A postscript on degrees of freedom necessary for a turbulence calculation.

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In the preceding post, we discussed the new method that has been a proposed by Donzis and Sajeev which reduces the computational effort but still retains all the degrees of freedom [1]. In this method, all the degrees of freedom are divided at the time step $t=t_n$ into resolved modes, which are integrated forward in time to $ t=t_{n+1}$ using the NSE; and unresolved modes which are assumed not to change at that time step. The resolved modes are selected by a random sampling process which is carried out at each time step. In this way, the total number of degrees of freedom remains the same, but those actually being computed differ from one time step to another. This has the great advantage, compared to other methods, of apparently being able to correctly retain the initial value of the total energy. However, some qualification of this point needs to be made, as follows.

Consider the total energy $E(t_n)$ at time t_n, which is given by \begin{equation}E(t_n)=\frac{1}{2}\sum_{k=0}^N \langle u^2(k,t_n)\rangle, \end{equation}where N is the total number of wavenumber modes or degrees of freedom. (Note that in numerical simulation, the symbol N is used for the number of resolved modes in one direction, with N^3 being used for the total number of resolved modes being simulated.) For a forced, stationary simulation of the NSE, the total energy tends to fluctuate about a mean value as time goes on: see Figure 3 in Reference [2], despite the fact that in principle it should be constant. This behaviour is presumably due to rounding errors.

However, in the Donzis-Sajeev proposal, there may be an additional cause of fluctuation. At time $t=t_n$ they divide the total number of modes up into resolved modes, which are obtained by integrating the NSE forward in time to $t=t_{n+1}$, and are denoted by k_r; and unresolved modes which are kept at the same value and are denoted by k_u. The corresponding numbers of modes are $n(k_r)$ and $n(k_u)$, which add up to the total number $n(k_t)=N$. At the time $t=t_{n+1}$ the expression for the total energy should be decomposed into two parts:

$$\begin{equation}E(t_{n+1})=\frac{1}{2}\sum_{k\in \{k_r\}}^{n(k_r)} \langle u^2(k,t_{n+1})\rangle +\frac{1}{2}\sum_{k\in \{k_u\}}^{n(k_u)} \langle u^2(k,t_n)\rangle . \end{equation}$$

As the first term on the right belongs to the sub-ensemble of the resolved modes and the second term belongs to the sub-ensemble of the unresolved modes, there is no *a priori* reason that $E(t_n)$ should be equal to $E(t_{n+1})$. It would be interesting to see how the behaviour of the total energy would compare with that of the equivalent full simulation.

This will be my last post before the holidays. I hope to resume in January 2025.

[1] Diego Donzis and Shilpa Sajeev. Degrees of freedom and the dynamics of fully developed turbulence. Physical Review Fluids, 9:044605–1–11, 2024.
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How can we reduce the number of degrees of freedom necessary for a turbulence calculation?

How can we reduce the number of degrees of freedom necessary for a turbulence calculation?
This question first arose in the 1960s with attempts to do meteorological simulations. The response was Large Eddy Simulation (LES), with its associated sub-grid modelling. A short introduction to this topic can be found in section 3.1 of the book [1] with a longer treatment in chapter 10 of the same reference. A more fundamental approach was introduced with the work of Kraichnan [2] in 1976 who applied his test field model to calculate an effective viscosity to represent the sub-grid flux in isotropic turbulence. This resulted in the well-known 'plateau and peak' form; but, having been derived from a statistical theory, it could not represent the phase effects of the random velocity field.

In a series of papers, beginning in 1990, McComb, Roberts and Watt introduced the iterative conditional averaging method to calculate a sub-grid viscosity for isotropic turbulence [3–5]. This method is the only use of the renormalization group algorithm that takes account of the differences between classical fluid turbulence and quantum field theory or the theory of critical phenomena. The resulting sub-grid viscosity takes good account of the inertial energy transfer to the sub-grid modes but does not fully represent the phase effects.

Lastly, the operational method of Young and McComb [6,7] used a feedback loop to modify the velocity field to ensure the correct monotonic decline of the energy spectrum. Good results are obtained, and phase effects are fully taken into account.

However, none of these methods retains all the degrees of freedom. For isotropic turbulence, these are the Fourier components of the velocity field, and referring to [1] page 113, we have: \begin{equation}E=\frac{1}{2}\sum_{k=0}^N \langle u^2(k)\rangle,\end{equation}where E is the total kinetic energy per unit mass of the turbulence. The total number of degrees of freedom N can be shown to depend on the Reynolds number to the power of $9/4$, and so increases rapidly with increasing Reynolds number. Accordingly, the price one pays for reducing the number of degrees of freedom while still representing the dissipation rate correctly means that in principle and practice there must be some reduction of the total kinetic energy. This may not be too serious a matter: see the figure 2 in reference [8]. Here we see that the maximum resolved wavenumber must be greater than $1.5k_d$, where k_d is the Kolmogorov dissipation wavenumber; whereas the total energy may be represented with a maximum resolved wave number of less then $0.5k_d$. It should be noted that this graph is based on a particular model spectrum, but similar results would be achieved with other model spectra or indeed with the results of numerical simulation.

This year a new method has been a proposed by Donzis and Sanjeev which reduces the computational effort but still retains all the degrees of freedom [9]. In this method, all the degrees of freedom are divided at the time step $t=t_n$ into resolved modes, which are integrated forward in time to $ t=t_{n+1}$ using the NSE; and unresolved modes which are assumed not to change at that time step. The resolved modes are selected by a random sampling process which is carried out at each time step. In this way, the total number of degrees of freedom remains the same, but those actually being computed differ from one time step to another. The method was investigated for two different sampling criteria and for different fractions of the total number of modes being resolved, ranging from 10% to 100% (corresponding to full simulation), with impressive qualitative and quantitative results for energy spectra, dissipation rate, skewness and so on. This seems to be a promising method of numerical simulation, but also promises to shed light on the nature of turbulence.

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Theory versus model: what's the difference?

Theory versus model: what's the difference?
A consideration of this point inevitably underlines the cultural chasms that exist within the turbulence community. It was prompted by my consideration of turbulence phenomenology in the preceding post, and in particular by the discussion given in the book by Arkady Tsinober [1].

He argued that the broad phenomenology of turbulence "includes also most of the semiempirical approaches and turbulence modelling". He added a footnote to this which refers to "this enormous material" and cites ten books as an indication of the material that he is omitting. There are two points that I would like to make about this.

First, judging by the books he cites, he included in this category the statistical theories. In contrast, I think inclusion of this topic violates the definition of phenomenology as being a substitute for a satisfactory statistical theory. I would also draw a distinction between statistical theories which are an attempt at a fundamental solution of the problem and models which are intended to give numerical answers in practical situations.

Secondly, I am surprised by the use of the word "enormous", as I believe that pure theoretical work is a very small discipline indeed, and dwarfed by the enormous subject of phenomenology. As regards engineering models I would have thought that was a small field too, but I may well be very much out of date on this. However, these models may be seen as expedients, and not intended primarily to increase our understanding of the subject. They do not seem to me to be natural candidates for inclusion in the category of phenomenology.

Turning now to our initial question, the answer so far as turbulence is concerned, would seem to be the following. Theory involves general operations with a minimum of specific assumptions. Modelling relies on more specific assumptions with the introduction of one or more adjustable constants. That is to say you have to complete a theoretical model by a comparison with experiment in order to fix a value of the constant. A very clear case was Kraichnan's introduction of the *test field model* where in contrast to his direct interaction theories he had introduced a specific approximation along with an adjustable constant. Of course, the distinction may be different in other fields, but this is what it is for turbulence.

[1] A. Tsinober. An Informal Conceptual Introduction to Turbulence. Springer, Dordrecht, 2nd edition, 2009.

What is turbulence phenomenology?

What is turbulence phenomenology?

I was quite disconcerted to see that my last post had been at the end of July, just before I took my summer break. However, I've been busy planning a new book on the phenomenology of isotropic turbulence. This term now seems to be in quite general use, extending back over the last couple of decades, so it would be worth considering what exactly it means.

If we go back to sources, phenomenology seems to have originated in mathematics and then been taken over by philosophy. It appears to have found its main applications in philosophy and sociology. A basic characteristic is that it is an activity which avoids theorising or attempting to explain. That is, it is an attitude of mind adopted when investigating a field in which one is content to describe the field without attempting to explain it. This actually is a good way of approaching any new investigation. We need to establish the facts, uncontaminated by speculation or even emotional reactions. Such attitudes must underpin any effective legal-judicial system.

My impression is that nowadays in physics what is called phenomenology is in fact an attempt to explain things; largely because the traditional fundamental theoretical physics approach is too difficult. This seemed to me to be particularly so in particle physics, and I thought it appropriate to adopt it for turbulence. So, around the turn of the millennium, I began using it in that sense in turbulence. I also noted that others were using it, and I assumed that it was in the same sense. But this could be a dangerous assumption, so I was pleased to find out that the subject of this usage had been discussed in the book by the late Arkady Tsinober [1], where it appears as chapter 5.

Tsinober quotes various views on the subject and one clear point that emerges is that in science and particularly turbulence it is regarded as an inferior activity and spoken of in a disparaging way. This is not surprising. Many turbulence researchers are applied mathematicians, whose ingrained approach is to solve mathematical equations subject to initial and boundary conditions. Unfortunately, the governing equations of turbulent motion are nonlinear and hence insoluble.

After pointing out that there is no definition of phenomenology of turbulent flows, Tsinober puts forward two views of his own. First, he suggests that it is a statement of impotence, consisting of everything but direct experimental results and any results that can be obtained from first principles. As an example of the latter, he cites the (presumably) derivation of the Navier-Stokes equations. At first sight this seems reasonable because to a physicist experiment provides the foundation of a subject. However, second thoughts suggest that the vast and diverse set of experimental results in turbulence are not really foundational in character. Thus, I would be inclined to class them as part of the phenomenology.

His second point of view is:

“*Phenomenology of turbulence involves use of dimensional analysis, a variety of scale arguments, symmetry, invariant properties, and various assumptions, some of which are of unknown validity and obscure physical and mathematical justification (if any).*”

I would agree with that. Broadly the motive for this activity is to increase understanding. Where I differ from him is his inclusion of semiempirical approaches and turbulence modelling and above all with his blurring of the distinction between theory and modelling. I will discuss this in my next post.

[1] A. Tsinober. An Informal Conceptual Introduction to Turbulence. Springer, Dordrecht, 2nd edition, 2009.

The humble Table of Contents.

The humble Table of Contents.

With my holiday approaching, I thought it would be pleasant to talk about something less demanding, and which involved zero LaTeXing. So, I thought that I would speak up for the Table of Contents being included in journal articles. Of course its inclusion is commonplace in books, and one couldn't imagine a textbook or monograph being without one.

At one time it was quite usual in journal articles too, and Bob Kraichnan's pioneering paper, presenting the direct-interaction approximation, in the Journal of Fluid Mechanics in 1959 was a case in point [1]. In fact I have normally put in a table of contents in submitted manuscripts for many years now, and it can easily be removed once the paper has been accepted for publication. Some years back, I submitted a paper thus equipped to the JFM; and, still trying not to move with the times, I tried to avoid the web submission method and sent it as a pdf attachment in an email to an editor. His shocked reaction seemed not unlike that of a maiden lady in a Victorian novel encountering some coarse language. His complaints ended up with: 'and you have even put an index in it!'

Well, I removed the '\toc' command and submitted the MS through the website. However, worse was to come and the paper ultimately went elsewhere. The resulting transaction came under the heading of what someone has called 'The combination of lazy editor and biased referees which plagues turbulence research'. Actually, I don't think 'lazy' is quite the right word. Perhaps something like 'conformist' would be better?

Returning to the present time, my recent experiences as Guest Editor have made me aware of just how useful a table of contents is when one is assessing a new manuscript, particularly when it is a review article. Indeed, even recently, certain review journals did require a list of contents for each article. For example, see reference [2].

So I wish to conclude with a plea. At least put a temporary table of contents in in your preprints and submissions. And, if we can persuade editors to allow them in the journals, rather than draw their skirts aside, then it should improve communication by making it easier for us all to get to grips with each other’s work.

This is my last post for the moment. I hope to resume in September.

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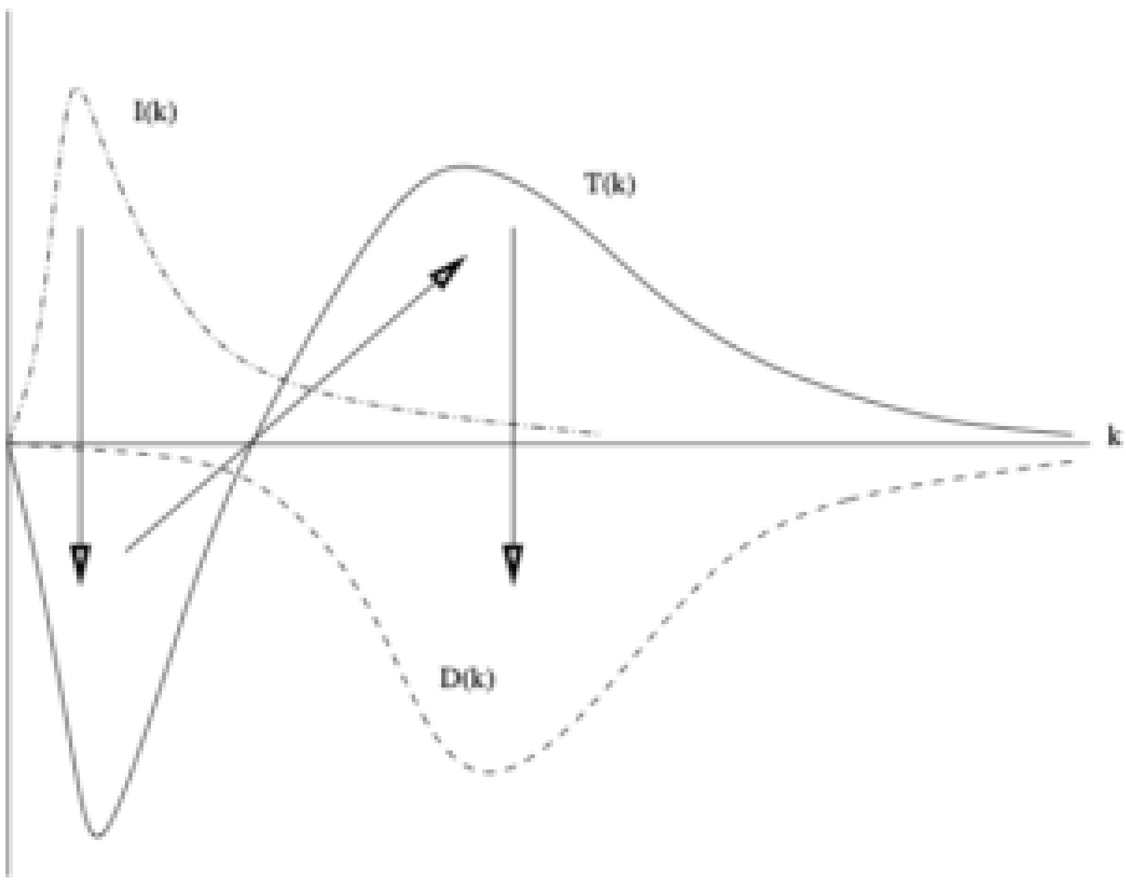
An assessment of Onsager’s concept of scale invariance: 3

An assessment of Onsager’s concept of scale invariance: 3

I will complete this sequence of posts by discussing the scale-invariance paradox and showing how the Onsager criterion for the inertial range should be modified in order to resolve it.

We begin by noting that the first measurement of the transfer spectrum was made in 1963 by Uberoi, who measured the time-derivative and the viscous dissipation terms in the Lin equation and obtained the transfer spectrum as the remaining term in that equation [1]. He was surprised to find that $T(k,t)$ had a single zero crossing when he had expected that it would be zero over an extended range of wavenumbers, corresponding by simple calculus to an extended region of scale-invariant flux. He attributed this failure to the Reynolds number being too low. However, over the following years it became clear the transfer spectrum only ever has a single zero-crossing at even the highest Reynolds numbers, and so the scale-invariance paradox was born. A fuller account of this topic can be found in the article [2] or the book [3].



Sketch of the energy balance in the Lin equation.

The behaviour of the terms in the Lin equation is illustrated schematically in the figure. Note that the input term is $I(k)

=- dE(k,t)/dt$ in this case, whereas if we were considering stationary turbulence it would be the rate of work done by the stirring forces. Further discussion of these points can be found in [3].

It is of interest to note that we can use simple mathematical reasoning to make two general observations from this figure. First, it could have been anticipated that $T(k)$ would only have a single zero-crossing at $k=k_{*}$. Evidently if the input term decreases monotonically as the inertial range is approached from below, while the dissipation spectrum also decreases monotonically as the inertial range is approached from above, then where these two terms are equal at $k=k_{*}$ (say) we must have the single zero of $T(k)$.

Secondly, from equation (9) in the first post in this sequence (on 27 June), we may infer from general conditions of smoothness corresponding to physical behaviour, that the flux must go through a maximum value at $k=k_{*}$. Thus the conclusion of Shanmugasundaram [4] from detailed computations of the LET theory could also have been anticipated.

Let us now consider the Onsager criterion in rather more detail and apply it to the interval $0\leq k \leq k_{*} $. We may write this in terms of the spectral density function $S(k,j)$ as: \begin{equation}\Pi(k_{*})=\int_0^{k_*}T(k)dk=\int_0^{k_*}dk\left[\int_0^\infty dj S(k,j)\right].\end{equation}We may then divide up the range of integration over j at $j=k_{*}$ to obtain:\begin{equation}\Pi(k_{*})=\int_0^{k_{*}}dk=\int_0^{k_{*}}dk\left[\int_0^{k_{*}} dj\,S(k,j)+\int_{k_{*}}^\infty dj\,S(k,j)\right].\end{equation}From the antisymmetry of $S(k,j)$ under the interchange of k and j, it follows that the first term in the square brackets gives zero and we are left with: \begin{equation}\Pi(k_{*})=\int_0^{k_{*}}dk\int_{k_{*}}^\infty dj\,S(k,j)\equiv \int_0^{k_{*}}dk T^{-+}(k|k_{*}),\end{equation} where we have introduced the filtered-partitioned form of the spectral transfer function $T^{-+}(k|k_{*})$.

Further details of these filtered-partitioned forms may be found in [2], where some results are cited which suggest that this procedure may resolve the scale-invariance paradox. However our main conclusion here is that it is necessary to introduce such forms, rather than just use $T(k)$, in order to understand results such as [3] and [4], particularly if we are to disentangle the effects of scaling on the Kolmogorov length versus the Taylor microscale.

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An assessment of Onsager’s concept of scale invariance: 2

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For many years, arising from Onsager’s observation in 1945 [1], the condition $\Pi_{max} = \varepsilon$ for a range of wavenumbers $k_{bot}\leq k \leq k_{top}$ has been seen as a criterion for the existence of an inertial range, and hence for the Kolmogorov $-5/3$ spectrum holding over that range. It has been a cornerstone of both statistical theories and direct numerical simulations of the Navier-Stokes equation. Thus, it is surprising that it has been subject to very little critical assessment. The picture established in many investigations is of the flux tending to a constant value, along with an increasing extent of the $k^{-5/3}$ spectrum, as the Reynolds number is increased.

However, a numerical investigation by Shanmugasundaram, which was based on the Local Energy Transfer (LET) theory [2], does not seem to support this conventional picture, which amounts to scale-invariance of the energy flux. For Taylor-Reynolds numbers $4.7\leq R_{\lambda} \leq 254$, the energy flux Π was found to take the peaked form predictable from the general behavioural arguments given in our previous post, with the peak occurring at $k=k_\star$; while, as the Reynolds number increases, the energy spectrum tends to the $-5/3$ form.

For the particular case of $R_{\lambda} = 254$, the energy spectrum in their Fig. 3 shows at least a decade of $k^{-5/3}$, as one would expect at that value of the Taylor-Reynolds number. Whereas, from their Fig. 6 we see that the energy flux, corresponding to the energy balance in the upper panel of the figure, takes the form of a peak, with Π_{max}/ε lying between 0.70 and 0.80, rather than the value of unity that Onsager suggested.

This would seem to be a good example of what prompted Kraichnan’s comment [3]: `Kolmogorov’s 1941 theory has achieved an embarrassment of success.’ In other words, despite the underlying conditions not apparently being satisfied, the $-5/3$ spectrum was still observed.

More recently, Meldi and Vassilicos [4] used the single-time EDQNM closure and, over a much greater range of Reynolds numbers, also found a small maximum in the inertial flux. They showed that the position of this maximum in wavenumber scaled on the Taylor micro length scale. Elementary calculus would then imply that the position of the single zero of the transfer spectrum also depended on the Taylor microscale and the authors confirmed that this was the case [5].

In the next post we shall argue that both these investigations were incorrect in using the flux $\Pi(k)$. Instead, they should have used the non-conservative part of the flux $\Pi^{-+}(k | k_{*})$ and the corresponding transfer spectrum: \begin{equation}T^{-+}(k|k_{*})=\int_{k_{*}}^\infty\,dj S(k,j) \quad \mbox{for} \quad 0\leq k \leq k_{*},\end{equation} as introduced by McComb [6] in the course of resolving the scale-invariance paradox.

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An assessment of Onsager's concept of scale invariance: 1

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We stated Onsager's criterion [1] for an inertial range in terms of a scale-invariant flux as equation (4) in our post of 24 June 2024. In order to assess Onsager's concept, we begin by considering the Lin equation in terms of the energy spectrum $E(k,t)$ and the transfer spectrum $T(k,t)$. We may write it in its well-known form: \begin{equation}\frac{d E(k,t)}{dt} = T(k,t) – 2\nu k^2E(k,t) \equiv T(k,t) – D(k,t),\label{lin}\end{equation}where $D(k,t)$ is the energy dissipation spectrum, as given by $D(k,t) = 2\nu k^{2}E(k,t)$. Here, we assume that there are no forces acting. We may also express the transfer spectrum in terms of its spectral density $S(k,j;t)$ thus: \begin{equation} T(k,t) = \int_0^\infty\, dj \,S(k,j;t),\quad \mbox{where} \quad S(k,j;t) = -S(j,k;t);\label{Tdef}\end{equation}and $S(k,j;t)$ contains the triple moment in wavenumber space: see [2]. Note that the antisymmetry of $S(k,j;t)$ under the interchange of k and j guarantees that conservation of energy is maintained in the form $\int_0^\infty\, dk T(k) = 0$. When we substitute for

$T(k)$ in terms of $S(k,j;t)$, we obtain the second form of the Lin equation.

If we write it in this, its full form, the Lin equation, tells us that all the Fourier modes are coupled to each other. It is, in the language of physics, an example of the *many-body problem*. It is in fact highly non-local, as in principle it couples every mode to every other mode. A corollary of this is that it predicts an energy cascade. This can be deduced from the nonlinear term which couples all modes together plus the presence of the viscous term which is symmetry-breaking. If the viscous term were set equal to zero, then the coupled but inviscid equation would yield equipartition states.

We may consider the transfer of energy from wavenumbers less than κ to wavenumbers greater than κ. To do this, we integrate the terms of the Lin equation from $k=0$ to an arbitarily chosen $k=\kappa$, with the result:\begin{eqnarray}\frac{d}{dt}\int^{\kappa}_{0}dk E(k,t) & = &\int_{0}^{\kappa}dk\int^{\infty}_{0}dj S(k,j;t)-\int^{\kappa}_{0}dk D(k,t) \nonumber \\& = & \int_{0}^{\kappa}dk\int^{\infty}_{\kappa}dj S(k,j;t)-\int^{\kappa}_{0}dk D(k,t),\label{linint}\end{eqnarray}where the second form of the right hand side relies on the fact that the double integral over $ 0\leq k,j \leq \kappa $ vanishes due to the antisymmetry of $S(k,j;t)$. Evidently this equation tells us that the loss of energy from modes with $k\leq \kappa$ is due to transfer to modes with $k\geq \kappa$, as well as the direct loss to dissipation.

In order to consider the inertial transfer further, we first introduce the symbol $E_{\kappa}(t)$ for the amount of energy contained in modes $0\leq k \leq \kappa$, thus:\begin{equation}\frac{d}{dt}E_{\kappa}(t) = \frac{d}{dt}\int^{\kappa}_{0}dk E(k,t).\end{equation}We then introduce the symbol $\Pi(\kappa,t)$, defined by:\begin{equation}\Pi(\kappa,t) = \int^{\infty}_{\kappa}dk T(k,t) = -\int^{\kappa}_{0}dk

T(k,t)\label{Pidef}\end{equation}which represents the flux through mode $k=\kappa$; and, in terms of the transfer spectral density,\begin{equation} \Pi(\kappa,t) = \int^{\infty}_{\kappa}dk\int^{\kappa}_{0}dj\,S(k,j;t) = -\int^{\kappa}_{0}dk\int^{\infty}_{\kappa}dj\, S(k,j;t).\end{equation}
Accordingly, we may write the partially integrated Lin equation (\ref{linint}) as:\begin{equation}\frac{d}{dt}E_{\kappa}(t) = -\Pi(\kappa,t) – \int^{\kappa}_{0}dk\,D(k,t).\end{equation}Thus, the energy contained in modes $0\leq k \leq \kappa$ is lost partly to direct viscous dissipation and partly due to transfer to higher-wavenumber modes.This is the only concept of localness which is required for the Richardson-Kolmogorov picture in wavenumber space (K41).

This analysis holds for any wavenumber $k=\kappa$, but the most important case occurs at $\kappa =k_{*}$, which is the wavenumber where $T(k,t)$ has its single zero-crossing. As is well known, the zeros of the transfer spectrum are given by:\begin{equation}T(0,t) =0;\qquad T(k_{*},t)=0; \quad \mbox{and} \quad \lim_{k \to \infty} T(k,t)=0: \end{equation}while, by simple calculus, the behaviour of the energy flux is given by: \begin{equation}\Pi(0,t) =0;\qquad \Pi(k_{*},t)=\Pi_{max}; \quad \mbox{and} \quad \lim_{k \to \infty} \Pi(k,t)=0.\end{equation}
It follows from conservation of energy that the maximum value that Π_{max} can take is the rate of viscous dissipation, thus we have the general result:\begin{equation}\Pi_{max} \leq \varepsilon;\end{equation}where the equality applies if the *local* viscous dissipation can be neglected at $k=k_{*}$.

In our next post, we will take a more critical look at this criterion.

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Onsager’s (1945) interpretation of Kolmogorov’s (1941a) theory: 4

Onsager’s (1945) interpretation of Kolmogorov’s (1941a) theory: 4

In his 1964 paper, Corrsin [1] explained Onsager’s theory in a more up to date notation. We will build on that treatment here, but we shall use an even more modern notation. In particular, we will use $E(k)$ for the energy spectrum and ε for the dissipation rate.

In general, energy flows from small wavenumbers to large, and Corrsin noted that Onsager envisaged this cascade as proceeding stepwise, with the sequence of wavenumbers involved taking the form of a geometric progression. He had chosen this to be wavenumber doubling at each step, with the implication that the step length was $\Delta k = k$. Arguably the amount of energy transferred at each step is: \begin{equation}\Delta E = \Delta k E(k)= kE(k)\end{equation}

Representing the flux of energy through wavenumber k by $\Pi(k)$, we may write an approximate expression for it as: \begin{equation}\Pi(k) \sim kE(k)/\tau(k),\end{equation} where $\tau(k)$ is an appropriate characteristic time for energy transfer through mode k.

We now concentrate on the inertial range and note that as the cascade is conservative and there is no significant loss of energy to viscous dissipation in this range, we may write: \begin{equation}d\Pi/dk =0, \end{equation} with the integral result: \begin{equation}\Pi = \varepsilon.\end{equation} This means that the energy flux is independent of wavenumber in the inertial range. Nowadays, this is referred to as `scale-invariance' of the energy flux in the inertial range and is widely used as a criterion for the presence of an inertial range. It is a very important concept and we shall subject it to critical scrutiny in later posts. For the moment, we will concentrate on showing how it leads to the $-5/3$ wavenumber spectrum in Onsager's theory.

In order to make progress, we introduce a characteristic time for energy transfer through mode k, which we denote by $\tau(k)$. From a simple dimensional argument, this is taken to be: \begin{equation}\tau(k)= \left[k^3E(k)\right]^{-1/2}.\end{equation} Then we substitute (5) into (4) and impose the invariance condition given by (4), to obtain for the energy spectrum: \begin{equation}E(k)=\alpha \varepsilon^{2/3}k^{-5/3},\end{equation} where the prefactor α is the well known Kolmogorov constant.

The introduction of the characteristic time $\tau(k)$ seems to be analogous to the renormalised inverse modal lifetime $\omega(k)$ which arises in the Edwards self-consistent field theory [2]. If we assume that one is the inverse of the other, then the substitution of the Kolmogorov spectrum, as given by (6), into (5) for the characteristic time yields: \begin{equation}\omega(k) = \frac{1}{\tau(k)}=\alpha^{1/2}\varepsilon^{1/3}k^{2/3},\end{equation} in agreement with the Edwards result.

It is worth noting that the power-law form of the energy spectrum is not so much an additional assumption, as it was in

Kolmogorov's earlier theory, as a natural consequence of scale-invariance because it is a scale-invariant form.

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Onsager's (1945) interpretation of Kolmogorov's (1941a) theory: 3

Onsager's (1945) interpretation of Kolmogorov's (1941a) theory: 3

Returning to this topic after my holiday, I will focus on Onsager's 1945 abstract [1]. This is brief to the point of being cryptic and requires exegesis, but we shall defer that to the next post. For the moment we will concentrate on its relationship to Kolmogorov K41A [2].

As we mentioned earlier, this fragment of Onsager's work introduced the term `cascade' as his interpretation of the Richardson-Kolmogorov picture of the nonlinear transfer of energy from large scales to small. Or, as he worked with wavenumber k, the energy cascade is from small wavenumbers to large, where it is terminated by the action of the viscosity. We shall not enlarge on that here, but merely note that he states that dimensional analysis leads to the expression for the spectral density \begin{equation}C(k) = \frac{E(k)}{4\pi k^2} = ({\mbox{universal factor}})\varepsilon^{2/3}k^{-11/3},\end{equation} where the `universal factor' equals the Kolmogorov constant divided by 4π. The $-11/3$ power law may seem unfamiliar to most people who will be used to the $-5/3$ form, but in statistical theory it is usual to work with the spectral density $C(k)$.

Onsager also pointed out that the corresponding correlation function takes the form \begin{equation}f(r)=1-(\mbox{constant})r^{2/3}.\end{equation} The term `corresponding' refers to Fourier transformation of equation (1). Note that, as well as modernising the notation, I have taken the correlation function to be the `longitudinal correlation function'. The relationship between $f(r)$ and the energy spectrum can be found as equation (2.91) in the book [3].

Bearing in mind that Kolmogorov worked with the structure functions, equation (2) is just his result with the factor $\varepsilon^{2/3}$ absorbed into the constant. In other words, we can derive Kolmogorov's result for the second-order structure function by Fourier transforming Onsager's result, and I shall argue in later posts that that is the fundamental derivation.

However, the argument works both ways, and we can argue that the $-5/3$ law for the spectrum can be derived trivially by Fourier transformation of Kolmogorov K41A for $S_2(r)$. Accordingly it is appropriate to refer to it as the Kolmogorov spectrum.

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Onsager's (1945) interpretation of Kolmogorov's (1941a) theory: 2

Onsager's (1945) interpretation of Kolmogorov's (1941a) theory: 2

Before we discuss Onsager's contribution, it will be helpful to first make some observations
about Kolmogorov's derivation of the $r^{2/3}$ law for the second-order structure function in the inertial range of scales r [1]. This is sometimes referrred to as K41A and there are many treatments of it (e.g. see the book [2]) so we will not reproduce the details here. Instead, we will attempt to highlight aspects of it, when it is judged in the context of its pioneering status. What I mean by this will quickly emerge.

First of all, Kolmogorov envisaged the effect of the nonlinearity in terms of Richardson's pictorial view of eddies being created at ever smaller scales until the smallest eddies are damped by viscosity (see page 11 of the book [2]). Of course, Kolmogorov did not acknowledge this at the time, but he subsequently did so in his 1962 paper [3]. He then introduced the idea, familiar in statistical physics, that a stepwise stochastic process with decreasing scale, could lead to a range of scales in which average properties were independent of the conditions of formation. Among other things, he also introduced the ideas of local isotropy and local stationarity; but although these are of considerable importance, for a purely theoretical look at the problem, we will restrict our attention for the moment, to fields that are both isotropic and stationary.

Now, we are dealing with a model in which the basic entities – Richardson’s `whirls’ – are not well defined. Hence, different people have different ideas of what types of eddies may be involved. Accordingly, we should examine just how sensitive the measurement of the structure functions is to the form assumed for the eddies making up the turbulent field. Let us consider an experiment where an anemometer measures the instantaneous velocity $u(x; t)$ at points $x = 0$ and $x = r$, and we take the difference between these values $\Delta u(r; t) = u(r; t) - u(0; t)$. Trivially, this velocity difference $\Delta u(r; t)$ is a random variable, which fluctuates rapidly about a zero mean. Note that its mean is identically zero because its two constituent means are themselves zero.

Next, we consider the second-order moment of $\Delta u(r; t)$, which we obtain by ensemble average. That is, labelling a single realization of the velocity field by a superscript i, we define the ensemble average by:

$$S_2(r) = \lim_{N\rightarrow \infty} \frac{1}{N}\sum_{i=1}^N\left[\Delta^{\{I\}}u(r,t)\right]^2,$$

where $S_2(r)$ is the second-order structure function of the velocity field. The value of N has to be determined empirically from a consideration of the limit.

It should be clear that the ensemble average involves a summation over many random phases with consequent cancellations. Thus, dogmatic arguments about the form of the `whirls', particularly those arguments which seek to refute the Richardson-Kolmogorov picture, have little place. To argue otherwise is to give undue weight to the one particular eddy structure that one is visualizing. Even so, when Kolmogorov made the assumption that, for values of r that are not too large, the structure function can only depend on the variables r, ε and ν, then that is just an assumption.

The next step, in obtaining the two-thirds law, is to argue that, for very small values of r and/or very large values of the Reynolds number, the dependence on the viscosity can be dropped. This is an empirical matter, as it involves evaluating the appropriate limits, and stems from the Navier-Stokes equation (NSE). While it is often said that Kolmogorov's result does not stem from the NSE, in fact this particular step does.

The final step is to assume that the inertial-range structure function takes the form of a power law and to use dimensional analysis. Evidently, expressing the energy in terms of a rate of change of energy with time necessarily brings in the two-thirds power law. I emphasise that this step is an assumption because in a later post I shall argue that the power-law may have a different status in Onsager's theory.

I am publishing this post a day earlier than usual, as I will be going on holiday on Thursday. My hope is to carry on this series of posts in early June.

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Onsager's (1945) interpretation of Kolmogorov's (1941a) theory: 1

Onsager's (1945) interpretation of Kolmogorov's (1941a) theory: 1

It is a well known fact that as one gets older time speeds up! This (apparent) reverse
time-dilation is something to be reckoned with once one has passed `the big eight-oh'. So much so, that I was convinced that I had posted blogs quite recently only to find that my last post was in January of this year and that was my only post so far this year. Obviously I have my excuses, such as involvement with a special issue of the journal Atmosphere, as mentioned in a previous post. In particular I have been preparing some potential editorial material which I have posted on the physics ArXiV, while I consider what to do with it [1]. However, that work has stimulated various lines of thought which have led to the present post. Or rather, what I

expect to be a series of posts.

My thinking on this goes back to 1999, when I was a participant in the turbulence programme at the Isaac Newton Institute. At one of our informal meetings, there was some discussion of the fact that Kolmogorov had not actually derived the eponymous -5/3 spectrum, but in fact the 2/3 power law for the second-order structure function [2]. This may seem a strange observation to make; but, from the 1950s onwards, theorists had worked exclusively in terms of wavenumber space, and the test of a successful theory was its ability to reproduce the Kolmogorov spectrum. By this stage there was a growing awareness that Kolmogorov had not actually derived the distribution that bears his name. Someone then asked who had derived the -5/3 spectrum. It seemed that no one was quite sure, but there was general agreement that it must have been Obukhov, and there the matter rested.

Nevertheless, this did not seem an entirely satisfactory answer, because Obukhov's paper [3], although in spectral space, introduced a turbulent effective viscosity, in order to calculate the inertial-range spectrum. So that was something that I wondered about. Then it turned out that the answer was hiding in plain sight all the time.

In 1945, Onsager published an abstract [4], giving a brief summary of his method, which identified the key importance the inertial flux of energy through wavenumber. In 1949, he published a fuller account of his method, in which he introduced the idea of the energy transfer being a cascade [5]. In 1964, Corrsin published an exegesis of this work, and even used the term `Onsager's model' in his title [6]. A noteworthy feature of this work is that it relied on the introduction of a modal decay time, which was determined, like the energy spectrum, by dimensional analysis. This is of interest because of its relationship to the renormalization methods being introduced by Kraichnan [7] and Edwards [8] at much the same time.

We shall return to this point in later posts. For the moment, we note that the work of Onsager was brought to a wider audience by the 2006 review of Eyink and Sreenivasan [9], which also pointed out that Onsager had introduced the term `energy cascade', as of course we all use today.

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Is it possible to achieve an infinite Reynolds number?

Is it possible to achieve an infinite Reynolds number?
There has been an increasing awareness in the turbulence community of the significance of finite-Reynolds-number (FRN) effects, corresponding to an impression that Kolmogorov's theory requires an infinite Reynolds number. At the same time, there has been a recent growth of interest in *Onsager's Conjecture*, which is essentially taken to mean that turbulent dissipation is still present even when the Reynolds number is infinite, and this is normally interpreted as being when the fluid viscosity is zero. Oddly, there never seems to be any mention of the word '*limit*'; and one detects a degree of uncertainty about the whole matter, with typical comments like: 'when the Reynolds number is infinite, or at least very large'.

In order to examine this topic, we may begin by remarking that the Newtonian fluids we study always have a finite viscosity. Also, reducing the viscosity seems like an unlikely method of increasing the Reynolds number. If we take pipe flow as an example, the normal procedure is to increase the velocity of the fluid, as being much easier than increasing the pipe diameter or decreasing the fluid viscosity. Nevertheless, the idea of varying the viscosity has been around for a long time, with Batchelor discussing the idea of taking the limit as $\nu \rightarrow 0$, at a constant rate of dissipation. He argued that this would push the effect of viscosity to an infinite value of the wavenumber, i.e. $k=\infty$ [1]. Edwards took the idea further; and, in order to test his statistical theory, argued that the input (due to forces) and the output (due to viscosity) could be represented by delta functions at $k=0$ and $k=\infty$, respectively [2]. However, both examples were in the context of continuum mechanics and, most importantly, involved the taking of limits. That is, the case of $\nu=0$ is the Euler equation, and there is no dissipation. The case $\nu \rightarrow 0$, such that dissipation is maintained constant, involves a limiting process and is the *Navier-Stokes equation in the infinite Reynolds limit*. It is not the Euler equation.

This procedure of taking the viscosity to tend to zero can seem counter-intuitive; but an example that is even more so can be found in microscopic physics, where the classical limit can be obtained by letting Planck's constant h tend to zero. This is definitely counter-intuitive: after all, h is not just a constant, it is a universal constant. The answer to this is that we must be taking a limit where the quantum levels become infinitesimally small in comparison to the energies involved in the macroscopic system.

We can apply the same idea to turbulence. For pipe flow, we can work with a non-dimensional viscosity of the form $\tilde{\nu}=\nu/U d$, where U is the bulk mean velocity and d is the diameter of the pipe. Evidently increasing the velocity is the equivalent of decreasing the scaled viscosity. Moreover, as the scaled velocity is a pure number, concepts of large and small are better defined.

The idea of zero scaled viscosity then corresponds to an infinite value of the velocity and clearly is not achievable. So, in practice, zero scaled viscosity means that it is small enough compared to other relevant quantities that it may be neglected. The best way to do this, is to look for a limit. That is, as the scaled viscosity tends to zero, the dissipation (say) tends asymptotically to a constant value. When variations in the dissipation are too small to resolve, either numerically or experimentally, we have in fact reached a limiting value. This behaviour can be seen in the variation of dissipation rate with increasing Reynolds number in reference [3].

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Postscript. This is my first post in some time because I have been busy with acting as Guest Editor in a special edition of Atmosphere. I hope to post frequently from now on. The link to the journal is:
https://www.mdpi.com/journal/atmosphere/special_issues/FOH7AK5UB1

Special Issue of the journal Atmosphere on Isotropic Turbulence.

Special Issue of the journal Atmosphere on Isotropic Turbulence.

It has been some time since I last posted, and the title of this post suggests the main reason, as being a Guest Editor has proved to be quite time consuming. Nevertheless, I have agreed to be Guest Editor because, although I think it is a rather ambitious project, I also believe that it is very timely.

The Special Issue has the subtitle Recent Advances and Current Challenges, and further details can be found at the link:

https://www.mdpi.com/journal/atmosphere/special_issues/FOH7AK5UB1

Our aim in this Special Issue is to publish papers which can help to identify the most recent advances and what the current challenges are. Hence, we are asking for submissions that have both a pedagogical and a review perspective.

At the moment, I am writing an Editorial to give an overview of the subject and to try to indicate some promising candidates for being regarded as significant advances. Equally, I hope to be able to suggest some topics which can be classified as current challenges.

We already have six promised contributions and two have been posted on the website as tentative titles; and I hope that many more will contribute to this worthy attempt to clear up some long-standing issues in the study of isotropic turbulence. The submission deadline is 29 February 2024. You may send your manuscript now or up until the deadline. Submitted papers should not be under consideration for publication elsewhere. The journal also encourages authors to send a short abstract or tentative title to the Editorial Office in advance (atmosphere@mdpi.com). Further details of how to submit to this Open Access journal may be found at the link for the special issue.

If anyone would like to discuss their ideas for a possible paper with me, I will be very happy to hear from them.

Two-time correlations and temporal spectra: the analysis by Tennekes [1].

Two-time correlations and temporal spectra: the analysis by Tennekes [1].
In this post we take a closer look at the analysis by Tennekes [1] in which he differed from the earlier analysis of Tennekes and Lumley [2] and concluded that large-scale sweeping is the determining factor in the decorrelation of the two-time correlation in the inertial range. As noted in my post of 27 April 2023, this leads (rather confusingly) to a \`$-5/3$' power law for the Eulerian temporal spectrum, when the Kolmogorov form is actually $n=-2$. His starting point is equation (1) in [1], which may be written in our present notation as: \begin{equation}\frac{\partial u_1}{\partial t}=-\left(u_1\frac{\partial u_1}{\partial x_1}+u_2\frac{\partial u_1}{\partial x_2}+u_3\frac{\partial u_1}{\partial x_3}\right),\end{equation} and this is justified by assuming that Taylor's hypothesis of frozen convection applies.

The usual application of Taylor's hyopothesis is to situations where there is a mean or free stream velocity U_1, which is much larger than the turbulent velocity $\mathbf{u}(\mathbf{x},t)$. Then the changes in the velocity field with time at a fixed measuring point could be due to the passage of a frozen pattern of turbulent motion past that point. Hence the local time derivative at a point may be replaced by the convective derivative, thus: \begin{equation}\frac{\partial}{\partial t} \rightarrow -U_1\frac{\partial}{\partial x_1} \quad \mbox{if} \quad U_1 \gg u.\end{equation} Or in the context of spectra,

\begin{equation}k_1 = \omega/U_1.\end{equation} A fuller discussion of this can be found in Section 2.6.5 of [3].

Thus (1) seems a rather extreme application of Taylor’s hypothesis. In fact we can write down an exact expression for ${\partial u_1}/{\partial t}$ by invoking the Navier-Stokes equation. This gives us \begin{equation}\frac{\partial u_1}{\partial t}=-\left(u_1\frac{\partial u_1}{\partial x_1}+u_2\frac{\partial u_1}{\partial x_2}+u_3\frac{\partial u_1}{\partial x_3}\right)-\frac{\partial p}{\partial x_1} + \nu \nabla^2 u_1,\end{equation} where p is the kinematic pressure and ν is the kinematic viscosity. Thus in using equation (1), Tennekes neglects both the pressure and the viscous terms. The latter may seem reasonable, as his main concern was with the inertial range, but it must be borne in mind that the subsequent analysis involves squaring and averaging both sides of equation (1) so the neglect of the viscous term may introduce significant error. However, the neglect of the pressure term is even more concerning, as this is a highly non-local term with the pressure being expressed in terms of integrals of functions of the velocity field over the entire system volume: see Section 2.1 of [3].

This analysis relies on imponderable assumptions about scale separation and statistical independence. Such ideas were discussed much later on, and rather more quantitatively, in the context of mode eliminations and large eddy simulation: see Chapter 8 in the book [4] for an account of this work. It is clear that the analysis by Tennekes has swept a great deal under the carpet. In contrast, the arguments given by Tennekes and Lumley [2] seem, to me at least, more confident and well justified than those given in [1]. In his conclusion, Tennekes remarked on the difference between the two analyses, stating that it was `embarrassing in a personal sense.’ Certainly both sets of arguments might repay closer study.

As a final point, he expresses the view that the implications of [1] support Kraichnan’s view that Lagrangian coordinates are more suited to statistical closure theories than the more usual Eulerian variety. However, it is worth pointing out that all the analyses that support such a view are valid (if at all) only for stationary turbulence, whereas all the numerical assessments of closure theories are restricted to freely decaying turbulence. I intend to go on working on this topic as time permits.

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Two-time correlations and temporal spectra: the Lagrangian case.

Two-time correlations and temporal spectra: the Lagrangian case.
In my previous post on 27 April 2023, I promised to come back to the Lagrangian case. Over the years, I have taken the view that the discussion of the Lagrangian case along with the

Eulerian case, which is the one that is of more practical importance, is an unnecessary complication. At the same time, I have had to acknowledge that the application of these ideas to the assessment of statistical closure theories should take account of the fact that there are Lagrangian theories as well as Eulerian theories. However, there is an interesting point to be made when we compare the treatment in the book by Tennekes and Lumley [1] with the later analysis of Tennekes [2].

In the previous post, we only mentioned the discussion by Tennekes and Lumley [1] of the inertial range behaviour of the Eulerian spectrum. In fact they not only derive the inertial range form of the Lagrangian spectrum, and find it to be the same power law as the Eulerian case, but also obtain a relationship between the constants of proportionality in the two cases.

The crucial step in this work is the equivalence of the two correlations (see Section 8.5 of [1]), where the authors refer back to their discussion of Lagrangian forms in Section 7.1 (actually they incorrectly give this as 7.2). Following their notation, we represent the Lagrangian velocity of a fluid point by $V_{\alpha}(t)$ where $\alpha = 1, 2, \mbox{or}, 3$. Then, they assert that $\langle V_{\alpha}V_{\alpha}\rangle =\langle u_{\alpha} u_{\alpha}\rangle$, where u_{α} is of course the Eulerian velocity; leading on to their equation (8.5.3). This is the step that provides the basis for their assertion of the equivalence of the Eulerian and Lagrangian inertial range spectra.

However, the later work of Tennekes [2] leads to the Eulerian spectrum being different from the Lagrangian form, due to the supposed predominance of sweeping effects. This would seem to be an inconsistency and we will return to this in future posts when we examine the work of Tennekes more closely.

We close by pointing out that in our previous post we noted that the form of two-time correlation being studied in [1] was limited to stationary flows. This point was also made by Hinze [3]: see equation (1.57), page 39 in the first edition. However, in discussing the motion of fluid points in Lagrangian coordinates, Tennekes and Lumley emphasise the need for both homogeneity and stationarity. So in effect this restriction has already been made. We also note that an alternative discussion of the original work by Lumley can be found in Section 12.2 of [4].

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Two-time correlations and temporal spectra

Two-time correlations and temporal spectra
I previously discussed this topic in my posts of 25 February 2021 and 10 March 2022. In the succeeding months I have become increasingly aware that there is dissension in the literature, with people citing the temporal spectrum as ω^{-2}, if the arguments of Kolmogorov apply; and $\omega^{-5/3}$, if convective sweeping applies. Statements about these forms are often made without any supporting reference, so my next step

was to identify the sources; and then try to make a critical assessment of both forms and their relationship to each other. In fact the source of the first result seems to be the book by Tennekes and Lumley [1], while the second form is due to later work by Tennekes [2]. So here I will make a start by outlining the general problem, in order to fix notation and definitions.

We begin with the general two-point, two-time correlation tensor $R_{\alpha\beta}(\mathbf{x},\mathbf{x'};t,t')$, where α and β are the cartesian tensor indices, taking the values $1,2$ or 3. The correlation is defined in terms of the velocity field $u(\mathbf{x},t)$, thus: \begin{equation}R_{\alpha\beta}(\mathbf{x},\mathbf{x'};t,t')=\langle u_\alpha(\mathbf{x},t)u_\beta(\mathbf{x'},t')\rangle, \end{equation} where the angle brackets denote the ensemble average. In everything that follows we will restrict our attention to homogeneous turbulence and consider a fixed point in space. This means that we may simplify the notation by omitting the space variables, and write the correlation tensor as: \begin{equation}R_{\alpha\beta}(\mathbf{x},\mathbf{x'};t,t')=R_{\alpha\beta}(t,t'). \end{equation} Then, for generality, we may introduce the sum and difference variables for the times, as: \begin{equation} \mathcal{T}=(t+t')/2 \quad \mbox{and} \quad \tau = (t'-t). \end{equation} Accordingly, the two-time correlation tensor may be written in the form: \begin{equation} R_{\alpha\beta}(t,t') = R_{\alpha\beta}(\mathcal{T},\tau) \end{equation} We still have one more restriction to make: Tennekes and Lumley restrict their attention to isotropic turbulence, which means that we can replace the correlation tensor by a single scalar correlation function which we will denote by R_E, where the subscript E denotes `Eulerian'. Thus, for isotropic turbulence, \begin{equation}R_{\alpha\beta}(\mathcal{T},\tau) \rightarrow R_E(\mathcal{T},\tau), \end{equation} In a later post we will introduce the Lagrangian correlation function.

Now, at this stage, we have imposed all the restrictions that Tennkekes and Lumley have made in specifying their problem. However their subsequent analysis seems to imply that they are also considering stationary turbulence and this is an important point. We will underline this fact by continuing to treat the problem more generally.

The energy spectrum $\phi_E(\mathcal{T},\omega)$ is defined by the Fourier transform,
$$R_E(\mathcal{T},\tau) = \int_{-\infty}^\infty \exp(i\omega \tau) \phi_E(\mathcal{T},\omega)d\omega,$$
where ω is the angular frequency; and the Fourier pair is completed by:
$$\phi_E(\mathcal{T},\omega)= \frac{1}{2\pi}\int_{-\infty}^\infty \exp(-i\omega \tau)R_E(\mathcal{T},\tau)d\tau.$$

As a preliminary to considering the inertial-range form of $\phi_E(\mathcal{T},\omega)$ we need to establish its dimensions. If we integrate the spectrum over all frequencies, we have:
$$\int_{-\infty}^{\infty}\phi_E(\mathcal{T},\omega)d\omega = U^2(\mathcal{T}),$$
where U is the root mean square velocity. Recall that $\mathcal{T}$ is the clock time, as opposed to the difference time τ. From this relationship it follows that the dimensions of the spectrum are:
$$[\phi_E(\mathcal{T},\omega)] = L^2 T^{-1},$$
where as usual square brackets indicate the dimensions of a quantity.

At this point we assume stationarity, which is in effect what Tennekes and Lumley have done [1] and we omit the dependence on $\mathcal{T}$. Having, in effect, done this, they apply the well known argument of Kolmogorov to limit the dependence of the spectrum to the two independent variables ω and the dissipation rate ε. They state that the only dimensionally consistent result is:
$$\phi_E(\omega) \equiv f(\varepsilon, \omega) = \beta \varepsilon \omega^{-2},$$
where f is some

arbitrary function, assumed to be a power and β is a constant. Checking the dimensions, we find: \begin{equation} [\phi_E(\omega)] = (L^2 T^{-3})T^{2} = L^2 T^{-1}, \end{equation} as required.

Later Tennekes presented a different analysis [2] in which he argued that the inertial-range temporal spectrum would be determined by convective sweeping and this led to the result: \begin{equation}\phi_E(\omega)= \beta_E \varepsilon^{2/3}U^{2/3}\omega^{-5/3}. \end{equation} It is readily verified that this result has the correct dimensions, thus: \begin{equation} [\phi_E(\omega)] = (L^2T^{-1})^{2/3}(LT^{-1})^{2/3}T^{5/3}= L^2T^{-1}.\end{equation}
It should be noted that irrespective of the merits or otherwise of this analysis by Tennekes, it is limited to stationary turbulence in principle due to omission of any dependence on the clock time $\mathcal{T}$. In future posts I intend to give some critical attention to both these theories.

[1] H. Tennekes and J. L. Lumley. A first course in turbulence. MIT Press. Cambridge, Mass., 1972.
[2] H. Tennekes. Eulerian and Lagrangian time microscales in isotropic turbulence. J. Fluid Mech., 87:561, 1975.

Mode elimination: taking the phases into account: 5

Mode elimination: taking the phases into account: 5

When I began this series of posts on the effects of phase, I had quite forgotten that I had once looked into the effects of phase in quite a specific way. This only came back to me when

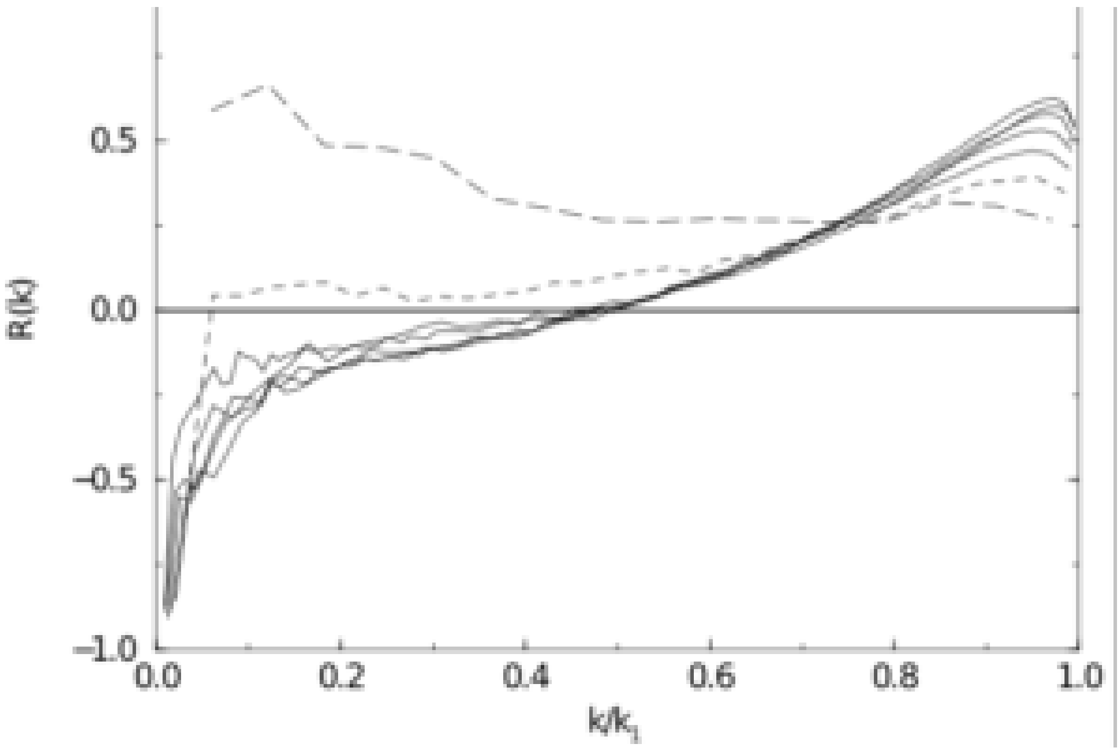
I was using my own book [1] to remind me about conditional averaging. And that book was published as recently as 2014!

In effect, McMillan and Ferziger tested the significance of taking phase into account as long ago as 1979, in the context of sub-grid modelling [2]. They did this by measuring correlations between exact sub-grid stresses and eddy viscosity models. In the case of the Smagorinsky model, which is widely used with reasonable success in shear flows, they found correlations as low as 0.1 – 0.2. Then, in 1998, McComb and Young [3] showed that, for isotropic turbulence at least, low values of the correlations between sub-grid stresses and eddy-viscosity models are due to phase effects. A brief pedagogical demonstration of the need to take phases into account in an eddy-viscosity model can be found in Section 8.7 of [1], but we will not pursue that here; but instead concentrate on the numerical demonstration of the effects of phase.

We carried out a numerical simulation of stationary, isotropic turbulence, with the velocity field in wavenumber defined on the interval $0\leq k \leq k_0$. Various cut-off wave numbers $k_1 \leq k_0$, $k_2 \leq k_1$, $k_3 \leq k_2$; and so on, were considered, so that a series of large-eddy simulations could be compared to the fully resolved simulation. I discussed in my post of 23 March 2023 how the complex velocity field in wavenumber (a.k.a the Fourier transform of the real-space velocity field) could be separated into amplitude and phase; and this was the method employed in [3], from which I have taken three figures. In all cases, we evaluated a correlation coefficient $R(k)$ and this is plotted against $k/k_$, where k_1 is the maximum resolved wavenumber in all cases.

In Figure A, we show the correlation $R(k)$ between the subgrid stresses and the eddy viscosity for seven cut-off wavenumbers in the range $16.5 \leq k_1 \leq 112.5$ with $k_0 = 128$. It can be seen that for most cases (shown by continuous lines) the correlation is not very good, varying from $0.25 – 0.5$ at the cut-off wavenumber to essentially being anti-correlated as $h/k_1 \rightarrow 0$. The exceptions are the curves for the lowest cut-off wavenumbers $k_1 =16.5$ (long dashes) and $k_1= 32.5$ (short dashes); and in particular the first of these. It should be noted that the first of these is the only one to yield a finite plateau region in the plot of the effective viscosity against wavenumber [3]. This latter property is an indication that it is only this lowest cut-off wavenumber which gives an adequate degree of scale separation compared to the maximum value.

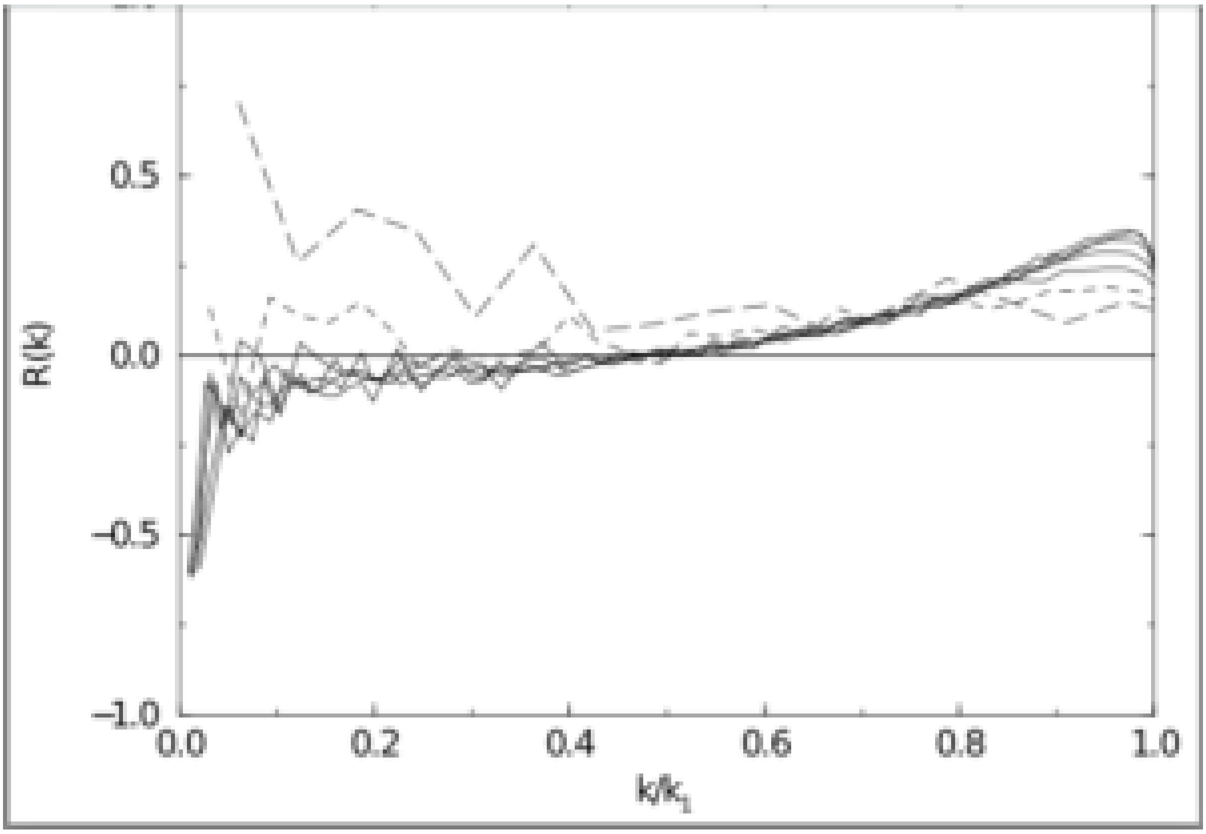
FIG A



Correlation R(k) between subgrid stresses and eddy-viscosity model.

In Figure B, we show the phase correlations for the same cases, and the similarity to the results of Figure A are quite marked.

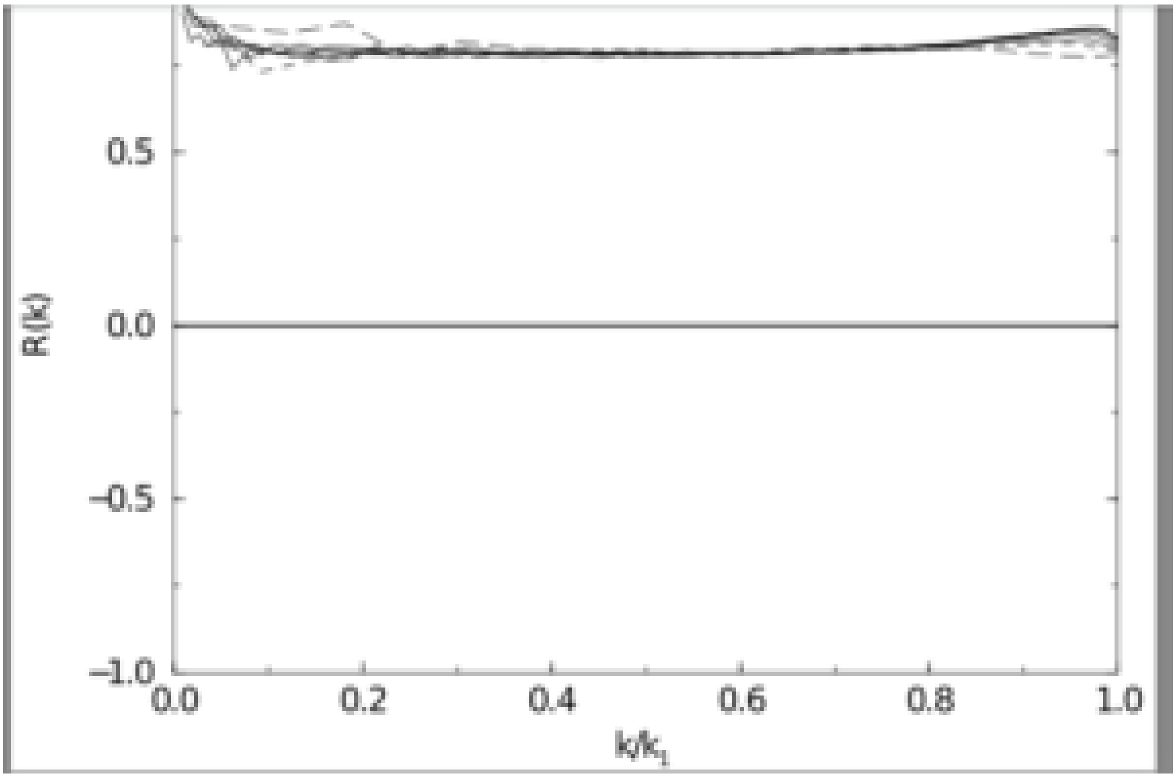
FIG B



The phase correlation R(k) between subgrid stresses and the eddy-viscosity model.

On the other hand, the results for amplitude correlations in Figure C show a high level of correlation over the entire range of wavenumbers, with very little variation between the results for the various cut-off wavenumbers.

FIG C



Amplitude correlations R(k) between subgrid stresses and eddy-viscosity models.

In this case, isotropic turbulence, we are mainly interested in modelling the inertial transfer through wavenumber and for this purpose a model which represents the amplitudes is quite effective. However, given that all such formulations are based on average quantities it is not easy to see how the phases can be taken into account.

[1] W. David McComb. Homogeneous, Isotropic Turbulence: Phenomenology, Renormalization and Statistical Closures. Oxford University Press, 2014.
[2] O. J. McMillan and J. H. Ferziger. Direct testing of subgrid-scale models. AIAA Journal, 17:1340, 1979.
[3] W. D. McComb and A. J. Young. Explicit-Scales Projections of the Partitioned Nonlinear Term in Direct Numerical Simulation of the Navier-Stokes Equation. Presented at 2nd Monte Verita Colloquium on Fundamental Problematic Issues in Turbulence: available at arXiv:physics/9806029 v1, 1998.

Mode elimination: taking the phases into account: 4

Mode elimination: taking the phases into account: 4

In the previous post we came to the unsurprising conclusion that as a matter of rigorous mathematics, we cannot average out the high-wavenumber modes while leaving the low-wavenumber modes unaffected. However, turbulence is a matter of physics rather than pure mathematics and the initial conditions are not known with mathematical precision. Here the concept of deterministic chaos comes to our rescue. If we accept that the initial condition must have some uncertainty attached to it, then there is a possibility that such an average can be carried out approximately.

We can generalise the conditional average, given as equation (3) in the previous post, by extending it to some arbitrary well-behaved functional $H[u(k,t)]$. Here we are also using the simplified notation of the previous post; and in fact we shall simplify it even further, and write $u(k,t)\equiv u_k$. Then we can replace that equation by:\begin{equation}\langle H[u_k]\rangle_c = \langle H[u_k]\mid u^-_k \rangle,\end{equation} where, as before, the subscript \`$c$' on the left hand side denotes \`conditional average'; and the notation on the right hand side indicates that the ensemble average is carried out while keeping the low-wavenumber part of the velocity field $ u^-_k$ constant. From the previous discussion, we know that this average amounts to a delta function, as both u_k and u^+_k are also held constant.

The way out of this impasse is the recognition that, in the real physical situation, u^-_k cannot be held precisely to any exact value. There must be some uncertainty, however small, in the application of this constraint. Accordingly we introduce an uncertainty into our definition of a conditional average by writing it as: \begin{equation}\langle H[u_k]\rangle_c = \langle H[u_k]\mid u^-_k + \phi^-_k\rangle.\end{equation} Evidentally, $ u^-_k + \phi^-_k$ must be a solution of the Navier-Stokes equation, but the uncertainty ϕ^-_k is otherwise arbitrary and may be chosen to have convenient properties. In fact, McComb, Roberts and Watt [1] chose it to satisfy the conditions:\begin{equation} \langle u^-_k\rangle_c = u^-_k + \langle \phi^-_k \rangle_c,\end{equation} along with:\begin{equation} \langle u^-_k u^-_j\rangle_c = u^-_k + \langle \phi^-_k \phi^-_j \rangle_c,\end{equation} and \begin{equation} \langle u^-_ku^+_j\rangle_c = u^-_k \langle u^+_k \rangle_c.\end{equation} These relationships are then used in decomposing the NSE and implementing an RGl calculation. It should be noted that $ \langle u^+_k \rangle_c$ is not zero and an equation of motion must be derived for it.

The problem posed by the correction terms in ϕ^-_k depends on just how chaotic the turbulence is, but the calculations suggest that these terms can be neglected. In fact the calculation of the invariant energy flux yields a value of the Kolmogorov spectral constant of $\alpha = 1.62$ which is the generally accepted value. Further details can be found in the original paper [1] and in the appropriate sections of the book [2].

However, despite the above procedures, there are still phase effects that are not being taken into account, and this will be the subject of the next post.

[1] W. D. McComb, W. Roberts, and A. G. Watt. Conditional-averaging procedure for problems with mode-mode coupling. Phys. Rev. A, 45(6):3507- 3515, 1992.
[2] W. David McComb. Homogeneous, Isotropic Turbulence:

Phenomenology, Renormalization and Statistical Closures. Oxford University Press, 2014.

Mode elimination: taking the phases into account: 3

Mode elimination: taking the phases into account: 3

In this post we look at some of the fundamental problems involved in taking a conditional average over the high-wavenumber modes, while leaving the low-wavenumber modes unaffected.

Let us consider isotropic, stationary turbulence, with a velocity field in wavenumber space which is defined on $0\leq k \leq k_0$. Note that the maximum wavenumber k_0 is not the Kolmogorov dissipation wavenumber, although in both large-eddy simulation and in the application of renormalisation group (RG) to turbulence, it is often taken to be so. The only definition that I know of, is the one I put forward in 1986 [1], which is:\begin{equation}\int^{k_0}_0 2\nu_0 k^2 E(k)dk \approx \int^{\infty}_0 2 \nu_0 k^2 E(k)dk = \varepsilon, \end{equation} where ν_0 is the kinematic viscosity of the fluid, $E(k)$ is the energy spectrum, and ε is the dissipation rate. Obviously the value of k_0 depends on how closely the integral on the left approximates the actual dissipation rate, which corresponds to the upper limit on the integral being taken as infinity. Some people apparently find this definition puzzling, possibly because they are familiar with RG in the context of the theory of critical phenomena, where the maximum wavenumber is determined by the inverse of the lattice constant. In contrast, fluid dynamicists may find our definition here quite intuitive, as it is analogous to Prandtl’s definition of the laminar boundary layer.

Something which may be counter-intuitive for many, is the choice of k_0 as the maximum wavenumber. This is because in RG we progressively eliminate modes in wavenumber bands: $k_1\leq k \leq k_0$, $k_2 \leq k \leq k_1$, $k_3\leq k \leq k_2$, and so on, where k_n decreases with increasing integer n, until the iteration reaches a fixed point. Also, the fluid viscosity ν_0 is so denoted, because it is progressively renormalized until it reaches a value $\nu_{n-1}=\nu_n \equiv \nu_N$, at the fixed point $n=N$.

The first step in eliminating a band of modes is quite straightforward. We high-pass, and low-pass, filter the velocity field at $k=k_1$, thus: \begin{eqnarray} u^{-}(k,t) = u(k,t) \quad \mbox{for} \quad 0 \leq k \leq k_1; \nonumber\\ u^{+} (k,t)= u(k,t) \quad \mbox{for} \quad k_1 \leq k \leq k_2,\end{eqnarray} where we have adopted a simplified notation. Then we can substitute the decomposition given by equation (2) into the Navier-Stokes equation in wavenumber, and study the effect. However we will not pursue that here, and further details can be found in Section 5.1.1 of [2]. Instead, we will concentrate here on the following question: how do we average out the effect of the u^+ modes, while keeping the u^- modes constant?

The condition for such an average can be written as: \begin{equation}\langle u^-(k,t)\rangle_c = u^{-}(k,t),\end{equation}where the subscript `$c$’ denotes `conditional’. We should also recall that isotropic turbulence requires a zero mean velocity, that is: $\langle u(k,t)\rangle=0$.

Actually, it would be quite simple to carry out such an average, provided that the velocity field $u(k,t)$ were multivariate normal. In that case, each of the various modes could be averaged out, independently of all the rest. However, the turbulent velocity field is not Gaussian so, in attempting to carry out a such an average, we would run into the following two problems.

First, we must satisfy the boundary condition between the two regions of k-space. Hence, \begin{equation} u^-(k_1,t)=u^+(k_1,t). \end{equation} This is the extreme case, where we would be trying to average out a high-k mode while leaving the identical low-k mode unaffected. At the very least, this draws attention to the need for scale separation.

Secondly, there are some questions about the nature of the averaging over modes, in terms of the averaging of the velocity field in real space. In order to consider this, let us introduce a combined Fourier transform and filter $F_T^{\pm}(k,x;t)$ acting on the velocity field $u(x,t)$, such that:\begin{equation}u^{\pm}(k,t)=F_T^{\pm}(k,x;t)u(x,t). \end{equation} Noting that both the Fourier transform and the filter are purely deterministic entities, the average can only act on the real-space velocity field, leading to zero!

So it seems that a simple filtered average, as used in various attempts at subgrid modelling or RG applied to turbulence, cannot be correct at a fundamental level. We will see in the next post how the introduction of a particular kind of conditional average led to a more satisfactory situation [3].

[1] W. D. McComb. Application of Renormalization Group methods to the subgrid modelling problem. In U. Schumann and R. Friedrich, editors, Direct and Large Eddy Simulation of Turbulence, pages 67- 81. Vieweg, 1986.
[2] W. David McComb. Homogeneous, Isotropic Turbulence: Phenomenology, Renormalization and Statistical Closures. Oxford University Press, 2014.
[3] W. D. McComb, W. Roberts, and A. G. Watt. Conditional-averaging procedure for problems with mode-mode coupling. Phys. Rev. A, 45(6):3507- 3515, 1992.

Mode elimination: taking the phases into account: 2

Mode elimination: taking the phases into account: 2

In last week's post, we mentioned Saffman's criticism of models like Heisenberg's theory of the energy spectrum in terms of their failure to take the phases into account. In this post we explore this idea and try to elucidate this criticism a little further. We can take Heisenberg's model as representative and an introductory discussion of it can be found in Section 2.8.1 of reference [1]. It is also discussed in Batchelor's book, and he made the general comment about it: `The notion that the small eddies act as an effective viscosity is plausible enough but does not seem a suitable description of the mutual action of eddies whose sizes are of the same order of magnitude.'

In other words, he is expressing the need for what later became known as `*scale separation'*: see, for example Section 5.1.1 of the book [3]. (Note: in last week's post I incorrectly wrote *scale invariance* when I meant *scale separation*. This has now been corrected.)

This is an important observation, and it is related to the way in which the phases come into the problem; but Batchelor did not mention this particular aspect. However, if we wish to be precise about the concept of phase, then we must turn again to Batchelor's book [2], where on page 83 he remarked that the Fourier components of the velocity field are complex, and hence may be written, in the usual way, in terms of an amplitude and a phase. In other words, for any particular wavenumber and time, $u_\alpha(\mathbf{k},t)$ is a complex number.

Accordingly, following Batchelor, and with a change of notation, we may write: \begin{equation} u_\alpha(\mathbf{k},t) = |u_\alpha(\mathbf{k},t)| \exp\{i\theta_\alpha(\mathbf{k},t)},\end{equation}where $i = \surd\{-1\}$, θ_α is the phase, and the Cartesian index α takes the values $\alpha=1,2,3$.

Batchelor then discussed its general importance, remarking that: `The exchanges of energy are dependent, in general, on the relationships between the phases of the different Fourier component as well as on their amplitudes, and it is in the elucidation of the average properties of the phase relations that the key to the determination of the energy spectrum during the decay lies.'

Little more has been said on this aspect of turbulence theory, apart perhaps Kraichnan's frequent use of the term `phase mixing' in his many papers on the direct interaction family of closures: for further details see either of the books [1] or [3]. In the next post we will look more specifically at mode elimination to try to establish what the limitations of the process are.

[1] W. D. McComb. The Physics of Fluid Turbulence. Oxford University Press, 1990.
[2] G. K. Batchelor. The theory of homogeneous turbulence. Cambridge University Press, Cambridge, 2nd edition, 1971.
[3] W. David McComb. Homogeneous, Isotropic Turbulence: Phenomenology, Renormalization and Statistical Closures. Oxford University Press, 2014.

Mode elimination: taking the phases into account: 1

Mode elimination: taking the phases into account: 1

This is my first blog this year, very largely because I have been working on a review article as my contribution to a journal issue commemorating Jack Herring, who died last year. We were asked to include some personal recollections of Jack, in addition to the physics, and I began by remarking that I had first met Jack Herring at the NASA-ICASE workshop on turbulence theory at Virginia Beach in October 1984. Taking part were physicists, mathematicians and engineers; and I, as very much a new boy, was glad to be welcomed into the physicists' group with Jack, Bob Kraichnan and various others.

This was a long time ago, and I only have a few memories of the social interactions, but I do recall that there was a `No-host cocktail hour' when the day's programme finished. I was quite amused by this phrase, which I hadn't met before, and which simply meant that the organisers weren't about to pay for the drinks! Thinking about this reminded me of an interaction with Phillip Saffman, which I think leads to a point of general interest.

My purpose at the workshop was to give a talk on my application of renormalization group (RG) to turbulence and details of that, along with the other talks may be found in the published proceedings [1]. I do not recall much discussion after the talks but I do remember that Philip Saffman stood up when questions were invited after my talk. He pointed out that my method wouldn't work because I hadn't taken the phase into account! When I joined him at the `No host cocktail hour', he said that he hoped that I didn’t mind his comment. I assured him that I didn’t as I had no idea what it meant. We didn’t discuss it further, and spoke of other matters; but it was to act as the grit in the oyster which ultimately leads (one hopes) to a pearl.

At the time I was uneasy about my theory anyway, and began to play safe and classify it as a mean-field theory. After some years of brooding about this, and other things, I saw that eliminating modes from the Navier-Stokes equation, while leaving other modes unaffected, required a nontrivial conditional average. I worked on this with two of my students, and we formulated a conditional average, along with a means of approximating it, which led to a better theory: see references [2]-[4] and also see [5]

However, in recent years I was reading some lecture notes by Saffman from the sixties [6], and I saw that he had made a similar criticism of theories such as that of Heisenberg (see Section 2.8 of the book [7]), which represented the effect on lower wavenumbers of inertial transfer to higher wavenumbers by some model. In Heisenberg’s case, this involved an eddy viscosity hypothesis, but Saffman made the general criticism of all models of this type, which ran as follows.

`Conceptually, the theories are also objectionable as they ignore the phases of the Fourier components and almost regard Fourier components as having a real physical existence, rather than being a mathematical representation of the motion.’

He doesn’t explain any further what he means by this, nor does he mention the lack of scale separation of the kind that justifies the calculation of the viscosity in the kinetic theory of gases. I will return to these points in the next blog post, but what really fascinates me is the cultural dissonance, which seems to take us into the realm of the philosophy of science. This too, I hope to return to in a future post.

[1] W. D. McComb. Renormalization Group Methods and Turbulence. In D. L. Dwoyer, M. Y. Hussaini, and R. G. Voigt, editors, Theoretical Approaches to Turbulence. Springer Verlag, 1985.
[2] W. D. McComb and A. G. Watt. Conditional averaging procedure for the elimination of the small-scale modes from incompressible fluid turbulence at high Reynolds numbers. Phys. Rev. Lett., 65(26):3281-3284, 1990.
[3] W. D. McComb, W. Roberts, and A. G. Watt. Conditional-averaging procedure for problems with mode-mode coupling. Phys. Rev. A, 45(6):3507 -3515, 1992.
[4] W. D. McComb and A. G. Watt. Two-field theory of incompressible-fluid turbulence. Phys. Rev. A, 46(8):4797-4812, 1992
[5] W. D. McComb. Asymptotic freedom, non-Gaussian perturbation theory, and the application of renormalization group theory to isotropic turbulence. Phys. Rev. E, 73:26303-26307, 2006.
[6] P. G. Saffman. Lectures on homogeneous turbulence. In N. Zabusky, editor, Topics in nonlinear physics, pages 485-614. Springer-Verlag, 1968.
[7] W. D. McComb. The Physics of Fluid Turbulence. Oxford University Press, 1990.

What are the first and second laws of turbulence?

What are the first and second laws of turbulence?

Occasionally I still see references in the literature to the

Zeroth Law of Turbulence. The existence of a zeroth law would seem to imply that there is at least a first law as well. But, so far as I know, there are no other laws of turbulence, and hence my question is purely rhetorical.

The so-called zeroth law is the fact the turbulent dissipation tends to a limit as the Reynolds number increases. Some people seem to be obsessed by the fact that this is equivalent to a finite dissipation limit as the viscosity tends to zero. Unfortunately, they become hypnotised by the zero viscosity and completely overlook the word `limit'! This becomes translated into `finite turbulent dissipation at zero viscosity' and is also referred to as the `dissipation anomaly'. If this were true, then it certainly would be anomalous, to say the least. But it isn't true. Turbulent dissipation is ultimately, like all dissipation in fluid systems, the transformation of macroscopic kinetic energy into heat by the action of viscosity. No viscosity means no dissipation.

I do not wish to become hypnotised myself by this particular manifestation of folklore. I have written about it before in these blogs and will write about it again. Right now I wish to concentrate only on the oddity of the terminology: `zeroth law'. Presumably it has been so named by analogy with the situation in thermodynamics, where the well-established first and second laws were later supplemented by both a third law and a zeroth law. The third law was part of the subject when I took my first degree but the zeroth law wasn't. It amounts essentially to a definition of temperature that provides a basis for its measurement. I suppose that it became thought to be so fundamental that it really ought to precede the existing first and second laws.

However, if that was the case, then surely it would be better to name it something like `The fundamental principle of thermodynamics'? The trouble with zeroth law is that zero means nothing. That is, when you don't have any of something, then you have zero.

It is a failure to recognise this that causes confusion about the calendar when a century changes. One needs to realize that there is no `year zero'. Everything is zero to begin with. Then we start counting seconds, minutes, days and 365 days later we have achieved one year which we denote by `1'. When we reach ten years, we have completed a decade, and we can label that year by `10', with zero fulfilling its mathematical significance by giving us a symbol for `10'. Thus the year 10 is the last year of the decade, the year 100 is last year of the century, and the year 1000 is the last year of the millennium. Thus Year 2000 is the last year of the second millennium and Year 2001 is the first year of the third millennium. (I hope that digression made sense!)

In my view, the use of the term `zeroth law' is lame in thermodynamics and doubly lame in turbulence, where we do not even have an agreed first law. It also reflects muddled thinking, based very largely on a failure to understand the mathematical concept of a limit, which ends up with the erroneous supposition that the infinite Reynolds number limit corresponds to the Euler equation. This amounts to a failure to recognize that the Euler equation throughout its entire life has been indomitably non-dissipative.

This will be my last blog of this year. I intend to resume posting in the new year. In the meantime, I hope that we shall all have a pleasant holiday.

The non-Markovian nature of turbulence 9: large-eddy simulation (LES) using closure theories.

The non-Markovian nature of turbulence 9: large-eddy simulation (LES) using closure theories.

In this series of posts we have argued that the three pioneering theories of turbulence (due to Kraichnan, Edwards and Herring, respectively) are all Markovian with respect to wavenumber interactions. Thus, despite their many successful features, the ultimate failure of these theories to give the correct infinite-Reynolds number limit arises from the fact that they cannot reproduce the non-Markovian nature of fluid turbulence. In the immediately preceding post, we drew a distinction between the concept of a process being Markovian in its wavenumber interactions and the `almost-Markovian' nature of certain single-time theories, where the term `Markovian' refers to their development with time. In this final post in the series, we may shed some further light on these matters by considering the use of closures to calculate the subgrid viscosity for a large-eddy simulation.

This activity was initiated in 1976 by Kraichnan [1] who considered isotropic turbulence and based his approach on his own test-field model. In fact this publication led to quite a lot of activity by others, although this was generally based on the very similar EDQNM model (see the previous post).

The LES equations for isotropic turbulence can be formulated in wavenumber space by filtering the velocity field at some fixed cut-off wavenumber k_c. Then, for the explicit (resolved) wavenumbers $k\leq k_c$, we have the resolved velocity field $u^{<}(\mathbf{k,t})$; while the subgrid field takes the form $u^{>}(\mathbf{k,t})$ for $k_c\leq k$. Then substituting into the Navier-Stokes equations, we obtain separate equations for the low-k and high-k, ranges. However, the nonlinear term ensures that the two equations of motion are coupled together. This coupling of explicit and implicit modes is the subgrid modelling problem.

A detailed discussion of these matters may be found in Section 10.3 of the book [2], but here we only wish to sketch out some features of Kraichnan's approach insofar as they bear on the earlier posts in this series. We may do this schematically in terms of the Lin equation, as follows. Evidentally, corresponding to the explicit modes of the velocity field, we may define an explicit modes energy spectral density $C^{<}(k,t)$, and correspondingly the filtered energy spectrum $E^{<}(k,t) = 4\pi k^2 C^{<}(k,t)$. Accordingly we may write the energy balance for the explicit modes as: \begin{equation}\left(\frac{\partial}{\partial t} + 2\nu k^2 \right)E^{<}(k,t) = T^{<}(k,t) + T^{<>}(k,t),\end{equation} where $T^{<}(k,t)$ is the transfer spectrum for the explicit modes and contains only couplings within these modes; whereas $T^{<>}(k,t)$ contains terms involving the implicit modes. Kraichnan proposed [1] that the second transfer term could be modelled in terms of an effective subgrid viscosity $\nu(k|k_c)$, such that \begin{equation}T^{<>}(k,t) \equiv T(k|k_c) =-2\nu(k|k_c)k^2 E^{<}(k,t),\end{equation} where at the same time he introduced the parametric notation shown.

The point that we wish to highlight here is that in using $T(k|k_c)$ Kraichnan only took the output term into acccount. In fact the input term, even if small, must be included. In fact there are circumstances where it is not small and in general $\nu(k_c|k)$ is not positive definite, nor should it be. Thus, an adherence to the Markovian point of view that underpinned the DIA and the other pioneering closures, leads to an incorrect result. A full discussion of this may by found in Section 10.3 of [2] and on page 394 Kraichnan's effective viscosity can be found as equation (10.17), while the corrected form with the input term of the transfer spectrum included may be found as a footnote on page 403 of the same reference.

As a corollary here, on page 392 of [2] I have noted that Kraichnan showed that his first lagrangian theory reduced to a Markovian form under certain circumstances. In the case of the LET theory, I know that it is non-Markovian but I had only assumed that was the case for all the Lagrangian theories. So, at least for the first one, it has been shown to be the case.

[1] R. H. Kraichnan. Eddy-viscosity in two and three dimensions. J. Atmos. Sci., 33:1521, 1976.
[2] W. D. McComb. The Physics of Fluid Turbulence. Oxford University Press, 1990.

The non-Markovian nature of turbulence 8: Almost-Markovian models and theories

The non-Markovian nature of turbulence 8: Almost-Markovian models and theories

Previously, in my post of 10 November 2022, I mentioned, purely for completeness, the work of Phythian [1] who presented a self-consistent theory that led to the DIA. The importance of this for Kraichnan was that it also led to a model representation of the DIA and in turn to the development of what he called `almost-Markovian' theories. Some further discussion of this topic can be found in Section 6.3.2 of the book [2], but here we will concentrate on the general class of almost-Markovian models and theories. My concern here is to draw a distinction between their use of `Markovian', which refers to evolution in time, and my use in this series of posts, which refers to interactions in wavenumber.

This class consists of the Eddy-damped, Quasi-normal, Markovian (EDQNM) model of Orszag in 1970 [3], the test-field model of Kraichnan in 1971 [4], the modified LET theory of McComb and Kiyani in 2005 [5], and the theory of Bos and Bertoglio in 2006 [6]. Here we follow the example of Kraichnan who described a theory which relied on a specific assumption that involved the introduction of an adjustable constant as a *model*. In order to illustrate what is going on in this kind of approach, I will discuss the EDQNM in some detail, as follows.

We begin with the quasi-normal expression for the transfer spectrum $T(k)$ from the Lin equation. This is found to be: \begin{eqnarray}T(k,t) & & =8\pi^2\int d^{3}j\,L\left(\mathbf{k},\mathbf{j}\right)\int_{0}^{t}ds\,R_0 \left(k;t,s\right)R_0\left(j;t,s\right)R_0\left(\left|\mathbf{k}-\mathbf{j}\right|;t,s\right) \nonumber \\& \times &\left[C\left(j,s\right)C\left(\left|\mathbf{k}-\mathbf{j}\right|,s\right)-C\left(k,s\right)C\left(\left|\mathbf{k}-\mathbf{j}\right|,s\right)\right],\label{KWE2} \end{eqnarray} where the viscous response function is given by \[R_0(k;t,t')=\exp[-\nu k^2 (t-t')],\] and the coefficient $L(\mathbf{k,j})$ is defined as: \begin{equation}L(\mathbf{k,j}) = -2M_{\alpha\beta\gamma}(\mathbf{k})M_{\beta\alpha\delta}(\mathbf{j})P_{\gamma\delta}(\mathbf{k-j}),\label{lkj1}\end{equation} and can be evaluated in terms of three scalar variables as \begin{equation}L(\mathbf{k,j}) = -\frac{\left[\mu\left(k^{2}+j^{2}\right)-kj\left(1+2\mu^{2}\right)\right]\left(1-\mu^{2}\right)kj}{k^{2}+j^{2}-2kj\mu},\label{lkj2}\end{equatio n} where μ is the cosine of the angle between the vectors $\mathbf{k}$ and $\mathbf{j}$. For further discussion and details see Appendix C of the book [7].

Now Orszag argued that the failure of QN was basically due to the use of the viscous response function, when in fact one would expect that the turbulence interactions would contribute to the response function. Accordingly he proposed a modified response function: \begin{equation}R(k;t,t')=\exp[-\omega(k)(t-t')],\end{equation}where $\omega(k)$ is a renormalized inverse modal response time. One may note that this is now becoming the same form as that of the Edwards transfer spectrum, but that it is also *ad hoc* and thus there is the freedom to choose $\omega(k)$. After some experimentation using dimensional analysis, Orszag chose the form: \begin{equation}\omega(k)=\nu k^2 + g\left[\int_0^k dj j^2 E(j)\right]^{1/2},\end{equation} where the constant g is chosen to give the correct (i.e. experimental) result for the Kolmogorov spectrum. This is the *eddy damped* part of the model, so replacing R_0 by R gives us the EDQN.

Even with the introduction of the damping term, the EDQN model can still lead to negative spectra. This was cured by introducing the *Markovian* step with respect to time. This rested on the assumption that the characteristic time $[\omega(k) +\omega(j) + \omega(|\mathbf{k-j}|)]^{-1}$ is negligible compared to the evolution time of the products of covariances in the expression for $T(k)$. The equation for the transfer spectrum was Markovianised by replacing the time integral by a memory function $D(k,j;t)$, thus: \begin{equation}T(k,t) =8\pi^2\int d^{3}j\,L\left(\mathbf{k},\mathbf{j}\right) D(k,j;t)\left[C\left(j,s\right)C\left(\left|\mathbf{k}-\mathbf{j}\right|,s\right)-C\left(k,s\right)C\left(\left|\mathbf{k}-\mathbf{j}\right|,s\right)\right],\end{equation} where the memory function is given by \begin{equation}D(k,j;t)= \int_0^t ds \, \exp[\omega(k)+\omega(j)+\omega(|\mathbf{k-j}|)](t-s).\end{equation}This is now the EDQNM model.

When applied to the stationary case, this result for $T(k)$ is identical to the Edwards result, as given in the post of 3 November 2022; but there are crucial differences. The function $\omega(k)$ in the Edwards theory arises from a Markovian theory with respect to wavenumber interactions and is accordingly related to $T(k)$, thus giving the second equation of the closure. In contrast, the function $\omega(k)$ in EDQNM is fixed independently of the transfer spectrum by means of dimensional analysis and accordingly is not Markovian in the sense of the Edwards SCF. It is important to distinguish between the two kinds of Markovianisation.

In our next post, we will conclude this series of posts by discussing how these considerations affect the application of closures to large-eddy simulation.

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The non-Markovian nature of turbulence 7: non-Markovian closures and the LET theory in particular.

The non-Markovian nature of turbulence 7: non-Markovian closures and the LET theory in particular.

We can sum up the situation regarding the failure of the pioneering closures as follows. Their form of the transfer spectrum $T(k)$, with its division into input and output parts, with the latter being proportional to the amount of energy in mode k, is only valid for Markov processes, so it is incompatible with the nature of turbulence which is non-Markovian. It is also incompatible with the phenomenology of turbulence, where the entire $T(k)$ acts as input (or output), depending on the value of k, as I pointed out in 1974 [1]. It is worth noting that the first measurement of $T(k)$ was made by Uberoi in 1963 [2], so turbulence phenomenology was in its infancy at the time the first closures were being developed. In later years, numerical experiments based on high-resolution direct numerical simulations, did not bear out the Markovian picture. In particular, we note the investigation by Kuczaj et al [3]. This is in fact the basic flaw in Kraichnan's DIA and also the SCF theories of Edwards and Herring: the fault lies not in the covariance equations but in the relationship of the response function to them.

As mentioned in the first blog in the present series (posted on 13 October) a response to this problem took, and continues to take, the form of an extension of the DIA approach to

Lagrangian coordinates. A consideration of these theories would take us too far away from our present objective although it should be mentioned that they are non-Markovian in that they are not expressible as Master equations. Instead we will concentrate on the LET theory which exposes the underlying physics of the turbulence energy transfer process.

The LET theory was introduced with the hypothesis that $\omega(k)$ is determined by the entire $T(k)$, not just part of it, and can be defined by a local energy balance [1]. It was extended to the two-time case [4] in 1978; and, less heuristically, in subsequent papers by McComb and co-workers: see [5] for a review. Essentially, the two-time LET theory comprises the DIA covariance equations plus the generalized fluctuation-response relation. It may be compared to Herring’s two-time SCF [6] which comprises the DIA response equation, single-time covariance equation and the generalized fluctuation-response equation. It may also be compared directly to DIA in terms of response equations. However, for our present purposes, we will go back to the simplest case, and show how LET arose in relation to the Edwards SCF.

It was argued by McComb [1], that a correct assignment of the system response in terms of $T(k)$ (i.e. `correct’ in the sense of agreeing with the turbulence phenomenology of energy transfer) could lead to a response function which was compatible with K41. This was found to be the case and, citing the form given in [1], we may write for the turbulence viscosity $\nu_T(k)$: \begin{equation} \nu_T(k)= k^{-2}\int_{j\geq k}d^3 j \frac{L(\mathbf{k,j})C(|\mathbf{k j}|)[C(k)-C(j)]}{C(k)[\omega(k)+\omega(j)+\omega(|\mathbf{k-j}|)]},\label{let-visc}\end{equation} where $\omega(k) = \nu_T(k) k^2$. The lower limit on the integral with respect to j arises when we consider the flux through mode k. It was used in [1] to justify wavenumber expansions leading to differential forms but is not needed here and can be omitted. The interesting point here is made by rewriting this in terms of the Edwards dynamical friction $r(k)$. From equation (5) in the post on 3 November, rewritten as \[\omega(k)=\nu k^2 + \nu_T(k)k^2 =\nu k^2 + r(k),\] we may rewrite (\ref{let-visc}) as: \begin{equation}\nu_T(k)= r(k) – k^{-2}\int d^3 j \frac{L(\mathbf{k,j})C(|\mathbf{k,j}|)C(j)}{C(k)[\omega(k)+\omega(j)+ \omega(|\mathbf{k-j}|)]}. \label{let-visc-rk}\end{equation}

It was shown [1] that the second term in the LET response equation cancelled the divergence in $r(k)$ in the limit of infinite Reynolds number. Hence the term which destroys the Markovian nature of the renormalized perturbation theory is the term which makes the theory compatible with the Kolmogorov $-5/3$ spectrum.

In the next post we will consider the subject of almost-Markovian models, where the term refers to the integrals over time rather than to the energy transfer through wavenumber.

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The non-Markovian nature of turbulence 6: the assumptions that led to the Edwards theory being Markovian.

The non-Markovian nature of turbulence 6: the assumptions that led to the Edwards theory being Markovian.
Turbulence theories are usually referred to by acronyms e.g. DIA, SCF, ALHDIA, \dots, and so on. Here SCF is Herring's theory and, to avoid confusion, Herring and Kraichnan referred to the Edwards SCF as EDW [1]. Later on, when I came to write my first book on turbulence [2], I referred to it as EFP, standing for `Edwards-Fokker-Planck' theory. This seemed appropriate as Edwards was guided by the theory of Brownian motion. But it did not occur to me at the time that the significance of this was that his theory was Markovian with respect to interactions in wavenumber space; nor indeed that a Markovian form was the common denominator in all three of the pioneering Eulerian theories. In recent years, it did occur to me that it was not necessary to be so prescriptive; and if one took a less constrained approach the result was a non-Markovian theory, in fact the LET theory.

Following Edwards [3], we define a model system in terms of a Gaussian distribution $P_0[\mathbf{u}]$, which is chosen such that it is normalised to unity and recovers the exact covariance. That is: \begin{equation}\int \mathcal{D}\mathbf{u} \ P_0[\mathbf{u}] = 1,\end{equation} and \begin{equation}\int \mathcal{D}\mathbf{u} \ P_0[\mathbf{u}]\ u_\mu(\mathbf{k},t) u_\beta(\mathbf{k'},t') = \langle u_\alpha(\mathbf{k},t) u_\beta(\mathbf{k’},t’)\rangle=\delta(\mathbf{k}+\mathbf{k’})C _{\alpha\beta} \mathbf{k};t,t’) \ ,\end{equation} respectively. Then one solves the Liouville equation for the exact probability distribution in terms of a perturbation series with $ P_0[\mathbf{u}]$ as the zero-order term. We will not go into further details here, as we just want to understand how the Edwards theory was constrained to give a Markovian form.

Equations (1) and (2) introduce the two-time covariance. However, in order to explain the Edwards theory, we will consider the single-time case. Also, for sake of simplicity, we will employ the reduced notation of Herring, as used extensively by Leslie [4] and others (see [2]). In this notation we represent the velocity field by X_i, where the index is a combined wave-vector and cartesian tensor index (i.e our $\mathbf{k}$ and α). Accordingly, we introduce the Edwards-Fokker-Planck operator as the sum of single-mode operators, in the form: \begin{equation} L_{EFP} = -\omega_i \frac{\partial}{\partial X_i}\left(X_i + \phi_i \frac{\partial}{\partial X_i}\right), \label{efp} \end{equation} where ω_i is a renormalized eddy decay rate and ϕ_i is the covariance of the velocity field, such that \begin{equation} \phi_i =\int_{-\infty}^\infty X_i^2 P(X_i)dX_i, \end{equation} and P is the exact distribution. Then it is readily verified that the model equation: \begin{equation}L_{EFP}P^{(F)} = 0 \end{equation} has the Gaussian solution \begin{equation} P^{(F)} = \frac{e^{ X_i^2/2\phi_i}}{(2\pi\phi_i)^\frac{1}{2}}. \end{equation}

However, it is important to note, and is also readily verified, that a more general form of the operator L_0, which is given by \begin{equation} L_0 = H(X_i)\left[X_i + \phi_i \frac{\partial}{\partial X_i}\right],\end{equation} where $H(X_i)$ is an arbitrarily chosen well behaved function, also yields the same Gaussian solution for the zero-order equation: \begin{equation}L_0P_0 =0.\label{baseop}\end{equation} Hence at this stage the operator L_0 is not fully determined. Edwards was guided by an analogy with the theory of Brownian motion and in effect made the choice \begin{equation} H(X_i) = -\omega_i \frac{\partial}{\partial X_i}, \end{equation}in order to generate a base operator which could be inverted in terms of an eigenfunction expansion of Hermite polynomials. In this process, the $\{\omega_i\}$ appeared as eigenvalues.

It is this specific choice which over-determines the basic operator which constrained the Edwards theory to be Markovian. More recently it was found that a more minimalist choice, allied to a two-time representation, leads formally to the LET theory [5]. We will consider a more physical basis for the LET theory in the next post.

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The non-Markovian nature of turbulence 5: implications for Kraichnan's DIA and Herring's SCF

The non-Markovian nature of turbulence 5: implications for Kraichnan's DIA and Herring's SCF

Having shown that the Edwards theory is Markovian, our present task is to show that Kraichnan's DIA and Herring's SCF are closely related to the Edwards theory.
However, we should first note that, in the case of the DIA, one can see its Markovian nature by considering its prediction for $T(k,t)$, and this was pointed out by no less a person than Kraichnan himself in 1959 [1]. We may quote the relevant passage as follows:

'The net flow is the resultant of these *absorption* and *emission* terms. It will be noticed that in contrast to the absorption term, the emission terms are proportional to $E(k)$. This
indicates that the energy exchange acts to maintain equilibrium. If the
spectrum level were suddenly raised to much higher than the equilibrium
value in a narrow neighbourhood of k, the emission terms would be
greatly increased while the absorption term would be little affected,
thus energy would be drained from the neighbourhood and equilibrium
re-established. The structure of the emission and absorption terms is
such that we may expect the energy flow to be from strongly to

weakly
excited modes, in accord with general statistical mechanical principles.’

Note that the *absorption* term is what Edwards would call the *input to mode* k *from all other modes*, while the *emission* term is the *loss from mode* k.

Kraichnan’s argument here is essentially a more elaborate version of that due to Edwards, and presents what is very much a Markovian picture of turbulence energy transfer. But, in later years, numerical experiments based on high-resolution direct numerical simulations did not bear that picture out. In particular, we note the investigation by Kuczaj *et al* [2].

Going back to the relationships between theories, in 1964 Kraichnan [3] showed that if one assumed that the time-correlation and response functions were assumed to take exponential forms (with the same decay parameter $\omega(k,t)$), then the DIA reduced to the Edwards theory, although with only two ωs in the denominator of the equation for ω, rather than the three such parameters as found in the Edwards case: see equations (4) and (5) in the previous blog. Thus the arguments used to demonstrate the Markovian nature of the Edwards theory do not actually work for the single-time stationary form of DIA. See also [4], Section 6.2.6. All we establish by this procedure is that the theories are cognate: that is, they have identical equations for the energy spectrum and similar equations for the response function.

Herring’s SCF has been discussed at some length in Section 6.3 of the book [4]. In time-independent form, it is identical to the DIA with assumed exponential time-dependences. The relationship between the two theories can also be demonstrated for the two-time case. The case for the SCF being classified as Markovian seems strong to me. However, there is some additional evidence from other self-consistent field theories.

Balescu and Senatorski [5] actually formulated the problem in terms of a master equation and then treated it perturbatively. Summation of certain classes of diagrams led to the recovery of Herring's SCF. For completeness, we should also mention the work of Phythian [6], whose self-consistent method resembled those of Edwards and Herring. However his introduction of a infinitesimal response function, like that of DIA, meant that his theory ended up re-deriving the DIA equations.

In the next post we will examine the question of how the Edwards theory came to be Markovian. In particular, we will answer the question: what were the relevant assumptions made by Edwards?

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[6] R. Phythian. Self-consistent perturbation series for stationary homogeneous
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The non-Markovian nature of turbulence 4: the Edwards energy balance as a Master Equation.

The non-Markovian nature of turbulence 4: the Edwards energy balance as a Master Equation.

In this post we will rely on the book [1] for background material and further details. We begin with the well-known Lin equation for the energy spectrum in freely decaying turbulence, thus: \begin{equation}\label{Lin}\frac{\partial E(k,t)}{\partial t}= T(k,t) – 2\nu k^2 E(k,t),\end{equation} where $T(k,t)$ is the transfer spectum: see [1] for further details. This equation is the Fourier transform of the better known Karman-Howarth equation in real space. But, although the KHE is a local energy balance in the separation of measuring points r, the Lin equation is not actually a local energy balance in wavenumber space since the transfer spectrum depends on an integral of the triple-moment over all wavenumbers. For explicit forms, see [1].

The energy spectrum is defined in terms of the covariance in wavenumber space (or spectral density) by the well-known relation: \begin{equation} \label{spect}E(k,t)= 4\pi k^2C(k,t),\end{equation} but in theory it is more usual to work in terms of the latter quantity, and accordingly we transform (\ref{Lin}) into \begin{equation}\label{covlin}\frac{\partial C(k,t)}{\partial t}= \frac{T(k,t)}{4\pi k^2} – 2\nu k^2 C(k,t),\end{equation}. The Edwards statistical closure for the transfer spectrum may be written as:\begin{equation}\label{sfecov}\frac{T(k,t)}{4\pi k^2}=2\int d^3 j\frac{ L(k,j)C(|\mathbf{k}-\mathbf{j}|,t)\{C(j,t)-C(k,t)\}}{\omega(k,t)+\omega(j,t)+\omega(|\mathbf{k}-\mathbf{j}|,t)},\end{equation} where $L(k,j)=L(j,k)$ and the inverse modal response time is given by: \begin{equation}\label{sferesponse}\omega(k,t)=\nu k^2 + \int d^3 j\frac{ L(k,j)C(|\mathbf{k}-\mathbf{j}|,t)}{\omega(k,t)+\omega(j,t)+\omega(|\mathbf{k}-\mathbf{j}|,t)}.\end{equation} This controls the loss of energy from mode k, while the term giving the gain to mode k from all the other modes takes the form: \begin{equation}\label{sfegain}S(k,t)=2\int d^3 j\frac{ L(k,j)C(|\mathbf{k}-\mathbf{j}|,t)C(j,t)}{\omega(k,t)+\omega(j,t)+\omega(|\mathbf{k}-\mathbf{j}|,t)}.\end{equation} Then, the Edwards form for the transfer spectral density may be written as:\begin{equation}\label{sfetrans}\frac{T(k,t)}{4\pi k^2}=S(k,t)-2\omega(k,t)C(k,t),\end{equation} and from (\ref{covlin}) the Edwards theory gives the Lin equation as:\begin{equation}\label{sfecovlin}\frac{\partial C(k,t)}{\partial t}=S(k,t)-2\omega(k,t)C(k,t).\end{equation}

Our next step is to compare this to the Master Equation and for simplicity we will consider a quantum system which can exist in any one of a large number of discrete states $|i\rangle$, where i is a positive integer. The relevant equation is the Fermi master equation (see Section 9.1.2 of the book [2]), which may be written as: \begin{equation}\label{fermi}\frac{d p_i}{d t}= \sum_{j}\nu_{ij} p_j – \left\{\sum_j \nu_{ij}\right\}p_i,\end{equation} where: p_i is the probability of the system being in state $|i\rangle$; ν_{ij} is the conditional probability per unit time of the system jumping from state $|i\rangle$ to state $|j\rangle$; and the principle of jump rate symmetry gives us $\nu_{ij}=\nu_{ji}$.

Either from this comparison with the Fermi master equation or from comparison with other master equations such as the Boltzmann equation, it is clear that the Edwards theory of turbulence is a Markovian approximation to turbulence which is itself non-Markovian. The two questions which now arise are: first, what are the implications for the other closures due to Kraichnan and to Herring? And, secondly, how did the Markovian nature of the Edwards theory come about? These will be dealt with in the next two posts?

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The non-Markovian nature of turbulence 3: the Master Equation.

The non-Markovian nature of turbulence 3: the Master Equation.

In the previous post we established that the 'loss' term in the transport equation depends on the number of particles in the state currently being studied. This followed straightforwardly from our consideration of hard-sphere collisions. Now we want to establish that this is a general consequence of a Markov process, of which the problem of N hard spheres is a particular example.

We follow the treatment given in pages 162-163 of the book [1] and consider the case of Brownian motion, as this is relevant to the Edwards self-consistent field theory of turbulence. We again consider a multipoint joint probability distribution and now consider a continuous variable X which takes on specific values x_1 at time t_1, x_2 at time t_2, and in general x_n at time t_n; thus; \[f_n(x_1,t_1; x_2,t_2; \dots x_n,t_n).\] We then introduce the conditional probability density: \[p(x_1,t_1|x_2,t_2),\] which is the probability density that $X=x_2$ at $t=t_2$, given that X had the value $X=x_1$ when $t=t_1\leq t_2$. It is defined by the identity: \begin{equation}f_1(x_1,t_1)p(x_1,t_1|x_2,t_2)=f_2(x_1,t_1; x_2,t_2).\label{pdef}\end{equation}

From this equation (see [1]), we can obtain a general relationship between the single-particle probabilities at different times as: \begin{equation}f_1(x_2,t_2)=\int p(x_1,t_1|x_2,t_2)f_1(x_1,t_1)dx_1. \label{pprop}\end{equation}

Next we formally introduce the concept of a Markov process. We now define this in terms of the conditional probabilities. If: \begin{equation}p(x_1,t_1;x_2,t_2; \dots n_{n-1},t_{n-1}|x_n,t_n)=p(n_{n-1},t_{n-1}|x_n,t_n),\label{markdef}\end{equation}then the current step depends *only* on the immediately preceding step, and not on any other preceding steps. Under these circumstances the process is said to be Markovian.

It follows that the entire hierarchy of probability distributions can be constructed from the single-particle distribution $f_1(x_1,t_1)$ and the transition probability $p(x_1,t_1|x_2,t_2)$. The latter quantity can be shown to satisfy the Chapman-Kolmogorov equation:

\begin{equation}p(x_1,t_1|x_3,t_3)=\int p(x_1,t_1|x_2,t_2)p(x_2,t_2|x_3,t_3) dx_2,\label{ck}\end{equation} indicating the transitive property of the transition probability.

It is of interest to consider two specific cases.

First, for a chain which has small steps between events, the integral relation (\ref{ck}) can be turned into a differential equation by expanding the time dependences to first order in Taylor series. Putting $f_1 = f$ for simplicity, we may obtain: \begin{equation}\frac{\partial f(x_2,t_2)}{\partial t}=\int\, dx_1\left\{W(x_1,x_2)f(x_1,t)-W(x_2,x_1)f(x_2,t)\right\}, \label{me}\end{equation} where $W(x_1,x_2)$ is the rate per unit time at which transitions from state x_1 to state x_2 take place. This is known as the *master equation*.

Secondly, if X is a continuum variable, we can further derive the Fokker-Planck equation as: \begin{equation} \frac{\partial f(x,t)}{\partial t}= \frac{\partial [A(x)f(x,t)]}{\partial x} + \frac{\frac{1}{2}\partial^2[B(x)f(x,t)]}{\partial x^2}. \label{fp}\end{equation} This equation describes a random walk with diffusivity $B(x)$ and friction damping $A(x)$. A discussion of this equation as applied to Brownian motion may be found on pages 163-164 of [1] but we will not pursue that here.

In the next post we will discuss the Edwards theory of turbulence (and by extension the other pioneering theories of Kraichnan and of Herring) in the context of the present work.

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The non-Markovian nature of turbulence 2: The influence of the kinetic equation of statistical physics.

The non-Markovian nature of turbulence 2: The influence of the kinetic equation of statistical physics.

The pioneering theories of turbulence which we discussed in the previous post were formulated by theoretical physicists who were undoubtedly influenced by their background in statistical physics. In this post we will look at one particular aspect of this, the Boltzmann equation; and in the next post we will consider the idea of Markov processes more explicitly.

For many people, a Markov process is associated with the concept of a random walk, where the current step depends only on the previous one and memory effects are unimportant. However, for our present purposes, we will need the more general formulation as developed in the context of the kinetic equations of statistical mechanics. A reasonably full treatment of this topic may be found in chapter four of the book [1], along with some more general references. Here we will only need a brief summary, as follows.

We begin with a system of N particles satisfying Hamilton's equations (e.g. a gas in a box). We take this to be spatially homogeneous, so that distributions depend only on velocities and not on positions. Conservation of probability implies the exact Liouville equation for the N-particle distribution function f_N, but in practice we would like to have the single-particle distribution $f_1(u,t)$. If we integrate out independent variables progressively, this leads to a statistical hierarchy of governing equations, in which each *reduced distribution* depends on the previous member of the hierarchy: a closure problem!

The hierarchy terminates with an equation for the single-point distribution f_1 in terms of the two-particle distribution f_2. This is known as *the kinetic equation*. The kinetic equation for $f_1(x,u,t)$ may be written as: \begin{equation}\frac{\partial f_1}{\partial t} + (u.\nabla) f_1 =\{\mbox{Term involving}\, f_2\}, \end{equation} where x is the position of a single particle, u is its velocity, and ∇ is the gradient operator with respect to the variable x. If we follow Boltzmann and model the gas molecules as hard spheres, then we can assume that the right hand side of the equation is entirely due to collisions. Accordingly, we may write the kinetic equation as: \begin{equation}\frac{\partial f}{\partial t} = \left(\frac{\partial f}{\partial t}\right)_{collisions}, \end{equation} where the convective term vanishes because of the previously assumed homogeneity. Also, we drop the subscript `1’ as we will only be working with the single-particle distribution.

Now let us consider the basic physics of the collisions. We assume that three-body collisions are unlikely and restrict our attention to the two-body case. Assume we have a collision in which a particle with velocity u collides with another particle moving with velocity v, resulting in two particles with velocities $u’$ and $v’$. Evidently this represents a *loss* of one particle from the set of particles with velocity u. Conversely, the inverse two-body collision can result in the *gain* of one particle to the state u. Hence we may interpret the right hand side of (2) as: \begin{equation}\left(\frac{\partial f}{\partial t}\right)_{collisions} = \mbox{Rate of gain to state}\,u\,-

\mbox{Rate of loss from state}\,u. \end{equation}

The right hand side can be calculated using elementary scattering theory, along with the assumption of *molecular chaos* or *stossahlansatz,* in the form $f_2=f_1f_1$; with the result that equation (1) becomes: \begin{equation} \frac{\partial f(u,t)}{\partial t} = \int dv \int d\,\omega \,\sigma_d\,|u-v| \{f(u',t)f(v',t) – f(v,t)f(u,t)\},\end{equation} where σ_d is the differential scattering cross-section, the integral with respect to ω is over scattering angles, and the integral with respect to v stands for integration over all dummy velocity variables.

This is the Boltzmann equation and its key feature from our present point of view is that the rate of loss of particles from the state u depends on the number in that state, as given by $f(u,t)$. We will develop this further in the next post as being a general characteristic of Markovian theories. Of course the present treatment is rather sketchy, but a pedagogic discussion can be found in the book [2], which is free to download from Bookboon.com.

[1] W. D. McComb. The Physics of Fluid Turbulence. Oxford University Press 1990.
[2] W. David McComb. Study Notes for Statistical Physics: A concise, unified overview of the subject. Bookboon, 2014.

The non-Markovian nature of

turbulence 1: A puzzling aspect of the pioneering two-point closures.

The non-Markovian nature of turbulence 1: A puzzling aspect of the pioneering two-point closures.

When I began my postgraduate research on turbulence in 1966, the field had just gone through a very exciting phase of new developments. But there was a snag. These exciting new theories which seemed so promising were not quite correct. They had been found to be incompatible with the Kolmogorov spectrum.

This realisation had come about in stages. When Kraichnan published his pioneering paper in 1959 [1], he carried out an approximate analysis and concluded that his new theory (the direct interaction approximation, or DIA as it is universally known) predicted an inertial range energy spectrum proportional to $k^{-3/2}$. He also concluded that the experimental results available at the time were not sufficiently accurate to distinguish between his result and the well-known Kolmogorov $k^{-5/3}$ form. However, this situation had changed by 1962, with the publication of the remarkable results of Grant et al [2], which exhibited a clear $-5/3$ power law over many decades of wavenumber range.

In 1964, Edwards published a self-consistent field theory which, unlike Kraichnan's DIA, was restricted to single-time correlations [3]. This too turned out to be incompatible with the Kolmogorov spectrum [4]. Edwards attributed the problem to an infra-red divergence in the limit of infinite Reynolds number which, although a different explanation from Kraichnan's, at least also suggested that the problem was associated with low wavenumber behaviour. In 1965, Herring published a self-consistent field theory [5], which was comparable to that of Edwards, although the equation for the renormalized viscosity differed slightly, but not sufficiently to eliminate the infra-red divergence. In passing, I would note that Herring’s self-consistent field method was more general than that of Edwards, and that is a point which I will refer to in later posts in the present series. Also, for completeness, I should mention that Herring later extended his theory to the two-time case and this was found to be closely related to the DIA of Kraichnan [6].

Kraichnan, in a series of papers, responded to this situation by developing variants of his method in Lagrangian coordinates (later on, in collaboration with Herring); and later Lagrangian methods were introduced by Kaneda, Kida & Goto, and most recently Okumura. My own approach began in 1974, in correcting the Edwards theory, which involved the introduction of the local energy transfer (LET) theory and retained the Eulerian coordinate system. All of these theories are compatible with the Kolmogorov spectrum.

My point now, is really one of taxonomy, although it is nonetheless fundamental for all that. How should we classify the theories in order to distinguish between those which are compatible with Kolmogorov and those which are not? In my 1990 book [7], I resorted to the pragmatic classification: *Theories of the first kind* and *Theories of the second kind*; along with a nod to a popular film title! Actually, in recent times, the answer to this question has become apparent, along with the realisation that it has been hiding in plain sight all this time. The clue lies in the Edwards theory and that is the aspect that we shall develop in this series of posts.

The discussion above does not do justice to everything that was going on in this field in the 1960/70s. For instance, I could have mentioned the formalism of Wyld and the well-known EDQNM. Discussions of these, and many more, will be found in my book cited above as [7]. Also, the most recent significant research papers in this field are McComb & Yoffe [8] in 2017 and Okamura [9] in 2018.

[1] R. H. Kraichnan. The structure of isotropic turbulence at very high Reynolds numbers. J. Fluid Mech., 5:497{543, 1959.
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[6] J. R. Herring. Self-consistent field approach to nonstationary turbulence. Phys. Fluids, 9:2106, 1966.
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Work in progress.

Work in progress.

In my blog of 13 August 2020 I posted a `to-do list' that dated from November 2009. None of these jobs ever got done, because other jobs cropped up which had greater priority. I’m fairly confident that this won’t happen with my current `to-do list’ as I see all these jobs as very important and, in a sense, as rounding off my lifetime’s work in turbulence. The list follows below:

[A] Extension of my 2009 analysis of the Kolmogorov spectrum for the stationary case [1] to the case of free decay. It has become increasingly clear in recent years that there are non-trivial differences between stationary isotropic turbulence and freely decaying isotropic turbulence (and grid-generated turbulence is something else again!). As this analysis expresses the pre-factor (i.e. the Kolmogorov constant) in terms of an average over the phases of the system, it is of interest to see whether the peculiarities of free decay affect the pre-factor or the power law (or indeed both).

[**B] Turbulent inertial transfer as a non-Markovian stochastic process and the implications for statistical closures.** In 1974 [2] I diagnosed the failure of the Edwards single-time theory (and by extension Kraichnan’s two-time DIA) as being due to their dividing the transfer spectrum into *input* and *output*. The basis of my local energy transfer (LET) theory was to recognise that at some wavenumbers the *entire* transfer spectrum behaved as an *input* while at other wavenumbers it behaved as an *outpu*t. Subsequently I extended the LET theory to the two-time case by heuristic methods and this formulation was developed by myself and others over many years. However in 2017 [3, 4] I extended the general self-consistent field method of Sam Edwards to the two-time case and re-derived the LET in a more formal way. However, the puzzle was this: why did the Edwards procedure give the wrong answer for the single-time case, but not for the two-time case? I realised at the time (i.e. in 2017) that Edwards had over-determined his base distribution and that his base operator was of unnecessarily high order (see [4]), but it was only recently that the penny dropped and I realised that by specifying the

Fokker-Planck operator, Sam had effectively made a Markovian approximation. This needs to be written up in detail in the hope of throwing some light on the behaviour of statistical closure theories and that is my most urgent task. Please note that the letter `M' in EDQNM refers to the fact that it is Markovian in time.

[**C] Characteristic decay times of the two-time, two-point Eulerian correlation function and the implications for closures.** This is a very old topic which still receives attention: for instance, see [5, 6]. I have intended to get to grips with this for many years, as I have some concerns about the way that it is applied to statistical closures, beginning with the work of Kraichnan on DIA. One suspicion that I have is that the form of scaling is different in the stationary and freely-decaying cases; but I have not seen this point mentioned in the literature.

[**D] Reconsideration of renormalization methods in the light of the transient behaviour of the Euler equation.** I have posted five blogs with remarks on this topic, beginning on the 19 May 2022. My intention now is to combine these remarks into some more or less coherent analysis, as I believe they support my long-held suspicion (more suspicion!) that there are problems with the way in which stirring forces are used in formulating perturbation theories of the Navier-Stokes equations. Of course it is natural to study a dynamical system subject to a random force, but in the case of turbulence the force creates the system as well as sustaining it against dissipation.

This programme should keep me pretty busy so I don't expect to post blogs over the next month or two. However, by the autumn I hope to return to at least intermittent postings.

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turbulence. J.Phys.A, 7(5):632, 1974.
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Turbulence renormalization and the Euler equation: 5

Turbulence renormalization and the Euler equation: 5

In the preceding posts we have discussed the fact that the Euler equation can behave like the Navier-Stokes equation (NSE) as a transient for sufficiently short times [1], [2]. It has been found that spectra at lower wavenumbers are very similar to those of turbulence, and there appears to be a transfer of energy to the `thermal’ modes at higher wavenumbers. This raises some rather intriguing questions about the general class of renormalized perturbation theories which are often interpreted as renormalizing the fluid viscosity. As these theories are broadly in quite good qualitative and quantitative agreement with the observed behaviour of the NSE, they should also be in good agreement with the spectrally-truncated Euler equation, which of course is inviscid. So in this case there is nothing to renormalize!

In effect this latter point has already been demonstrated, in that [1] was based on direct numerical simulation of the Euler equation and [2] used the EDQN model with the viscosity set equal to zero. So this raises doubts about the concept of a renormalized fluid viscosity as an interpretation of the two-point statistical closure theories. As indicated at the end of the previous post, it may be helpful to consider a case where the renormalization of the fluid viscosity is central to the method and therefore unambiguous. This is provided by the application of renormalization group (RG) to turbulence. A background discussion of this method may be found in [3] and a schematic outline was given in my blog post of 7 May 2020. Here we will just summarise a few points.

Consider isotropic turbulence with wavenumber modes in the range $0\leq k\leq k_{max}$. The basic idea is to average out the modes with $k_1\leq k \leq k_{max}$, while keeping those modes with $0\leq k\leq k_1$ constant. It should be emphasised that such an average is a $\emph{conditional}$ average: it is not the same as the usual ensemble or time average. Once calculated, this average can be added to the molecular viscosity in order to represent the effect of the eliminated modes by an effective viscosity on the retained modes. Then the variables are all scaled (Kolmogorov scaling) on the increased viscosity; and the process repeated for a new cut-off wavenumber $k_2< k_1$; and so on, until the effective viscosity ceases to change. The result is a scale-dependent renormalized viscosity.

Now this appears to round off my series of posts on this topic quite well. There is no viscosity in the Euler equation and so we do not have a starting point for RG. It is as simple as that. Any attempt to categorise the energy sink provided by the equilibrium modes by an effective viscosity still does not appear to provide a starting point for RG. On the other hand, unlike in the so-called renormalized perturbation theories, there is no question about the fact that the kinematic viscosity of the fluid is renormalized.

My overall conclusion is a rather vague and open-ended one. Namely, that it would be interesting to consider all the renormalization approaches to turbulence very much in the context of how they look when applied to the Euler equation as well as the NSE, and I hope to make this the subject of further work. Lastly, before finishing I should enter a caveat about RG and also correct a typographical error.

$\emph{Caveat}$: The choice of the wavenumber k_{max} is crucial. The pioneering applications of RG to random fluid motion chose it to be small enough to exclude the turbulence cascade and found a trivial fixed point as $k \rightarrow 0$. This choice rendered the conditional average trivial, as it restricted the formulation to perturbation theory using Gaussian averages, and of course Gaussian distributions factorize. Unfortunately many supposed applications to NSE turbulence also treated the conditional average as trivial. In fact one must choose k_{max} to be large enough to capture all the dissipation, at least to a good approximation.

$\emph{Correction}$: The last word of the first paragraph of my post on 19 May 2022 should have been `viscosity’ not velocity. The correction has been made online.

[1] Cyril Cichowlas, Pauline Bonatti, Fabrice Debbasch, and Marc Brachet. Effective Dissipation and Turbulence in Spectrally Truncated Euler Flows. Phys. Rev. Lett., 95:264502, 2005.
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University Press, 1990.

Turbulence renormalization and the Euler equation: 4

Turbulence renormalization and the Euler equation: 4
In the previous post we mentioned that Kraichnan's DIA theory [1] and Wyld's [2] diagrammatic formalism both depended on the use of an externally applied stirring force to define the response function. This is also true of the later functional formalism of Martin, Siggia and Rose [3]. Both formalisms agree with DIA at second order, and a general discussion of these matters can be found in references [4] and [5]. We also pointed out that this use of applied forces poses problems for the Euler equation. This is because the absence of viscosity means that the kinetic energy will increase without limit. That of course is the reason why numerical studies are limited to the spectrally truncated Euler equation, where the modes are restricted to $0\leq k\leq k_{max}$. So the fact that the Euler equation can behave like the Navier-Stokes equation (NSE) in a transient way not only raises questions about the interpretation of renormalized perturbation theory (RPT) as a renormalization of the molecular viscosity, it also raises doubts about the use of external forces to develop the RPT in the first place.

In the investigations of Cichowlas \emph{et al} [6] and Bos and Bertoglio [7], as discussed in the first of this series of posts on 19 May, the system was given a finite amount of energy which it then redistributed among the modes. For modes with $k\leq k_{th}$, an NSE-like cascade was observed, with a Kolmogorov spectrum; while for $k\geq k_{th}$ the k^2 equipartition spectrum was observed. Obviously, in the absence of viscosity the total energy is constant and the system must move to equipartition for all values of wavenumber. Thus the value of k_{th} separating the two forms of behaviour must tend to zero in, it is reasonable to assume, a finite time.

If we applied stirring forces to the spectrally truncated Euler equation, such that they constituted an energy input at low modes at a rate ε_W, then in the absence of viscosity this could be balanced by a form of dissipation to the equipartition modes, where the energy contained in these modes is given by\begin{equation} E_{th}(t)=\int_{k_{th}}^{k_{}max}\,E(k,t) \,dk, \end{equation} and the dissipation rate by \begin{equation} \varepsilon(t)= dE_{th}(t)/dt,\end{equation} as discussed in reference [6]. Evidently as time goes on, k_{th} will decrease to some minimum value, which would be determined by the peakiness of the input spectrum near the origin, and after that the total energy would increase without limit.

The only way one could maintain a quasi-NSE form of behaviour in the presence of an input term would be by increasing the value of k_{max} and ultimately taking $k_{max} = \infty$. This naturally rules out numerical simulation but possibly some form of limit could be investigated numerically, rather as the infinite Reynolds number limit can be established in numerical simulations. Cichowlas \emph{et al} [6] introduced an analogue of the Kolmogorov dissipation wavenumber k_d such that \begin{equation} k_d \sim \left(\frac{\varepsilon}{E_{th}^{3/2}}\right)^{1/4}k_{max}^{3/4}.\end{equation} This raises the possibility that taking the limit of $k_{max} \rightarrow \infty$ would correspond to the infinite Reynolds number limit which is $\lim \nu \rightarrow 0$ such that $\varepsilon_W = constant$, leading to $k_d \rightarrow \infty$

I will extend the discussion to the use of Renormalization Group (RG) in the next post. In the meantime, for sake of completeness I should mention that there is a school of activity in which RPTs are derived in Lagrangian coordinates. The latest developments in this area, along with a good discussion of its relationship to Eulerian theories, can be found in the paper by Okamura [8].

[1] R. H. Kraichnan. The structure of isotropic turbulence at very high Reynolds numbers. J. Fluid Mech., 5:497{543, 1959.
[2] H. W. Wyld Jr. Formulation of the theory of turbulence in an incompressible fluid. Ann.Phys, 14:143, 1961.
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Turbulence renormalization and the Euler equation: 3

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In the previous post we saw that the mean-field and self-consistent assumptions/approximations are separate operations, although often referred to in the literature as if they could be used interchangeably. We also saw that the screened potential in a cloud of electrons could be interpreted as a Coulomb potential due to a renormalized charge. This type of interpretation was not immediately obvious for the magnetic case and indeed a much more elaborate statistical field theoretic approach would be needed to identify an analogous procedure in this case. It will be helpful to keep these thoughts in mind as we consider the theoretical approach to turbulence by Kraichnan in his DIA theory [1] in 1959. The other two key theories we shall consider are the diagrammatic method of Wyld [2] and the self-consistent field method of Edwards [3]. In what follows, we will adopt a simplified notation. Fuller details may be found in the books [4] or [5].

Kraichnan considered an infinitesimal fluctuation $\delta f(k,t)$ in the driving forces producing a fluctuation in the velocity field $\delta u(k,t)$. He then differentiated the NSE with respect to f to obtain a governing equation for δu, with exact solution: $$\delta u(k,t) = \int_{-\infty}^t \hat{G}(k;t,t') \delta f(k,t')dt',$$ where $\hat{G}$ is the infinitesimal response function. In this work Kraichnan made use of a mean-field assumption, viz. $$\langle \hat{G}(t,t')u(t)u(t')\rangle = \langle\hat{G}(t,t')\rangle \langle u(t)u(t')\rangle = G(t,t') \langle u(t)u(t')\rangle,$$ where G is the response function that is used for the subsequent perturbation theory.

For perturbation theory, a book-keeping parameter λ

(ultimately set equal to unity) is introduced to mutliply the nonlinear term and G is expanded in powers of λ, thus:

$$G(t,t')= G_0(t,t')+\lambda G_1(t,t') + \lambda^2 G_2(t,t') + \dots$$

For the zero-order term, we set the nonlinear term in the Navier-Stokes equation (NSE) equal to zero and the exact solution is:

$$G_0(k,t-t') = \exp[-\nu k^2(t-t')],\quad\mbox{for}\quad t\geq t';$$

where we have now introduced stationarity. This is the viscous response function. So the technique is to calculate an approximation to the exact response function by means of partial summations of the perturbation series to all orders. This can be thought of as renormalizing the viscosity and that interpretation emerges more clearly in the diagrammatic method of Wyld [2].

The work of Wyld is a very straightforward analysis of the closure problem using conventional perturbation theory and a field-theoretic approach. It has received criticism and comment over the years but the underlying problems are procedural and are readily addressed [6]. From our point of view the pedagogic aspects of his formalism are attractive and it is beyond dispute that at second-order of renormalized perturbation theory his results verify those of Kraichnan. This is an important point as Wyld's method does not involve a mean-field approximation.

At this stage it is clear that these two approaches cannot be directly applied to the Euler equation as there is no viscosity, and indeed the idea of forcing it would raise questions which we will not explore here. The interesting point here is that the Edwards self-consistent method does not rely explicitly on viscosity; nor, in the absence of viscosity, does it require stirring forces. Essentially it involves a self-consistent solution of the Liouville equation for the probability distribution of the velocities and, as it was applied to the forced NSE, it actually does involve both viscosity and stirring. Indeed it is known to be cognate with both the Kraichnan and the Wyld theories [4], [5]. Hence, like them it can be interpreted in terms of a renormalization of the viscosity.

These three theories, and other related theories, are all Markovian with respect to wavenumber (as opposed to time). The exception is the Local Energy Transfer (LET) theory [7], which does not divide the nonlinear energy transfer spectrum into input and output parts. Recently it has been found that the application of the Edwards self-consistent field method to the case of two-time correlations leads to a non-Markovian (in wavenumber) theory which has the response function R(t,t') determined by: $$R(t,t') = \left\langle u(t)\tilde{f}(t')\right\rangle_{0},$$ where $\tilde{f}(t)$ is a quasi-entropic force derived from the base distribution and the subscript 0 denotes an average against that distribution. As pointed out in [8], the tilde distinguishes the quasi-entropic force from the stirring force f. Edwards showed that $\langle uf\rangle$ was the rate of doing work by the stirring forces on the velocity field, whereas the new quantity $\langle u\tilde{f}\rangle$ determines the two-time response. It would seem that the LET theory can be applied directly to the Euler equation and this is something I hope to report on in the near future.

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Turbulence renormalization and the Euler equation: 2

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In the early 1970s, my former PhD supervisor Sam Edwards asked me to be the external examiner for one of his current students. It was only a few years since I had been on the receiving end of this process so naturally I approached the task in a merciful way! Anyway, if memory serves, the thesis was about a statistical theory of surface roughness and it cited various papers that applied methods of theoretical physics to practical engineering problems such as properties of polymer solutions, stochastic behaviour of structures and (of course) turbulence. To me this crystallized a problem that was then troubling me. If you regarded yourself as belonging to this approach (and I did), what would you call it? The absence of a recognisable generic title when filling in research grant applications or other statements about one's research seemed to be a handicap.

Ultimately I decided on the term *renormalization methods* but the term *renormalization* did not really come into general use, even in physics, until the success of renormalization group (or RG) in the early 1980s. Actually, the common element in these problems is that one is dealing with systems where the degrees of freedom interact with each other. So, another possible title would be *many-body theory*. We can also expect to observe collective behaviour, which is another possible label. We will begin by looking briefly at two pioneering theories in condensed matter physics, as comparing and contrasting these will be helpful when we go on to the theory of turbulence.

We begin with the Weiss theory of ferromagnetism which dates from 1907 (see Section 3.2 of [1]), in which a piece of magnetic material was pictured as being made up from tiny magnets at the molecular level. This predates quantum theory and nowadays we would think in terms of lattice spins. There are two steps in the theory. First there is the mean field approximation. Weiss considered the effect of an applied magnetic field B producing a magnetization M in the specimen, and argued that the tendency of spins to line up spontaneously would lead to a molecular field B_m, such that one could expect an effective field B_E, such that: \[B_E = B + B_m.\] This is the *mean-field approximation*.

Then Weiss made the assumption \[B_m\propto M.\] This is the *self-consistent approximation*. Combining the two, and writing the magnetization as a fraction of its saturation value M_∞, an updated treatment gives: \[\frac{M}{M_\infty}= \tanh\left[\frac{JZ}{kT}\frac{M}{M_\infty}\right],\] where J is the strength of interaction between spins, Z is the number of nearest neighbours of any one spin, k is the Boltzmann constant and T is the absolute temperature. This expression can be solved graphically for the value of the critical temperature T_C: see [1].

Our second theory dates from 1922 and considers electrons (in an electrolyte, say) and evaluates the effect all the other electrons on the potential due to any one electron. For any one electron in isolation, we have the Coulomb potential, thus: \[V(r)\sim \frac{e}{r}\] where e is the electronic charge and r is distance from the electron. This theory too has mean-field and self-consistent steps (see [1] for details) and leads to the so-called screened potential, \[V_s(r) \sim \frac{e \exp[-r/l_D]}{r},\] where l_D is the Debye length and depends on the electronic charge and the number density of electrons. This potential falls off much faster than the Coulomb form and is interpreted in terms of the screening effect of the cloud of electrons round the one that we are considering.

However, we can interpret it as a form of *charge renormalization*, in which the free-field charge e is replaced by a charge which has been renormalized by the interactions with the other electrons, or:\[e \rightarrow e \times \exp[-r/l_D].\] Note that the renormalized charge depends on r and this type of scale dependence is absolutely characteristic of renormalized quantities. In the next blog post we will discuss statistical theories of turbulence in terms of what we have learned here. For sake of completeness, we should also mention here that the idea of an `effective’ or `apparent’ or `turbulence’ viscosity was introduced in 1877 by Boussinesq. For details, see the book by Hinze [2]. This may possibly be the first recognition of a renormalization process.

[1] W. D. McComb. Renormalization Methods: A Guide for Beginners. Oxford University Press, 2004.
[2] J. O. Hinze. Turbulence. McGraw-Hill, New York, 1st edition, 1959. (2nd edition, 1975).

Turbulence renormalization and the Euler equation: 1

Turbulence renormalization and the Euler equation: 1

The term renormalization comes from particle physics but the concept originated in condensed matter physics and indeed could be said to originate in the study of turbulence in the late 19th and early 20th centuries. It has become a dominant theme in statistical theories of turbulence since the mid-20th century, and a very simple summary of this can be found in my post of 16 April 2020, which includes the sentence: `In the case of turbulence, it is probably quite widely recognized nowadays that an effective viscosity may be interpreted as a renormalization of the fluid kinematic viscosity.' Some further discussion (along with references) may be found in my posts of 30 April and 7 May 2020, but the point that concerns me here, is how can renormalization apply to the Euler equation when its relationship to the Navier-Stokes equation (NSE) corresponds to zero viscosity?

It is well known that a randomly excited and spectrally truncated Euler system corresponds to an equilibrium ensemble in statistical mechanics. This means that it must exhibit energy equipartition at long times (depending on initial conditions) with the spectral energy density $C(k) = A$, where A is constant, and therefore the energy spectrum taking the form $E(k) \sim A k^2$. Indeed this was demonstrated as long ago as 1964 by Kraichnan in the course of testing his DIA statistical closure [1]. However, in 1993, She and Jackson studied a constrained Euler system in the context of reducing the number of degrees of freedom needed to describe Navier-Stokes turbulence [2]. This involves an Euler equation restricted to wavenumber modes $k_{min}\leq k \leq k_{max}$ which is embedded in a forced NSE system, with nonlinear transfer of energy in from the forced modes with $k<k_{min}$

and nonlinear dissipation of energy to modes with $k>k_{max}$, where viscous dissipation is present. This is a very interesting paper which I mention here for completeness and hope to return to at some later time. For the moment I want to concentrate on two rather simpler, but still important, studies of the incompressible Euler equation [3], [4].

In physical terms, it is well known that the presence of the viscous term in the NSE, with its k^2 weighting, breaks the symmetry of the nonlinear term common to both equations, and ensures a mean flux of energy in the direction of increasing wavenumber. This symmetry can also be broken by adopting as initial condition some energy spectrum which does not correspond to the equipartition solution. The resulting evolution of a system peaked initially near, but not at, the origin is shown in [1], along with a good discussion of the behaviour of the Euler equation as related to the NSE. Evidently the Euler equation may behave like the NSE as a transient, ultimately tending to equipartition. This behaviour has been studied by Cichowlas et al [3], using direct numerical simulation; and by Bos and Bertoglio [4], using the EDQNM spectral closure. They both find long-lived transients in which at smaller wavenumbers there is a Kolmogorov-type $k^{-5/3}$ spectrum and at higher wavenumbers an equipartition k^2 spectrum. In both cases, the equipartition range is seen as acting as a sink, and hence giving rise to an effect like molecular viscosity.

In [4], as in [2], there is some consideration of the relevance to large-eddy simulation, but it should be noted that in both investigations the explicit scales are not subject to molecular viscosity or its analogue. For sake of contrast we note that the operational treatment of NSE turbulence by Young and McComb [5] provides a subgrid sink for explicit modes which are governed by the NSE. It may not be a huge difference in practice, but it is important to be precise about these matters.

However, in the present context the really interesting aspect of [3] and [4] is that, in the absence of viscosity they obtain the sort of turbulent spectrum which may be interpreted in terms of an effective turbulent viscosity, and hence in terms of self-renormalization. In the next post, we will examine this further, beginning with a more detailed look at what is meant by the term renormalization.

[1] R. H. Kraichnan. Decay of isotropic turbulence in the Direct-Interaction Approximation. Phys. Fluids, 7(7):1030-1048, 1964.
[2] Z.-S. She and E. Jackson. Constrained Euler System for Navier-Stokes Turbulence. Phys. Rev. Lett., 70:1255, 1993.
[3] Cyril Cichowlas, Pauline Bonatti, Fabrice Debbasch, and Marc Brachet. Effective Dissipation and Turbulence in Spectrally Truncated Euler Flows. Phys. Rev. Lett., 95:264502, 2005.
[4] W. J. T. Bos and J.-P. Bertoglio. Dynamics of spectrally truncated inviscid turbulence. Phys. Fluids, 18:071701, 2006
[5] A. J. Young and W. D. McComb. Effective viscosity due to local turbulence interactions near the cutoff wavenumber in a constrained numerical simulation. J. Phys. A, 33:133-139, 2000.

Alternative formulations for statistical theories: 2.

Alternative formulations for statistical theories: 2.

Carrying on from my previous post, I thought it would be interesting to look at the effect of the different formulations on statistical closure theories. In order to keep matters as simple as possible, I am restricting my attention to single-time theories and their forms for the transfer spectrum $T(k,t)$ as it occurs in the Lin equation (see page 56 in [1]). For instance, the form for this due to Edwards [2] may be written in terms of the spectral energy density $C(k,t)$ (or spectral covariance) as: \begin{equation}T(k,t) = 4\pi k^{2}\int d^{3}j L(k,j,|\mathbf{k}-\mathbf{j}|)D(k,j,|\mathbf{k}-\mathbf{j}|)C(|\mathbf{k}-\mathbf{j}|,t)[C(j,t)-C(k,t)],\end{equation}where \begin{equation}D(k,j,|\mathbf{k}-\mathbf{j}|) = \frac{1}{\omega(k,t)+\omega(j,t)+\omega(|\mathbf{k}-\mathbf{j}|,t)},\end{equation}and $\omega(k,t)$ is the inverse modal response time. The geometric factor $L(\mathbf{k},\mathbf{j})$ is given by:\begin{equation}L(\mathbf{k},\mathbf{j}) = [\mu(k^{2}+j^{2})-kj(1+2\mu^{2})]\frac{(1-\mu^{2})kj}{k^{2}+j^{2}-2kj\mu},\end{equation} and can be seen by inspection to have the symmetry:\begin{equation}L(\mathbf{k},\mathbf{j}) = L(\mathbf{j,}\mathbf{k}).\end{equation}From this it follows, again by inspection, that the integral of the transfer spectrum vanishes, as it must to conserve energy.

Edwards derived this as a self-consistent mean-field solution to the Liouville equation that is associated with the Navier-Stokes equation, and specialised it to the stationary case. Later Orszag [3] derived a similar form by modifying the quasi-normality theory to obtain a closure called the eddy-damped quasi-normality markovian (or EDQNM) model. Although physically motivated, this was an *ad hoc* procedure and involved an adjustable constant. For this reason it is strictly regarded as a *model* rather than a *theory*. As this closure is much used for practical applications, we write in terms of the energy spectrum $E(k,t)=4\pi k^2 C(k,t)$ as:\begin{equation}T(k,t) = \int _{p+q=k} D(k,p,q)(xy+z^{3}) E(q,t)[E(p,t)pk^{2}-

E(k,t)p^{3}]\frac{dpdq}{pq},\end{equation}where \begin{equation}D(k,p,q) = \frac{1}{\eta(k,t)+\eta(p,t)+\eta(q,t)}, \end{equation}and $\eta(k,t)$ is the inverse modal response time (equivalent to $\omega(k,t)$ in the Edwards theory, but determined in a different way). Also $(xy+z^{3})$ is a geometric factor, where x, y and z are the cosines of the angles of the triangle subtended, respectively, by k, p and q.

My point here is that Orszag, like many others, followed Kraichnan rather than Edwards and it is clear that you cannot deduce the conservation properties of this formulation by inspection. I should emphasise that the formulation can be shown to be conservative. But it is, in my opinion, much more demanding and complicated than the Edwards form, as I found out when beginning my postgraduate research and I felt obliged to plough my way through it. At one point, Kraichnan acknowledged a personal communication from someone who had drawn his attention to an obscure trigonometrical identity which had proved crucial for his method. Ultimately I found the same identity in one of my old school textbooks [5]. The authors, both masters at Harrow School, had shown some prescience, as they noted that this identity was useful for applications!

During the first part of my research, I had to evaluate integrals which relied on the cancellation of pairs of terms which were separately divergent at the origin in wavenumber. At the time I felt that Kraichnan’s way of handling the three scalar wavenumbers would have been helpful, but I managed it nonetheless in the Edwards formulation. Later on I was to find out, as mentioned in the previous blog, that there were in fact snags to Kraichnan’s method too.

In 1990 [4] I wrote about the widespread use of EDQNM in applications. What was true then is probably much more the case today. It seems a pity that someone does not break ranks and employ this useful model closure in the Edwards formulation, rather than make *ad hoc* corrections afterwards for the case of wavenumber triangles with one very small side.

[1] W. David McComb. Homogeneous, Isotropic Turbulence: Phenomenology, Renormalization and Statistical Closures. Oxford University Press, 2014.
[2] S. F. Edwards. The statistical dynamics of homogeneous turbulence. J. Fluid Mech., 18:239, 1964.
[3] S. A. Orszag. Analytical theories of turbulence. J. Fluid Mech., 41:363, 1970.
[4] W. D. McComb. The Physics of Fluid Turbulence. Oxford University Press, 1990.
[5] A. W. Siddons and R. T. Hughes. Trigonometry: Part 2 Algebraic Trigonometry. Cambridge University Press, 1928.

Alternative formulations for statistical theories: 1.

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In the spectral representation of turbulence it is well known that interactions in wavenumber space involve triads of wave vectors, with the members of each triad combining to form a triangle. It is perhaps less well known that the way in which this constraint is handled can have practical consequences. This was brought home to me in 1984, when we published our first calculations of the Local Energy Transfer (LET) theory [1].

Our goal was to compare the LET predictions of freely decaying isotropic turbulence with those of Kraichnan's DIA, as first reported in 1964 [2]. With this in mind, we set out to calculate both DIA and LET under identical conditions; and also to compare out calculations of DIA with those of Kraichnan, in order to provide a benchmark. We applied the Edwards formulation [3] of the equations to both theories; but, apart from that, in order to ensure strict comparability we used exactly the same numerical methods as Kraichnan. Also, three of our initial spectral forms were the same as his, although we also introduced a fourth form to meet the suggestions of experimentalists when comparing with experimental results.

Reference should be made to [1] for details, but predictions of both theories were in line with experimental and numerical results in the field, with LET tending to give greater rates of energy transfer (and higher values of the evolved skewness factor) than DIA, which was assumed to be connected with its compatibility with the Kolmogorov spectrum. However, our calculation of the DIA value of the skewness was about 4% larger than Herring and Kraichnan found [4], which could only be explained by the different mathematical formulation.

Let us consider the two different ways of handling the wavenumber constraint, as follows.

Kraichnan's notation involved the three wave vectors $\mathbf{k}$, $\mathbf{p}$, and $\mathbf{q}$; and used the identity: \begin{equation}\int d^3p\int d^3q \,\delta(\mathbf{k}-\mathbf{p}-\mathbf{q})f(k,p,q)=\int_{p+q=k}dpdq\frac{2\pi pq}{k}f(k,p,q), \end{equation} where the constraint is expressed by the Dirac delta function and $ f(k,p,q)$ is some relevant function. Note that the domain of integration is in the (p,q) plane, such that the condition $p+q=k$ is always satisfied.

Edwards [3] used a more conventional notation of $\mathbf{k}$, $\mathbf{j}$, and $\mathbf{l}$; and followed a more conventional route of simply integrating over one of the dummy wave vectors in order to eliminate the delta function, thus: \begin{equation}\int d^3j\int d^3l \,\delta(\mathbf{k}-\mathbf{j}-\mathbf{l})f(k,j,l)=\int_0^\infty 2\pi j^2 dj\int_{-1}^{1}d\mu f(k,j,|\mathbf{k}-\mathbf{j}|),\end{equation}where $\mu = \cos \theta_{kj}$ and θ_{kj} is the angle between the vectors $\mathbf{k}$ and $\mathbf{j}$.

Of course the two formulations are mathematically equivalent. Where differences arise is in the way they handle rounding and truncation errors in numerical procedures. It was pointed out by Kraichnan [2], that corrections had to be made when triangles took the extreme form of having one side very much smaller than the other two. If this problem can lead to an error of about 4%, then it is worth investigating further. I will enlarge on this matter in my next post.

[1] W. D. McComb and V. Shanmugasundaram. Numerical calculations of decaying isotropic turbulence using the LET theory. J. Fluid Mech., 143:95-123, 1984.
[2] R. H. Kraichnan. Decay of isotropic turbulence in the Direct-Interaction Approximation. Phys. Fluids, 7(7):1030{1048, 1964.
[3] S. F. Edwards. The statistical dynamics of homogeneous turbulence. J. Fluid Mech., 18:239, 1964.
[4] J. R. Herring and R. H. Kraichnan. Comparison of some approximations for isotropic turbulence Lecture Notes in Physics, volume 12, chapter Statistical Models and Turbulence, page 148. Springer, Berlin, 1972.

From minus five thirds in wavenumber to plus two-thirds in real space.

From $k^{-5/3}$ to $x^{2/3}$.

From time to time, I have remarked that all the controversy about Kolmogorov's (1941) theory arises because his real-space derivation is rather imprecise. A rigorous derivation relies on a wavenumber-space treatment; and then, in principle, one could derive the two-thirds law for the second-order structure function from Fourier transformation of the minus five-thirds law for the energy spectrum. However, the fractional powers can seem rather daunting and when I was starting out I was fortunate to find a neat way of dealing with this in the book by Hinze [1].

We will work with $E_1(k_1)$, the energy spectrum of longitudinal velocity fluctuations, and $f(x_1)$, the longitudinal correlation coefficient. Hinze [1] cites Taylor [2] as the source of the cosine-Fourier transform relationship between these two quantities, thus:\begin{equation}U^2 f(x) = \int_0^\infty\, dk_1\,E_1(k_1) \cos(k_1x_1),\end{equation}and \begin{equation}E_1(k_1) =\frac{2}{\pi}\int_0^\infty\, dx_1\, f(x_1) \cos(k_1x_1),\end{equation} where U is the root mean square velocity.

In general, the power laws only apply in the inertial range, which means that we need to restrict the limits of the integrations. However, Hinze obtained a form which allows one to work with the definite limits given above, and reference should be made to page 198 of the first edition of his book

[1] for the expression: \begin{equation}U^2\left[1-f(x_1)\right] = C \int_0^\infty\,dk_1\,k_1^{-5/3}\left[1-\cos(k_1x_1)\right],\label{hinze} \end{equation} where C is a universal constant.

The trick he employed to evaluate the right hand side is to make the change of variables:\begin{equation}y= k_1x_1 \quad \mbox{hence} \quad dk_1 =\frac{dy}{x_1}. \end{equation} With this substitution, the right hand side of equation (\ref{hinze}) becomes: \begin{equation}\mbox{RHS of (3)} = C x_1^{2/3}\,\int_0^\infty \,dy [1-\cos y].\end{equation} Integration by parts then leads to: \begin{equation}\int_0^\infty \,dy [1-\cos y]=\frac{3}{2}\int_0^\infty\, dy\, y^{-2/3}\, \sin y =\frac{3}{4}\Gamma(1/3),\end{equation} where Γ is the gamma function. Note that I have omitted any time dependence for sake of simplicity, but of course this is easily added.

[1] J. O. Hinze. Turbulence. McGraw-Hill, New York, 1st edition, 1959. (2nd edition, 1975).
[2] G. I. Taylor. Statistical theory of turbulence. Proc. R. Soc., London, Ser.A, 151:421, 1935.

Compatibility of temporal spectra with Kolmogorov (1941) and with random

sweeping

Compatibility of temporal spectra with Kolmogorov (1941) and with random sweeping.

I previously wrote about temporal frequency spectra, in the context of the Taylor hypothesis and a uniform convection velocity of U_c, in my post of 25 February 2021. At the time, I said that I would return to the more difficult question of what happens when there is no uniform convection velcocity present. I also said that this would not necessarily be next week, so at least I was right about that.

As in the earlier post, we consider a turbulent velocity field $u(x,t)$ which is stationary and homogeneous with rms value U. This time we just consider the dimensions of the temporal frequency spectrum $E(\omega)$. We use the angular frequency $\omega = 2\pi n$, where n is the frequency in Hertz, in order to be consistent with the usual definition of wavenumber k. Integrating the spectrum, we have the condition: \begin{equation}\int_0^\infty E(\omega) d\omega = U^2,\end{equation}which gives us the dimensions: \begin{equation}\mbox{Dimensions of}\; E(\omega)d\omega = L^2 T^{-2};\end{equation} or velocity squared.

For many years, the literature relating to the wavenumber-frequency correlation $C(k,\omega)$ has been dominated by the question: is decorrelation due to random sweeping effects, which would mean that the characteristic time is the sweeping timescale $(Uk)^{-1}$; or is it characterised by the Kolmogorov timescale $(\varepsilon^{1/3}k^{2/3})^{-1}$? A recent article [1] makes a typical point about the consequences for the frequency spectrum of the dominance of the sweeping effect: '… the frequency energy spectrum of Eulerian velocities exhibits a $\omega^{-5/3}$ decay, instead of the ω^{-2} expected from K41 scaling'. Which is counter-intuitive at first sight! As we saw in my blog of

26/02/21, for the case of uniform convection $\omega^{-5/3}$ is associated with K41.

Let us begin by clearing up the latter point. The authors of [1] cite the book by Monin and Yaglom, but I was unable to find it. (I mean the reference, not the book which is quite conspicuous on my bookshelves. I think that anyone giving a reference to a book, should cite the page number. Sometimes I do that and sometimes I forget!) In any case, it is easy enough to work out. From equation (2) we have the dimensions of $E(\omega)$ as $L^{2}T^{-1}$. From the K41 approach we can write for the inertial range: \begin{equation}E(\omega) \sim \varepsilon^{n}\omega^{m} \sim \varepsilon \omega^{-2},\end{equation} where we fixed the dependence on the index n first.

The interest in random convective sweeping mainly stems from Kraichnan's analysis of his direct-interaction approximation (DIA), dating back to 1959. A general discussion of this will be found in the book [2], but we can take a shortcut by noting that Kraichnan obtained an approximate solution for the reponse function $G(k,\tau)$ of his theory (see page 219 of [2]) as: \begin{equation}G(k,\tau)=\frac{exp(-\nu k^2\tau)J_1(2Uk\tau)}{Uk\tau},\end{equation} where $\tau = t-t'$, ν is the kinematic viscosity, and J_1 is a Bessel function of the first kind. The interesting thing about this is that the K41 characteristic time for the inertial range does not appear. Also, in the inertial range, the exponential factor can be put to one, and the decay is determined by the sweeping time $(Uk)^{-1}$.

Corresponding to this solution for the inertial range, the energy spectrum takes the form: \begin{equation} E(k) \sim (\varepsilon U)^{1/2}k^{-3/2},\end{equation} as given by equation (6.50) in [2]. As is well known, this $-3/2$ law is sufficiently different from the observed form, which is generally compatible with the K41 $-5/3$ wavenumber spectrum, to be regarded as incorrect. We can obtain the frequency

spectrum corresponding to the random sweeping hypothesis by simply replacing the convective velocity U_c, as used in Taylor's hypothesis, by the rms velocity U. From equation (8) of the earlier blog, we have; \begin{equation}E(\omega) \sim (\varepsilon U_c)^{2/3}\omega^{-5/3} \rightarrow (\varepsilon U)^{2/3}\omega^{-5/3} , \quad \mbox{when} \quad U_c \rightarrow U. \end{equation}
This result is rather paradoxical to say the least. In order to get a $-5/3$ dependence on frequency, we have to have a $-3/2$ dependence on wavenumber! It is many years since I looked into this and in view of the continuing interest in the subject, I have begun to rexamine it. For the moment, I would make just one observation.

Invoking Taylor's expression for the dissipation rate, which is: $\varepsilon = C_\varepsilon U^3/L$, where L is the integral lengthscale (not to be confused with the symbol for the length dimension) and C_ε asymptotes to a constant value for Taylor-Reynolds numbers $R_\lambda \sim 100$ [3], we may examine the relationship between the random sweeping and K41 timescales. Substituting for the rms velocity, have: \begin{equation}\tau_{sweep} =(Uk)^{-1}\sim (\varepsilon^{1/3}L^{1/3}k)^{-1}.\end{equation} Then, putting $k\sim 1/L \equiv k_L$, we obtain:\begin{equation}\tau_{sweep}\sim (\varepsilon^{1/3}k_L^{2/3})^{-1} = \tau_{K41}(k_L).\end{equation} So the random sweeping timescale becomes equal to the K41 timescale for wavenumbers in the energy-containing range. Just to make things more puzzling!

[1] A. Gorbunova, G. Balarac, L. Canet, G. Eyink, and V. Rossetto. Spatiotemporal correlations in three-dimensional homogeneous and isotropic turbulence. Phys. Fluids, 33:045114, 2021.
[2] W. D. McComb. The Physics of Fluid Turbulence. Oxford University Press, 1990.

[3] 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 question of notation.

The question of notation.

In recent years, when I specify the velocity field for turbulence, I invariably add a word of explanation about my use of Greek letters for Cartesian tensor indices. I point out that these Greek indices should not be confused with those used in four-space for four-dimensional tensors, as encountered in Einstein's Relativity. I think that I began do this round about the time I retired in 2006 and at the same time began looking at problems in phenomenology. Previously I had just followed Sam Edwards, who had been my PhD supervisor, because it seemed such a very good idea. By reserving Greek letters for indices, one could use letters like k, j, l, p $\dots$ for wavenumbers, which reduced the number of primed or multiply-primed variables needed in perturbation theory.

Presumably it had occurred to me that a different audience might not be familiar with this convention, or perhaps some referee rejected a paper because he didn't know what Greek letters were [1]? In any case, it was only recently that it occurred to me that Kolmogorov actually uses this convention too. In fact in the paper that I refer to as Kolmogorov 41A [2], one finds the first sentence: 'We shall denote by \[u_{\alpha} (P) = u_{\alpha} (x_1,x_2,x_3), \quad \alpha=1,\,2,\,3,\] the components of the velocity at the moment t at the point with rectangular Cartesian coordinates

x_1,x_2,x_3. So in future, I could say ‘as used by Kolmogorov’.

Kolmogorov also introduced the second-order and third-order longitudinal structure functions as $B_{dd}(r,t)$ and $B_{ddd}(r,t)$ (the latter appearing in K41B [3]), and others followed similar schemes, with the number of subscript ds increasing with order. This was potentially clumsy, and when experimentalists became able to measure high-order moments in the 1970s, they resorted to the notation $S_n(r,t)$. That is, S for ‘structure function’ and integer n for order, which is nicely compact.

During the sixties, statistical turbulence theories used a variety of notations. Unfortunately, for some people a quest for an original approach to a well known problem can begin with a new notation. On one occasion, I remember thinking that I didn’t even know how to pronounce the strange symbol that one optimistic theorist had used for the vertex operator of the Navier-Stokes equation. That was back in the early 1970s and it is still somewhere in my office filing cabinets. I don’t think I missed anything significant by not reading it!

Notational changes should be undertaken with caution. During the late 1990s I was just about the only person working on statistical closure theory (at least, in Eulerian coordinates) and I decided to adopt an emerging convention in dynamical systems theory. That is, I decided to represent all correlations by C and response tensors by R.

The only other change I made was to change the symbol for the transverse projection operator to Kraichnan’s use of P, from Edwards’s use of D. The result is, in my view, a notationally more elegant formalism; and perhaps if people again start taking an interest in renormalized perturbation theories and renormalization group, this would get them off to a good start.

However, there can be more to a formalism than just the notation. The true distinction between the two really lies in the *formulation*. Starting with the basic vector triad $\mathbf{k},\mathbf{j},\mathbf{l}$, Edwards used the triangle condition to eliminate the third vector as $\mathbf{l}=\mathbf{k}-\mathbf{j}$. This was done by others, but in the context of the statistical theories virtually everyone followed Kraichnan's much more complicated approach, in which he retained the three scalar magnitudes and imposed on all sums/integrals the constraint that they should always add up to a triangle. The resulting formulation is more opaque, more difficult to compute and does not permit symmetries to be deduced by simple inspection. Yet for some reason virtually everyone follows it, particularly obviously in the use of EDQNM as a model for applications. A concise account of the two different formalisms can be found in Section 3.5 of the book [4].

[1] Just joking! I've never had a paper rejected for that reason, but some rejections over the years have not been a great deal more sensible.

[2] A. N. Kolmogorov. The local structure of turbulence in incompressible viscous fluid for very large Reynolds numbers. C. R. Acad. Sci. URSS, 30:301, 1941.
[3] A. N. Kolmogorov. Dissipation of energy in locally isotropic turbulence. C. R. Acad. Sci. URSS, 32:16, 1941.
[4] W. David McComb. Homogeneous, Isotropic Turbulence: Phenomenology, Renormalization and Statistical Closures. Oxford University Press, 2014.

The last post of the weekly Blogs … but intermittently hereafter!

The last post of the weekly Blogs … but intermittently hereafter!
I posted the first of these blogs on 6 February 2020, just as the pandemic was getting under way. Since then (and slightly to my surprise) I have managed to post a blog every week. In the case of holidays, I wrote an appropriate number of blogs in advance and scheduled them to post at the appropriate times.

I wouldn't like to say that I couldn't have managed this without the pandemic, but it has certainly cut down on my other ways of spending time. And, for that matter energy, because in normal times I play badminton twice a week. At the same time, I should acquit myself of taking advantage of the pandemic. A year or so earlier, I had set up a WordPress site on servers installed on my own computer, and had constructed a somewhat satirical blog dealing with the mythical activities at a fictitious university. This gave me practice, and when the University of Edinburgh advertised a blogging support service for academic staff, I was ready to go. As I hesitated on the brink, a very helpful young person in Information Services gave me the requisite push and my blogging career was fairly launched.

Now that I have completed two years, I am finding the regular weekly deadline rather onerous. To be blunt, it is getting in the way of jobs that need a longer-term approach. At the same time, there are still many things that I wish to blog about. So from now on, I intend to blog only when I can do so without losing momentum at some other task.
What I hope that you will do, is fill in the little form that

you will find beside any blog, with your email address. This will ensure that in future you will receive a notification of each blog as it is posted. I realise that people can be hesitant about putting their name on a list. I can myself, one worry being that it may be difficult to get off again. In this case nothing could be simpler. When you receive your notification email, you just have to click on a link and you will be automatically removed. I have checked this and can assure you that it works.

To end on a positive note, I intend to produce a book which will literally just consist of the individual posts organised into chapters corresponding to the months. I think there is sufficient material contained in them to make an index helpful and, in the case of the ebook version, it will also be searchable. I am very conscious of the need for these things, as at the moment I have to rely on my memory to be sure that I'm not repeating myself!

From 'wavenumber murder' to wavenumber muddle?

From 'wavenumber murder' to wavenumber muddle?
In my post of 20 February 2020, I told of the referee who described my use of Fourier transformation as '*the usual wavenumber murder*'. I speculated that the situation had improved over the years due to the use of pseudo-spectral methods in direct numerical simulation, although I was able to quote a more recent example where a referee rejected a paper because he wasn't comfortable with the idea that structure

functions could be evaluated from the corresponding spectra.

However, while it is good to see a growing use of spectral methods, at the same time there are differences between the x-space and k-space pictures, and this can be confusing. Essentially, the phenomenology of fluid dynamicists has been based on the energy conservation equation in real-space, mostly using structure functions; whereas theorists have worked with the energy balance in wavenumber space as a closure problem for renormalization methods. This separation of activities has gone on over many decades.

For the purpose of this post, I want to look again at the Kolmogorov-Obhukov (1941) theory in x-space and k-space. Kolmogorov worked in real space and it is convenient to denote his two different derivations of inertial range forms as K41A [1] and K41B [2]. We will concentrate on the second of these, where he derived the well-known `4/5′ law for $S_3(r)$, from the KHE equation. We have quoted this previously and it may be obtained from the book [3] as: \begin{equation}\varepsilon =-\frac{3}{4}\frac{\partial S_2}{\partial t}+\frac{1}{4r^4}\frac{\partial (r^4 S_3)}{\partial r} +\frac{3\nu}{2r^4}\frac{\partial}{\partial r}\left(r^4\frac{\partial S_2}{\partial r}\right),\end{equation} and all the symbols have their usual meanings.

In order to solve this equation for S_3, Kolmogorov neglected both the time-derivative of S_2 and the viscous term, and thus obtained a *de facto* closure. In the case of stationary turbulence the first step is exact but for decaying turbulence it is an approximation for the inertial range which Kolmogorov called *local stationarity*. Later Batchelor referred to this as *equilibrium* [4], which is rather unfortunate as turbulence is the archetypal non-equilibrium problem. In fact Batchelor was carrying on Taylor’s idea that the Fourier modes acted as mechanical degrees of freedom and so could be treated by the methods of statistical mechanics. As the classical canon of solved problems in statistical mechanics is limited to thermal equilibrium (normally referred to simply as equilibrium), Batchelor was arguing that Taylor’s approach would be valid for the inertial range. In fact it isn’t because the modes are strongly coupled and this too is not canonical.

In any case, the neglect of the time-derivative of S_2 is a key step and its justification in time-dependent flows poses a problem. More recently, McComb and Fairhurst [5] showed that the neglect of this term cannot be an exact step and also cannot be justified by appeal to large Reynolds numbers or restriction to any particular range of values of r. In other words, it is a constant term and its neglect must be justified by either measurement or numerical simulation.

The situation is really quite different in wavenumber space. Here we have the Lin equation which is the Fourier transform of the KHE and takes its simplest form as: \begin{equation}\left(\frac{\partial}{\partial t} + 2\nu k^2\right)E(k,t) = T(k,t).\end{equation}where\begin{equation}T(k,t)= \int_0^\infty dj\, S(k,j:t), \end{equation}and $S(k,j;t)$ can be expressed in terms of the third-order moment $C_{\alpha\beta\gamma}(\mathbf{j},\mathbf{k-j},\mathbf{-k};t)$.

One immediate difference is that the KHE is purely local in the variable r, whereas the Lin equation is non-local in wavenumber. In fact all Fourier modes are coupled together. We can define the inter-mode energy flux as: \begin{equation}\Pi (\kappa,t) = \int_\kappa^\infty dk\,T(k,t) = -\int_0^\kappa dk \, T(k,t).\end{equation}The criterion for an inertial range of wavenumbers is that the condition $\Pi = \varepsilon$ should hold and this is nowadays referred to as scale invariance. It does not apply in any way to the situation in real space and it has no connection with the concept of *local stationarity* which was renamed *equilibrium* by Batchelor.

Lastly, the interpretation of the time-derivative term in wavenumber space is quite different from that in real space. We may see this by rearranging the Lin equation as: \begin{equation}-T(k,t) = I(k,t) – 2\nu k^2 E(k,t), \quad \mbox{where} \quad I(k,t) = -\frac{\partial E(k,t)}{\partial t}.\label{diff}\end{equation}Evidently for free decay the input term $I(k)$ is positive, and this is actually how Uberoi [6] made the first measurements of the transfer spectrum in grid turbulence. He measured the input term and the viscous term and used equation (\ref{diff}) to evaluate $T(k,t)$.

McComb and Fairhurst [5] pointed out that the constant value of the time derivative term in the limit of infinite Reynolds numbers in r-space Fourier transforms to a delta function at the origin in k-space. In other words this amounts to a derivation of the form postulated by Edwards [7] (following Batchelor [4]) that the transfer spectrum is given in terms of the Dirac delta function δ by:\begin{equation}-T(k,t) = \varepsilon \delta(k,t) -\varepsilon \delta(k – \infty,t),\end{equation} in the limit of infinite Reynolds numbers, although the Edwards form was for the stationary case.

This of course is a very extreme situation. The key point to note is that, while the time-derivative of S_2 poses a problem for local stationarity in r-space, the time-derivative of $E(k,t)$ poses no problem for scale invariance in k-space. This is why the $-5/3$ spectrum is so widely observed.

[1] A. N. Kolmogorov. The local structure of turbulence in incompressible viscous fluid for very large Reynolds numbers. C. R. Acad. Sci. URSS, 30:301, 1941.
[2] A. N. Kolmogorov. Dissipation of energy in locally isotropic turbulence. C. R. Acad. Sci. URSS, 32:16, 1941.
[3] W. David McComb. Homogeneous, Isotropic Turbulence: Phenomenology, Renormalization and Statistical Closures. Oxford University Press, 2014.

[4] G. K. Batchelor. The theory of homogeneous turbulence. Cambridge University Press, Cambridge, 1st edition, 1953.
[5] W. D. McComb and R. B. Fairhurst. The dimensionless dissipation rate and the Kolmogorov (1941) hypothesis of local stationarity in freely decaying isotropic turbulence. J. Math. Phys., 59:073103, 2018.
[6] M. S. Uberoi. Energy transfer in isotropic turbulence. Phys. Fluids, 6:1048, 1963.
[7] S. F. Edwards. Turbulence in hydrodynamics and plasma physics. In Proc. Int. Conf. on Plasma Physics, Trieste, page 595. IAEA, 1965.

Is the concept of an energy cascade a help or a hindrance?

Is the concept of an energy cascade a help or a hindrance?

In his 1947 exegesis of Kolmogorov's theory, Batchelor [1] explained the underlying idea of a transfer of energy from large eddies to progressively smaller eddies, until the (local) Reynolds number becomes too small for new eddies to form. He pointed out that the situation had been summarized in a rhyme which he believed was due to L. F. Richardson (no reference given) and which is very well known as:

Big whirls have little whirls, Which feed on their velocity,

And little whirls have lesser whirls, And so on to viscosity!

Incidentally he misquoted 'whirls' as 'whorls', and since then most people seem to have followed suit.

In his discussion Batchelor sometimes followed Kolmogorov, and referred to ‘pulsations’ while at other times he used the more usual ‘eddies’. This variation seems to actually underline the lack of precision of the concept; although, despite this, it is intuitively appealing.

The term ‘cascade’, with its connotations of a stepwise process, and indeed of localness, is also appealing. According to Eyink and Sreenivasan [2] it was first used by Onsager [3]; but it is his earlier use of the concept of energy transfer in wavenumber space [4] that is truly significant. Obukhov had already obtained the inertial-range spectrum corresponding to Kolmogorov’s result for the second-order structure function, but this involved the introduction of an ad hoc eddy viscosity [5]. In [4], Onsager essentially pointed out that the energy flux through modes must be constant in the inertial range. This is the property that is often referred to as *scale invariance.*

The physics of turbulent energy transfer and dissipation can readily be deduced from the equation of motion in wavenumber space; but it is interesting to put oneself in the position of Richardson, looking at (one imagines) snapshots of flow visualizations and creating his mental picture of a cascade of eddies. The equation of motion in real space would have given some limited help perhaps. Evidently the nonlinear term had to be responsible for the creation of new, smaller eddies; and it was known that this term conserved energy. Also, one could deduce that the viscous term would be more significant at the smallest scales. Nevertheless, it was a remarkable achievement to summarise the essence of turbulence in this very persuasive way. So what are its disadvantages?

The first disadvantage, in my view, is that it focuses attention on a single snapshot of the turbulence. Or, in statistical terms, on a single realization. This leads to people drawing conclusions that require a single realization (e.g. the importance of internal intermittency). However, we must always bear in mind that we need average quantities, and to get to them we actually have to take an average. So, if we average our single snapshot of a flow visualization by taking many such snapshots and constructing a form of ensemble average, the result is a blur! For instance, the recent paper by Yoffe and McComb [6] shows how internal intermittency disappears under ensemble averaging.

Paradoxically, my choice for second disadvantage is that I have concluded that the term ‘cascade’ is unhelpful when applied to the inter-mode energy flux. And this, I may say, is despite the fact that I have spent a working lifetime doing just that! In principle, every wavenumber is coupled to every other wavenumber by the nonlinear term. So we can see the attraction of having some sort of cascade or idea of localness. Indeed, in the 1980s/90s there was quite a lot of attention given, using numerical simulations, to the relative importance of different triads of wavenumbers for energy transfer. Now I am not disparaging that work in any way, but it is very complicated and should not distract us from the essential fact: the flux of energy through a wavenumber κ, from *all other wavenumbers less than* κ, is constant for all values of κ in the inertial range. This fact is all the ‘localness’ that we need for the Obukhov-Onsager energy spectrum.

Lastly, it should be understood that the cascade in real space is spread out in space and time. That is, if we distinguish between scale and position by introducing relative and centroid coordinates, thus: \[r= (x-x’) \quad \mbox{and}\quad R=(x+x’)/2,\] then in order to observe a cascade through scale r we have to change the position of observation R with time. In contrast, the flux through a mode with wavenumber κ takes place at a single value of R. It is for this reason that the flux in wavenumber space cannot be applied to the cascade in real space.

Still, the term ‘cascade’, in the context of wavenumber space, is so embedded in the general consciousness (including my own!) that there is little possibility of making a change.

[1] G. K. Batchelor. Kolmogorov’s theory of locally isotropic turbulence. Proc. Camb. Philos. Soc., 43:533, 1947.
[2] G. L. Eyink and K. R. Sreenivasan. Onsager and the Theory of Hydrodynamic Turbulence. Rev. Mod. Phys., 87:78, 2006.
[3] L. Onsager. Statistical Hydrodynamics. Nuovo Cim. Suppl., 6:279, 1949.
[4] L. Onsager. The Distribution of Energy in Turbulence. Phys. Rev., 68:281, 1945. (Abstract only).
[5] A. M. Obukhov. On the distribution of energy in the spectrum of turbulent flow. C.R. Acad. Sci. U.R.S.S, 32:19, 1941.
[6] S. R. Yoffe and W. D. McComb. Does intermittency affect the inertial transfer rate in stationary isotropic turbulence? arXiv:2107.09112v1[physics.flu-dyn], 2021.

Chaos and Complexity.

Chaos and Complexity.
In the previous blog we discussed the growth of interest in deterministic chaos in low-dimensional dynamical systems, and the way in which it impinged on turbulence theory. Altogether, it seemed like a paradigm shift; in that we learned that only quantum effects were truly random, and that all classical effects were deterministic. If one knew the initial conditions of a classical dynamical system then one could, in principle, predict its entire evolution in time. If! Anyway, in those days we began to refer to the turbulent velocity field as

being chaotic rather than random; but I suspect that the majority are now back to random.

However, such ideas also arose in the late 19th Century, as part of the invention of Statistical Mechanics, with Boltzmann’s assumption of *molecular chaos*. This was to the effect that molecular motion was uncorrelated immediately before and after a two-body collision. It was made in the context of a gas modelled as N particles in a box (where N is of the order of Avogadro’s number), and the motion of the particles is governed by Hamilton’s equations. The system can be specified by the N-particle distribution (or density) which is the solution of Liouville’s equation. Although an exact formulation, this theory is contrary to experience because the entropy is found to be constant in time, which contradicts the second law of thermodynamics. This result is a well-known paradox and it was resolved by Boltzmann in his famous H-theorem.

Boltzmann wrote the entropy S in terms of a measure H of the information about the system, thus:\[S=-k_B H \quad \mbox{where} \quad H=\int \, du \, f(u,t) \,ln\,f(u,t),\] u is the molecular speed and $f(u,t)$ is the single-particle distribution of molecular speeds. In obtaining the equation for f, Boltzmann had to overcome a closure problem (much as in turbulence!) and his principle of molecular chaos justified the factorization of the two-particle function into the product of two single-particle functions. So Boltzmann’s H-theorem is that H decreases with time, meaning that the entropy increases.

Although this is not so well known, the paradox was also resolved by Gibbs, albeit in a more fundamental way. He showed that if a small amount of the information was lost from H, then it was no longer invariant and would increase with time. In his case, this was achieved by coarse-graining the exact Liouville distribution function so that it was no longer exact, but it could equally well be achieved in practice by a slight deficiency in our specification of the initial conditions (position and momentum) of each of the N particles. In fact, to put it bluntly, it would be difficult (or perhaps impossible) to specify the initial state of a system of order 10^{20} degrees of freedom with sufficient accuracy to avoid the system entropy increasing with time.

The point to be taken from all this is that Hamilton’s equations, although themselves reversible in time, can nevertheless describe a real system which has properties that are not reversible in time. The answer lies in the complexity of the system.
This applies just as much to the quantum form of Hamilton’s equations. Recently there has been an international discussion (by virtual means) of the Einstein-Podolsky-Rosen paradox, which asserts that quantum mechanics is not a complete theory. I read some of the contributions to this, but was not impressed. In particular the suggestion was put forward that the time-reversal symmetry of the basic quantum equations of motion, ruled out their ability to describe a real world which undergoes irreversible changes; something that is generally referred to as ‘time’s arrow’. But of course the same applies to the classical form of the equations, and one must bear in mind that one has to take not only large-N limits but also continuum limits to describe the real world.

There are lessons here, at least in principle, for turbulence theorists too, and I have given specific instances such as the irrelevance of intermittency or the incorrectness of the Onsager conjecture (when judged by the physics). No doubt I will give more in the months to come. Background material for this blog can be found in the lecture notes [1], which can be downloaded free of charge.

[1] W. David McComb. Study Notes for Statistical Physics: A concise, unified overview of the subject. Bookboon, 2014. (Free download of pdf from Bookboon.com)

Fashions in turbulence theory.

Fashions in turbulence theory.

Back in the 1980s, fractals were all the rage. They were going to solve everything, and turbulence was no exception. The only thing that I can remember from their use in microscopic physics was that the idea was applied to the problem of diffusion-limited aggregation, and I've no memory of how successful they were (or were not). In turbulence they were a hot topic for solving the supposed problem of intermittency, and there was a rash of papers decorated with esoteric mathematical terms. This could be regarded as '*Merely corroborative detail, intended to give artistic verisimilitude to an otherwise bald and unconvincing narrative*.'[1]. When these proved inadequate, the next step was *multi*fractals, which rather underlined the fact that this approach was at best a phenomenology, rather than a fundamental theory. And that activity too seems to have died away.

Another fashion of the 1970s/80s was the idea of deterministic chaos. This began around 1963 with the Lorentz system, a set of simple differential equations intended to model atmospheric convection. These equations were readily computed, and it was established that their solutions were sensitive to small changes in initial conditions. With the growing availability of desktop computers in the following decades, low-dimensional dynamical systems of this kind provided a popular playground for mathematicians and we all began to hear about Lorentz attractors, strange attractors, and the butterfly effect. Just to make contact with the previous fashion, the phase space portraits of these systems often were found to have a fractal structure!

In 1990, a reviewer of my first book [2] rebuked me for saying so little about chaos and asserted that it would be a dominant feature of turbulence theory in the future. Well, thirty years on and we are still waiting for it. The problem with this prediction is that turbulence, in contrast to the low-dimensional models studied by the chaos enthusiasts, involves large numbers of degrees of freedom; and these are all coupled together. As a consequence, the average behaviour of a turbulent fluid is really quite insensitive to fine details of its initial conditions. In reality that butterfly can flap its wings as much as it likes, but it isn't going to cause a storm.

In fairness, although we have gone back to using our older language of 'random' rather than 'chaotic' when studying turbulence, the fact remains that deterministic chaos is actually a very useful concept. This is particularly so when taken in the context of complexity, and that will be the subject of our next post.

[1] W. S. Gilbert, The Mikado, Act 2, 1852.
[2] W. D. McComb. The Physics of Fluid Turbulence. Oxford University Press, 1990.

Summary of the Kolmogorov-Obukhov (1941) theory: overview.

Summary of the Kolmogorov-Obukhov (1941) theory: overview.

In the last three posts we have summarised various aspects of the Kolmogorov-Obukhov (1941) theory. When considering this theory, the following things need to be borne in mind.

[a] Whether we are working in x-space or k-space matters. See my posts of 8 April and 15 April 2021 for a concise general discussion.
[b] In x-space the equation of motion (NSE) simply presents us with the problem of an insoluble, nonlinear partial differential equation.
[c] In k-space the NSE presents a problem in statistical physics and in itself tells us much about the transfer and dissipation of turbulent kinetic energy.
[d] The Karman-Howarth equation is a local energy balance that holds for any particular value of the distance r between two measuring points.
[e] There is no energy flux between different values of r; or, alternatively, through scale.
[f] The energy flux $\Pi(k)$ is derived from the Lin equation (i.e. in wavenumber space) and cannot be applied in x-space.
[g] The maximum value of the energy flux, $\Pi_{max}=\varepsilon_T$ (say), is a number, not a function, and can be used (like the dissipation ε) in both k-space and x-space.
[h] It also matters whether the isotropic turbulence we are considering is stationary or decaying in time.
[g] If the turbulence is decaying in time, then K41B relies on Kolmogorov's hypothesis of *local stationarity*. It has been pointed out in a previous post (Part 2 of the present series) that this cannot be the case by virtue of restriction to a range of scales nor in the limit of infinite Reynolds number [1]. See also the supplemental material for [2].
[h] In k-space this is not a problem and the $k^{-5/3}$ spectrum can still be expected [1], as of course is found in practice.
[i] If the turbulence is stationary, then K41B is exact for a range of wavenumbers for sufficiently large Reynolds numbers.

The extent of this inertial range increases with increasing Reynolds numbers.

I have not said anything in this series about the concept of intermittency corrections or anomalous exponents. This topic has been dealt with in various blogs and soon will be again.

[1] W. D. McComb and R. B. Fairhurst. The dimensionless dissipation rate and the Kolmogorov (1941) hypothesis of local stationarity in freely decaying isotropic turbulence. J. Math. Phys., 59:073103, 2018.
[2] W. David McComb, Arjun Berera, Matthew Salewski, and Sam R. Yoffe. Taylor's (1935) dissipation surrogate reinterpreted. Phys. Fluids, 22:61704, 2010.

Summary of the Kolmogorov-Obukhov (1941) theory. Part 3: Obukhov's theory in k-space.

Summary of the Kolmogorov-Obukhov (1941) theory. Part 3: Obukhov's theory in k-space.

Obukhov is regarded as having begun the treatment of the problem in wavenumber space. In [1] he referred to an earlier paper by Kolmogorov for the spectral decomposition of the velocity field in one dimension and pointed out that the three-dimensional case is carried out similarly by multiple Fourier integrals. He employed the Fourier-Stieltjes integral but fortunately this usage did not survive. For many decades the standard Fourier transform has been employed in this field.

[a] Obukhov's paper [1] was published between K41A and K41B, and was described by Batchelor 'as to some extent anticipating the work of Kolmogorov'. He worked with the energy balance in k-space and, influenced by Prandtl's work, introduced an *ad hoc* closure based on an effective viscosity.

[b] The derivation of the '-5/3' law for the energy spectrum seems to have been due to Onsager [2]. He argued that Kolmogorov's similarity principles in x-space would imply an invariant flux (equal to the dissipation) through those wavenumbers where the viscosity could be neglected. Dimensional analysis then led to $E(k) \sim \varepsilon^{2/3}k^{-5/3}$.

[c] As mentioned in the previous post (points [c] and [d]), Batchelor discussed both K41A and K41B in his paper [3], but did not include K41B in his book [4]. Also, in his book [4], he discussed K41A entirely in wavenumber space. The reasons for this change to a somewhat revisionist approach can only be guessed at, but there may be a clue in his book. On page 29, first paragraph, he says: 'Fourier analysis of the velocity field provides us with an extremely valuable analytical tool *and one that is well-nigh indispensable for the interpretation of equilibrium or similarity hypotheses*.' (The emphasis is mine.)

[d] This is a very strong statement, and of course the reference is to Kolmogorov's theory. There is also the fact that K41B is not easily translated into k-space. Others followed suit, and Hinze [5] actually gave the impression of quoting from K41A but used the word 'wavenumber', which does not in fact occur in that work. By the time I began work as a postgraduate student in 1966, the use of spectral methods had become universal in both experiment and theory.

[e] There does not appear to be any k-space *ad hoc* closure

of the Lin equation to parallel K41B (i.e. the derivation of the ‘4/5’ law); but, for the specific case of stationary turbulence, I have put forward a treatment which uses the infinite Reynolds number limit to eliminate the energy spectrum, while retaining its effect through the dissipation rate [6]. It is based on the scale invariance of the inertial flux, thus: \begin{equation}\Pi(\kappa)=-\int_0^{\kappa}dk\,T(k) = \varepsilon, \end{equation}which of course can be written in terms of the triple-moment of the velocity field. As the velocity field in k-space is complex, we can write it in terms of amplitude and phase. Accordingly, \begin{equation}u_{\alpha}(\mathbf{k}) = V(\kappa)\psi_{\alpha}(k’),\end{equation} where $V(\kappa)$ is the root-mean-square velocity, $k’=k/\kappa$ and ψ represents phase effects. The result is: \begin{equation}V(\kappa)=B^{-1/3}\varepsilon^{1/3}\kappa^{-10/3},\end{equation}where B is a constant determined by an integral over the triple-moment of the phases of the system. The Kolmogorov spectral constant is then found to be: $4\pi\, B^{-2/3}$.

[f] Of course a statistical closure, such as the LET theory, is needed to evaluate the expression for B. Nevertheless, it is of interest to note that this theory provides an answer to Kraichnan’s interpretation of Landau’s criticism of K41A [7]. Namely, that the dependence of an average (i.e. the spectrum) on the two-thirds power of an average (i.e. the term involving the dissipation) destroys the linearity of the averaging process. In fact, the minus two-thirds power of the average in the form of $B^{-2/3}$ cancels the dependence associated with the dissipation.

[1] A. M. Obukhov. On the distribution of energy in the spectrum of turbulent flow. C.R. Acad. Sci. U.R.S.S, 32:19, 1941.
[2] L. Onsager. The Distribution of Energy in Turbulence. Phys. Rev., 68:281, 1945. (Abstract only.)

[3] G. K. Batchelor. Kolmogoroff’s theory of locally isotropic turbulence. Proc. Camb. Philos. Soc., 43:533, 1947.
[4] G. K. Batchelor. The theory of homogeneous turbulence. Cambridge University Press, Cambridge, 1st edition, 1953.
[5] J. O. Hinze. Turbulence. McGraw-Hill, New York, 2nd edition, 1975. (First edition in 1959.)
[6] David McComb. Scale-invariance and the inertial-range spectrum in three-dimensional stationary, isotropic turbulence. J. Phys. A: Math. Theor., 42:125501, 2009.
[7] R. H. Kraichnan. On Kolmogorov’s inertial-range theories. J. Fluid Mech., 62:305, 1974.

Summary of the Kolmogorov-Obukhov (1941) theory. Part 2: Kolmogorov’s theory in x-space.

Summary of the Kolmogorov-Obukhov (1941) theory. Part 2: Kolmogorov’s theory in x-space.
Kolmogorov worked in x-space and his two relevant papers are cited below as [1] (often referred to as K41A) and [2] (K41B). We may make a pointwise summary of this work, along with more recent developments as follows.

[a] In K41A, Kolmogorov introduced the concepts of *local homogeneity* and *local isotropy*, as applying in a restricted range of scales and in a restricted volume of space. He also seems to have introduced what we now call the *structure functions*, allowing the introduction of *scale* through the correlations of velocity differences taken between two points

separated by a distance r. He used Richardson's concept of a cascade of eddies, in an intuitive way, to introduce the idea of an *inertial sub-range,* and then used dimensional analysis to deduce that (in modern notation) $S_2 \sim \varepsilon^{2/3}r^{2/3}$.

[b] In K41B, he used an *ad hoc* closure of the Karman-Howarth equation (KHE) to argue that $S_3 = 0.8 \varepsilon r$ in the inertial range of values of r: the well-known 'four-fifths law'. He further assumed that the skewness factor was constant and found that this led to the K41A result for S_2. The closure was based on the fact that the term explicit in S_2 would vanish as the viscosity tended to zero, whereas its effect could still be retained in the dissipation rate.

[c] In 1947, Batchelor [3] provided an exegesis of both these theories. In the case of K41A, this was only partial, but he did make it clear that K41A relied (at least implicitly) on Richardson's idea of the 'eddy cascade'. He also pointed out that K41B could not be readily extended to higher-order equations in the statistical hierarchy, because of the presence of the pressure term with its long-range properties in the higher-order equations.

[d] Moffatt [4] credited this paper by Batchelor with bringing Kolmogorov's work to the Western world. He also, in effect, expressed surprise that Batchelor did not include his re-derivation of K41B in his book. This is a very interesting point; and, in my view, is not unconnected to the fact that Batchelor discussed K41A almost entirely in wavenumber space in his book. I will return to this later.

[e] In 2002, Lundgren re-derived the K41B result, by expanding the dimensionless structure functions in powers of the inverse Reynolds number. By demanding that the expansions matched asymptotically in an overlap region between outer and inner scaling regimes, he was also able to recover the K41A result without the need to make an additional assumption about the

constancy of the skewness.

[f] More recently, McComb and Fairhurst [6] used the asymptotic expansion of the dimensionless structure functions to test Kolmogorov’s hypothesis of local stationarity and concluded that it could not be true. They found that the time-derivative must give rise to a constant term; which, however small, violates the K41B derivation of the four-fifths law. Nevertheless, they noted that in wavenumber space, this term (which plays the part of an input to the KHE) will appear as a Dirac delta function at the origin, and hence does not violate the derivation of the minus five-thirds law in k-space. We will extend this idea further in the next post.

[1] A. N. Kolmogorov. The local structure of turbulence in incompressible viscous fluid for very large Reynolds numbers. C. R. Acad. Sci. URSS, 30:301, 1941. (K41A)
[2] A. N. Kolmogorov. Dissipation of energy in locally isotropic turbulence. C. R. Acad. Sci. URSS, 32:16, 1941. (K41B)
[3] G. K. Batchelor. Kolmogoroff’s theory of locally isotropic turbulence. Proc. Camb. Philos. Soc., 43:533, 1947.
[4] H. K. Moffatt. G. K. Batchelor and the Homogenization of Turbulence. Annu. Rev. Fluid Mech., 34:19-35, 2002.
[5] Thomas S. Lundgren. Kolmogorov two-thirds law by matched asymptotic expansion. Phys. Fluids, 14:638, 2002.
[6] W. D. McComb and R. B. Fairhurst. The dimensionless dissipation rate and the Kolmogorov (1941) hypothesis of local stationarity in freely decaying isotropic turbulence. J. Math. Phys., 59:073103, 2018.

Summary of Kolmogorov-Obukhov (1941) theory. Part 1: some preliminaries in x-space and k-space.

Summary of Kolmogorov-Obukhov (1941) theory. Part 1: some preliminaries in x-space and k-space.

Discussions of the Kolmogorov-Obukhov theory often touch on the question: can the two-thirds *law;* or, alternatively, the *minus five-thirds law,* be derived from the equations of motion (NSE)? And the answer is almost always: ‘no, they can’t’! Yet virtually every aspect of this theory is based on what can be readily deduced from the NSE, and indeed has so been deduced, many years ago. So our preliminary here to the actual summary, is to consider what we know from a consideration of the NSE, in both x-space and k-space. As another preliminary, all the notation is standard and can be found in the two books cited below as references.

We begin with the familiar NSE, consisting of the equation of motion, \begin{equation}\frac{\partial u_{\alpha}}{\partial t} + \frac{\partial (u_{\alpha}u_\beta)}{\partial x_\beta} =-\frac{1}{\rho}\frac{\partial p}{\partial x_\alpha} + \nu \nabla^2 u_\alpha,\end{equation}which expresses conservation of momentum and is local, in that it gives the relationship between the various terms at one point in space; and the incompressibility condition \begin{equation}\frac{\partial u_\beta}{\partial x_\beta} = 0.\end{equation} It is well known that taking these two equations together allows us to eliminate the pressure by solving a Poisson-type equation. The result is an expression for the pressure which is an integral over the entire velocity field: see equations (2.3) and (2.9) in [1].

In k-space we may write the Fourier-transformed version of (1) as: \begin{equation}\frac{\partial u_\alpha(\mathbf{k},t)}{\partial t} + i k_\beta\int d^3 j u_\alpha(\mathbf{k-j}.t)u_\beta(\mathbf{j},t) = k_\alpha p(\mathbf{k},t) -\nu k^2 u_\alpha (\mathbf{k},t).\end{equation} The derivation can be found in Section 2.4 of [2]. Also, the discrete Fourier-series version (i.e. in finite box) is equation (2.37) in [2].

The crucial point here is that the modes $\mathbf{u}(\mathbf{k},t)$ form a complete set of degrees of freedom and that each mode is coupled to every other mode by the non-linear term. So this is not just a problem in statistical physics, it is an example of the many-body problem.

Note that (1) gives no hint of the cascade, but (3) does. All modes are coupled together and, if there were no viscosity present, this would lead to equipartition, as the conservative non-linear term merely shares out energy among the modes. The viscous term is symmetry-breaking due to the factor k^2 which increases the dissipation as the wavenumber increases. This prevents equipartition and leads to a cascade from low to high wavenumbers. All of this becomes even clearer when we multiply the equation of motion by the velocity and average. We then obtain the energy-balance equations in both x-space and k-space.

We begin in real space with the Karman-Howarth equation (KHE). This can be written in various forms (see Section 3.10.1 in [2]), and here we write in terms of the structure functions for the case of free decay: \begin{equation}\varepsilon =-\frac{3}{4}\frac{\partial S_2}{\partial t}+\frac{1}{4r^4}\frac{\partial (r^4 S_3)}{\partial r} +\frac{3\nu}{2r^4}\frac{\partial}{\partial r}\left(r^4\frac{\partial S_2}{\partial r}\right).\end{equation}Note that the pressure does not appear, as a correlation of the form $\langle up \rangle$

cannot contribute to an isotropic field, and that strictly the left hand side should be the decay rate ε_D but it is usual to replace this by the dissipation as the two are equal in free decay. Full details of the derivation can be found in Section 3.10 of [2].

For our present purposes, we should emphasise two points. First, this is one equation for two dependent variables and so requires a statistical closure in order to solve for one of the two. In other words, it is an instance of the notorious statistical closure problem. Second, it is local in the variable r and does not couple different scales together. It holds for any value of r but is an energy balance locally at any chosen value of r.

The Lin equation is the Fourier transform of the KHE. It can be derived directly in k-space from the NSE (see Section 3.2.1 in [2]):\begin{equation} \left(\frac{\partial}{\partial t} + 2\nu k^2\right)E(k,t) = T(k,t).\end{equation} Here $T(k,t)$ is called the *transfer spectrum*, and can be written as: \begin{equation}T(k,t)= \int_0^\infty dj\, S(k,j:t),\end{equation}where $S(k,j;t)$ is the *transfer spectral density* and can be expressed in terms of the third-order moment $C_{\alpha\beta\gamma}(\mathbf{j},\mathbf{k-j},\mathbf{-k};t)$.

Unlike the KHE, which is purely local in its independent variable, the Lin equation is non-local in wavenumber. We can define its associated inter-mode energy flux as:\begin{equation}\Pi (\kappa,t) = \int_\kappa^\infty dk\,T(k,t) = -\int_0^\kappa dk \, T(k,t).\end{equation}

We have now laid a basis for a summary of the Kolmogorov-Obhukov theory and one point should have emerged clearly: the energy cascade is well defined in wavenumber space. It is not defined at all in the context of energy conservation in real space. It can only exist as an intuitive phenomenon which is extended in space and time.

[1] W. D. McComb. The Physics of Fluid Turbulence. Oxford University Press, 1990.
[2] W. David McComb. Homogeneous, Isotropic Turbulence: Phenomenology, Renormalization and Statistical Closures. Oxford University Press, 2014.