduce a specific symbol Π for this energy flux, thus: \begin{equation}\Pi (\kappa,t) = \int^{\infty}_{\kappa} dk\, T(k,t) =-\int^{\kappa}_{0} dk\,T(k,t),\label{tp}\end{equation} where the second equality follows from (\ref{encon}).

The key to resolving the paradox is to introduce transfer spectra which have been filtered with respect to k and which have had their integration over j partitioned at the filter cut-off, i.e. $j=k_c$ [1],[4]. Beginning with the Heaviside unit step function, defined by:
\begin{eqnarray} H(x) & = & 1 \qquad \mbox{for} \qquad x > 0; \\& = & 0 \qquad \mbox {for} \qquad x < 0.\end{eqnarray} we may define low-pass and high-pass filter functions, thus: \begin{equation}\theta^{-}(x) = 1 – H(x),\end{equation} and \begin{equation} \theta^{+}(x) = H(x). \end{equation} We may

then decompose the transfer spectrum, as given by (\ref{ts}), into four constituent parts, \begin{equation}T^{–}(k|k_{c}) = \theta^{-}(k-k_{c})\int^{k_{c}}_{0}dj\, S(k,j); \label{tmm}\end{equation} \begin{equation} T^{-+}(k|k_{c}) = \theta^{-}(k-k_{c})\int^{\infty}_{k_{c}}dj\, S(k,j); \label{tmp}\end{equation} \begin{equation} T^{+-}(k|k_{c}) = \theta^{+}(k-k_{c})\int^{k_{c}}_{0}dj\, S(k,j); \label{tpm} \end{equation} and \begin{equation}T^{++}(k|k_{c}) = \theta^{+}(k-k_{c})\int^{\infty}_{k_{c}}dj\, S(k,j),\label{tpp} \end{equation} such that the overall requirement of energy conservation is satisfied: \begin{equation} \int^{\infty}_{0}dk\left[T^{–}(k|k_{c}) + T^{-+}(k|k_{c}) + T^{+-}(k|k_{c}) + T^{++}(k|k_{c})\right] = 0. \end{equation}It is readily verified that the individual filtered/partitioned transfer spectra have the following properties: \begin{equation} \int^{k_{c}}_{0}dk\, T^{–}(k|k_{c}) = 0; \label{mm} \end{equation} \begin{equation} \int^{k_{c}}_{0}dk\, T^{-+}(k|k_{c}) = -\Pi(k_{c});\label{mp} \end{equation} \begin{equation}\int^{\infty}_{k_{c}}dk\, T^{+-}(k|k_{c}) = \Pi(k_{c}); \label{pm} \end{equation} and \begin{equation} \int^{\infty}_{k_{c}}dk\, T^{++}(k|k_{c}) = 0. \label{pp} \end{equation} Equation (\ref{fluxbalt1}) may be rewritten in terms of the filtered/partitioned transfer spectrum as: \begin{equation} \frac{d}{dt}\int^{k_{c}}_{0}dk\, E(k,t) = -\int^{\infty}_{k_{c}}dk\, T^{+-}(k|k_{c}) -2\nu_{0}\int^{k_{c}}_{0}dk\, k^{2}E(k,t). \label{fluxbaltmod} \end{equation} We note from equation (\ref{mm}) that $T^{–}(k|k_c)$ is conservative on the interval $[0,k_c]$, and hence does not appear in (\ref{fluxbaltmod}), while $T^{-+}(k|k_{c})$ has been replaced by $-T^{+-}(k|k_{c})$, using (\ref{mp}) and (\ref{pm}). Those working with DNS or analytical theory, can avoid the paradox by changing their definition of energy fluxes, from those given by (\ref{tp}), to the forms: \begin{equation} \Pi (\kappa,t) = \int^{\infty}_{\kappa} dk\, T^{+-}(k|\kappa,t) =-\int^{\kappa}_{0} dk\, T^{-+}(k|\kappa,t),\label{tpmod} \end{equation} where $T^{+-}(k|\kappa,t)$ is defined by

(\ref{tpm}) and $T^{-+}(k|\kappa,t)$ by (\ref{tmp}). This is equivalent to (\ref{tp}); but, unlike it, avoids the paradox.

This behaviour is illustrated in the figure below, where we should note that $T^{-+}(k|\kappa)$ is defined below the cut-off wavenumber $\kappa = k_{c}$, and $-T^{+-}(k|\kappa)$ is defined above it.



Modified form of transfer spectrum to avoid the scale-invariance paradox.

This raises the question of how exactly the Lin equation should be written, in order to emphasise these properties. That will be the subject of a paper which is now in preparation [5]. It is worth making the point that the filtered-partitioned forms of the transfer spectrum have only been studied in the context of the subgrid modelling problem

[4]. Given the much more powerful computers now available, it would undoubtedly be rewarding to study the role of these terms in the energy balance for a range of Reynolds numbers. I very much hope that someone will do this.

Acknowledgement: the above figure was suggested by John Morgan, who also prepared it.

[1] David McComb. Scale-invariance in three-dimensional turbulence: a paradox and its resolution. J. Phys. A: Math. Theor., 41:75501, 2008.
[2] M. S. Uberoi. Energy transfer in isotropic turbulence. Phys. Fluids, 6:1048, 1963.
[3] W. David McComb. Homogeneous, Isotropic Turbulence: Phenomenology, Renormalization and Statistical Closures. Oxford University Press, 2014.
[4] W. D. McComb and A. J. Young. Explicit-Scales Projections of the Partitioned Nonlinear Term in Direct Numerical Simulation of the Navier-Stokes Equation. In Proc. 2nd Monte Verita Colloquium on Fundamental Problematic Issues in Turbulence: available at arXiv:physics/9806029 v1, 1998.
[5] W. D. McComb. A modified Lin equation for the energy balance in isotropic turbulence. arXiv:2007.13622v1 [physics.flu-dyn] 27 Jul 2020

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:
$$C(k;t,t') = R(k;t,t')C(k;t',t') \, \mbox{for} \, t'\leq t. \tag{3}$$
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.

[1] W. D. McComb. A local energy transfer theory of isotropic turbulence. J.Phys.A, 7(5):632, 1974.
[2] W. D. McComb. The inertial range spectrum from a local energy transfer theory of isotropic turbulence. J.Phys.A, 9:179, 1976.
[3] W. D. McComb. A theory of time dependent, isotropic turbulence. J.Phys.A:Math.Gen., 11(3):613, 1978.
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[5] K. Kiyani and W. D. McComb. Time-ordered fluctuation-dissipation relation for incompressible isotropic turbulence. Physical Review E, 70:66303-66304, 2004.
[6] W. D. McComb and S. R. Yoffe. A formal derivation of the local energy transfer (LET) theory of homogeneous turbulence.

J. Phys. A: Math. Theor., 50:375501, 2017.

Peer review: the role of the editor.

Peer review: the role of the editor.
In 1985 I published a paper in JFM on laser-doppler measurements in drag-reducing fibre suspensions. This was the only paper on experimental work that I published in that journal and the refereeing process was not without interest. There was the usual iteration process and Referees A and B were fine, but Referee C was something else. His comments had a curious, slightly hysterical tinge, I felt. For instance, `Something is very far wrong here.' and `Conservation of energy is being violated here.' And others like that. Each attempt I made to reassure him, simply made matters worse. I should just mention in parenthesis that when you get a referee like this, they are impossible to reassure or satisfy. Editors need to be alive to this fact and in this case George Batchelor eventually said something to the effect `I'm afraid that C is being rather too suspicious and so I am going to disregard his reports.' In my view this was a perfect example of a good editor in action. He had ample evidence from A and B that the paper should be published and he took responsibility for having made an unlucky choice in C.

Some years later I was again having a paper reviewed by JFM and once again Referees A and B were fine, but this time C objected to the fact that the LET theory was being applied to isotropic turbulence. He said `there is far too much of this sort of work going on' and `the real problems are shear flows'. In response I argued that this work was physics and that, in comparison to condensed matter physics or particle physics, the amount of work on isotropic turbulence is very small and we really need a great deal more. Again, in parenthesis, this remains my opinion. Referee C responded by recommending rejection, and this time the editor (not Batchelor!) said `well clearly C is an idiot and I'm going to ignore him'.

Actually this is all beginning to sound like it belongs in the story by the Canadian humourist Stephen Leacock `A, B and C: the human element in mathematics' in which he discusses problems in arithmetic of the type: `A, B and C are employed to dig a ditch. A can dig twice as fast as B and B can dig twice as fast C etc'. In his short story Leacock speculates about the three individuals and their interactions. He concludes that C always gets the dirty end of the stick and is a weak, undersized individual who dies young. Poor C!

So let us therefore turn to a bimodal form of refereeing, as practiced by the Physical Review. As I mentioned in my post of 25 June, when writing my book on HIT I found out that the coefficient E_2 in the Taylor expansion of the energy spectrum was identically zero. To my astonishment this appeared to be a new result, particularly in view of the ongoing controversy over `Saffman invariance *vs* Loitsianskii invariance'. After getting it independently checked, I wrote it up and submitted it to PRE. At the risk of spoiling the suspense, I should say that it was ultimately accepted for publication [1]. Nevertheless, the refereeing process had some remarkable features and raises some questions of interest.

First, Referees 1 and 2 replied. Referee 1 was positive and 2 was not. In fact their report was an incoherent rant which I found impossible to understand. I could manage to pick out phrases which I recognized as being points that are made about grid turbulence, but I was unable to discern anything relating to my paper. Moreover the entire report was in bold italic font, rather giving the impression of being what the police used to call `a green ink letter'.

So the Editor commissioned reports 3 and 4, one of which was favourable and the other was not. And then the Editor commissioned reports 5 and 6, one of which was favourable and the other was not. There was also a new development in that Referee 6 dragged in a recent disagreement between two different sets of investigators.

At this stage the Editor decided to reject my manuscript. This seemed to me to be `box ticking' of the worst kind. Three for and three against, so let's be on the safe side and reject it! Unlike in the two cases discussed above with JFM, there was no attempt to make a judgement of the relative quality of the referee reports. Naturally, I did not accept this. There followed a so-called arbitration, which was no such thing, and which I had no difficulty in shooting down. Then the Editor proposed a compromise. If I would add some material relating to the disagreement that Referee 6 had instanced, he would send it back to that referee. However, despite my adding material relating to that disagreement, Referee 6 did not change his extremely hostile attitude and recommended rejection. This time the Editor did what he should have done sooner and ignored this referee's unbalanced report.

I should say that when I say Editor, I mean one of the associate editors of PRE at that time. Also, as PRE doesn't come well out of this, I should mention a case where they did, and where (refreshingly!) the villains were not members of the turbulence community. I will keep this brief because I think this topic merits a post to itself. Basically I had done an analysis which showed that Galilean invariance did not suppress vertex renormalization in the NSE or similar equations which were of interest in soft condensed matter. Now unfortunately there was a substantial body of work in soft matter which relied very heavily on the supposition that it did, and not surprisingly my manuscript got a hostile reception. Any favourable reports were lukewarm (`might be of mild interest') and the Editor turned the MS down.

I wrote to the Editor to say that I accepted his decision but wanted to point something out. If I was wrong, then not only were the `soft matter' theorists better off as a result, but so also would I be, in that my LET theory would automatically be correct to fourth- rather than third-order in renormalized perturbation theory! The Editor suggested that I formally appeal against his decision, I did, and the arbitration was very much in my favour [2].

All four of these examples worked out satisfactorily, in my view, in that papers which should have been published were published. But they have worked out in different ways. In particular there is the question of should the editor pay attention to the quality of the reports? Let us bear in mind that editors are perhaps more reluctant to offend referees than authors. Also, when a number of referees are positive can that be cancelled out by a number being negative? I welcome comments on my posts and would particular welcome comments on these particular points.

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Further thoughts on free decay of isotropic

turbulence.

Further thoughts on free decay of isotropic turbulence.
In the previous post I discussed the initial value problem posed by the free decay of the energy in isotropic turbulence, along with things that we ought to bear in mind when considering its experimental or DNS realisations. We should also mention the more general problem of the free decay of two-point covariances (or spectra) as that merits a few words in the context of both DNS and the study of two-point statistical closures. However, before considering it, we should first consider an outstanding question about the simpler case: at what stage is the turbulence to be considered as evolved?

The question arises because the initial state of the turbulence is not actually a solution (or, more accurately, derived from a solution) of the Navier-Stokes equation. For the purely mathematical problem, we may indeed assume that the initial field corresponds to isotropic turbulence. But for grid turbulence, the wakes that form behind the bars of the grid are expected to coalesce into a three-dimensional turbulent field, which dies away with downstream distance. This stationary stream-wise decay has to be converted to decay with time by invoking Taylor’s hypothesis, but the crucial question is: at what distance downstream can the turbulence be said to be evolved?

The same question must arise with DNS, where we specify an initial spectrum on a lattice. Such initial spectra are arbitrarily chosen to have suitable properties. In particular, they are chosen to be peaked at low values of wavenumber, so that the evolution of turbulence can be seen as the spectrum not only decreases in magnitude, but also spreads out, as time goes on. So once again we wish to know at what time the spectrum will be representative of turbulence, rather than the initial conditions.

Probably most investigations into this topic have been concerned with establishing whether or not the decay follows a power law; and, if so, what that power law is. In fact some researchers cite the onset of power-law behaviour as indicating that the turbulence is well developed. Yet there is at least once situation where the need for a definite criterion matters and this is the study of the dimensionless dissipation in terms of its dependence on the Reynolds number. This is known to follow a characteristic curve in which it asymptotes to a constant value with increasing Reynolds number.

Now, for stationary turbulence, the existence of a unique curve is unambiguous on both experimental (i.e. DNS) and theoretical grounds), but for free decay there is a fair amount of scatter between the various investigations. When we began working on this problem at Edinburgh some years ago, we were surprised to find that most researchers seemed rather vague about the stage of the decay process at which their measurements were taken. It seemed to us that this was likely to prove crucial. An investigation would consist of carrying out a free decay simulation at a particular Reynolds number; then repeating it for a higher Reynolds number, and so on. Then the problem at any Reynolds number is to choose a decay time to take measurements that corresponds in some sense to `the same stage' at other Reynolds numbers. This is not a trivial problem and we decided to look into it in detail [1].

When a turbulence simulation is started from an arbitrary initial velocity field with a Gaussian distribution, both the inertial transfer and the skewness grow from zero, pass through a peak and then decay. In contrast, the dissipation rate starts with a finite value and either decays (low Reynolds numbers, say $R_\lambda(0) \leq 25$) or rises to a peak and then decays (higher Reynolds numbers). The existence of a peak offers the possibility of a well-defined criterion which would allow the results of one investigation to be compared with another. We plotted graphs of dimensionless dissipation $C_\varepsilon (t_e)$ against Reynolds number for various choices of evolved time t_e (see Fig. 13) and found that the resulting behaviour depended strongly on the choice made. For instance, choosing t_e to be either based on the peak skewness or peak inertial transfer led to the curve tending to zero. As there is no peak dissipation for low Reynolds numbers (and the variation of $ C_\varepsilon$ is predominantly a low Reynolds number phenomenon) this appeared to rule peak dissipation out as a criterion. However, we found that a composite criterion, based on peak transfer at low Reynolds numbers and on peak dissipation at larger Reynolds numbers, where a peak existed, gave very interesting results, with the dimensionless dissipation curve being very like the stationary forced case, and tending to a value of about 0.5.

I do not claim that these results are prescriptive or definitive in any way, although they are certainly quite plausible. But I hope they will encourage others to investigate further. If this is not done, the studies in decaying turbulence will remain a hodge-podge where variations between investigations are often probably due to a failure to compare like with like.

Lastly, in my previous post I said that at an early stage in my career I resolved to stay clear of the problem of free decay. To avoid any appearance of inconsistency I should point out that this resolution was limited to the theoretical problem of predicting the decay rate of the energy. In the late 1970s we began studying the LES theory applied to the problem of free decay of two-point, two-time statistics. This work was reported in 1984 [2], and involved a detailed comparison with DIA, using the same initial spectra and computational methods as previously used by Kraichnan. This allowed `like for like' comparisons in great detail, which was still the case when the comparisons were extended to DNS in later years. So the onset problem did not, as such, arise.

[1] S. R. Yoffe and W. D. McComb. Onset criteria for freely decaying turbulence. Phys. Rev. Fluids, 3:104605, 2018.
[2] W. D. McComb and V. Shanmugasundaram. Numerical calculations of decaying isotropic turbulence using the LET theory. J. Fluid Mech., 143:95-123, 1984.