Compatibility of temporal spectra with Kolmogorov (1941): the Taylor hypothesis.

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Earlier this year I received an enquiry from Alex Liberzon, who was puzzled by the fact that some people plot temporal frequency spectra with a $-5/3$ power law, but he was unable to reconcile the dimensions. This immediately took me back to the 1970s when I was doing experimental work on drag-reduction, and we used to measure frequency spectra and convert them to one-dimensional wavenumber spectra using Taylor's hypothesis of frozen convection [1]. It turned out that Alex's question was more complicated than that and I will return to it at the end. But I thought my own treatment of this topic in [1] was terse, to say the least, and that a fuller treatment of it might be of general interest. It also has the advantage of clearing the easier stuff out of the way!

Consider a turbulent velocity field $u(x,t)$ which is stationary and homogeneous with rms value U. According to Kolmogorov (1941) [2], the mean square variation in the velocity field over a distance r from a point x is given by:\begin{equation}\langle \Delta u^2_r \rangle \sim (\varepsilon r)^{2/3}.\end{equation} If we now consider the turbulence to be convected by a uniform velocity U_c in the x-direction, then the K41 result for the mean square variation in the velocity field over an interval of time τ at a point x is given by: \begin{equation}\langle \Delta u^2_\tau \rangle \sim (\varepsilon U_c\tau)^{2/3}.\end{equation}The dimensional consistency of the two forms is obvious from inspection.

Next let us examine the dimensions of the temporal and spatial spectra. We will use the angular frequency $\omega = 2\pi f$, where $f $ is the frequency in Hertz, in order to be consistent with the definition of wavenumber k_1, where k_1 is the component of the wavevector in the direction of x. Integrating both forms of the spectrum, we have the condition: \begin{equation} \int^\infty_0 E(\omega) d\omega = \int_0^\infty E_{11}(k_1) dk_1 = U^2. \end{equation} Evidently the dimensions are given by: \begin{equation}\mbox{Dimensions of}\, E(\omega)d\omega = \mbox{Dimensions of}\, E_{11}(k_1) dk_1 = L^2 T^{-2};\end{equation} or velocity squared.

Then we introduce Taylor’s hypothesis in the form: \begin{equation} \frac{\partial}{\partial t} = U_c \frac{\partial}{\partial x}, \quad \mbox{thus} \quad \omega = U_c k_1;\end{equation} and hence: \begin{equation}k_1= \frac{\omega}{U_c} \quad \mbox{and} \quad dk_1 = \frac{d\omega}{U_c}. \end{equation}
The Kolmogorov wavenumber spectrum (in the one-dimensional form that is usually measured) is given by:\begin{equation}E_{11}(k_1) = \alpha_1 \varepsilon^{2/3} k^{-5/3}_1 dk_1.\end{equation}We should note that α_1 is the constant in the one-dimensional spectrum and is related to the three-dimensional form α by $\alpha_1 = (18/55)\alpha $. Substituting for the wavenumbers from (6) into (7) we find:\begin{equation} E_{11}(k_{1})dk_{1} = \alpha_1 (\varepsilon U_c)^{2/3}\omega^{5/3} d\omega \equiv E(\omega)d\omega, \end{equation} which is easily shown to have the correct dimensions of velocity squared.

After seeing this analysis, Alex came back with: but what about when the field is homogeneous and isotropic, with $U_c=0$? That’s a very good question and takes us into a topic which originated with Kraichnan’s analysis of the failure of DIA in (1964) [1]: the importance of sweeping effects on the decay of the velocity correlation. There are now numerous papers which address this topic and they continue to appear. So it does not give the impression of being settled. From my point of view, this is important in the context of closure approximations; but I understand that the answer to the question of $f^{-5/3}$ or f^{-2} depends on the importance or otherwise of sweeping effects.

I intend to return to this, but not necessarily next week!

[1] W. D. McComb. The Physics of Fluid Turbulence. Oxford University Press, 1990.
[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.

The concept of universality classes in critical phenomena.

The concept of universality classes in critical phenomena.
The universality of the small scales, which is predicted by the Richardson-Kolmogorov picture, is not always observed in practice; and in the previous post I conjectured that departures from this might be accounted for by differences in the spatial symmetry of the large scale flow. To take this idea a step further, I now wonder whether it would be worth exploring how the idea of universality classes could be applied to the turbulent cascade? First, I should explain what universality classes actually are.

In the study of critical phenomena, we are concerned with changes of phase or state which can occur at a critical temperature, which is invariably denoted by T_c. For instance, the transition from liquid to gas, or the transition from para- to ferromagnetism. In general, it is found that the thermodynamic variables (e.g. heat capacity, magnetic susceptibility) of a system either tend to zero, or tend to infinity, as the system approaches the critical temperature. If we represent any such macroscopic variable by $F(T)$ and introduce the reduced temperature Θ_c by $$\Theta_c = \frac{T-T_c}{T_c}.$$ Then, as $T\rightarrow T_c$ and $\Theta_c \rightarrow 0$, we have $$F(\Theta_c) = A \Theta^{-n},$$ where A is a constant and n is the critical exponent. Obviously the critical exponent will be negative when $F(0)=0$ and positive when $F(0)=\infty$.

Here the constant A and the critical temperature T_c depend on the details of the system at the molecular level and therefore vary from one system to another. These quantities must be determined experimentally. However, in practice it is found that sometimes different systems have the same values of critical exponents and this depends only on symmetry properties of the microscopic energy function (or Hamiltonian). When this is found to be the case, the two systems are said to be in the same *universality class*.

Accordingly, in my view it would be worth reviewing the different investigations in order to find out if one could organise results for the inertial-range exponent into some kind of universality classes, although allowance should be made for experimental error, which tends to be much greater in fluid dynamics than in microscopic physics. I would be tempted to take a look through my files, but unfortunately I remain cut off from my university office by the pandemic.

Further details about critical phenomena may be found in reference [1] below.

[1] W. D. McComb. Renormalization Methods: A Guide for Beginners. Oxford University Press, 2004.

Macroscopic symmetry and microscopic universality.

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The concepts of *macroscopic* and *microscopic* are often borrowed, in an unacknowledged way, from physics, in order to think about the fundamentals of turbulence. By that, I mean that there is usually no explicit acknowledgement, nor indeed apparent realization, that the ratio of large scales to small scales is many orders of magnitude smaller in turbulence (which is at all scales actually a macroscopic phenomenon) than it is in microscopic physics.

This idea began with Kolmogorov in 1941, when he employed Richardson's concept of a cascade of energy from large eddies to small; to argue that, after a sufficiently large number of steps, there could be a range of eddy sizes which were statistically independent of their large-scale progenitors. In passing, it should be noted that the concept of 'eddy' can be left rather intuitive, and we could talk equally vaguely about 'scales'. However, combining the cascade idea with Taylor's earlier introduction of Fourier modes as the degrees of freedom of a turbulent system, leads to a much more satisfactory analogy with statistical physics, with the onset of scale invariance strengthening the analogy to the microscopic theory of critical phenomena. As is well known, that leads to the `$5/3$' spectrum, which was expected to be universal.

My own view is that it would be good to get it settled that the Kolmogorov spectrum holds for isotropic turbulence. There is still an absence of consensus about that. But the broader claim of universality has been supported by measurements of spectra in a vast variety of flow configurations; although, inevitably there have been instances where it is not supported. So we end up with yet another unresolved issue in turbulence. Is small-scale turbulence universal or not?

In order to consider whether or not the concept of symmetry could assist with this, it may be helpful to think in terms of definite examples. First, let us consider laminar flow in the x_1 direction between fixed parallel plates situated at $x_2=\pm a$. The velocity distribution between the plates will be a symmetric function of the variable x_2. If now we consider a flow where one plate is moving with respect to the other, and this is the only cause of fluid motion, then we have plane Couette flow and, as is well known, the velocity profile will now be an antisymmetric function of x_2. However, the molecular viscosity of the fluid will be unaffected by the different macroscopic symmetries and will be the same in both cases.

If we now extend this discussion to the case of turbulent mean velocities and inquire about the behaviour of the effective turbulent viscosity (ν_t, say: for a definition see Section 1.5 of reference [1]), it is clear that this will be very different in the two cases, and arguably that should apply to the cascade process as well.

In isotropic turbulence, the cascade is described by the Lin equation, with the key quantity being the transfer spectrum $T(k)$. Its extension to an inhomogeneous case will bring in a number of transfer spectra, such as T_{11}, T_{12} and so on. In order to cope with the dependence on spatial coordinates, the introduction of centroid and relative coordinates that we used in the previous post will prove useful. Recall that we considered a covariance function $C(\mathbf{x},\mathbf{x’})$, leaving the time variables out for simplicity and introduced the change of variables to centroid and relative coordinates, thus: \[\mathbf{R} = (\mathbf{x} + \mathbf{x’})/2 \qquad \mbox{and} \qquad

\mathbf{r} = (\mathbf{x} – \mathbf{x’}). \] In this case one component of the spectral tensor could be written as: $T_{11}(\mathbf{k}, R_2)$, where we have Fourier transformed with respect to the relative coordinate only. Then, at least in the core region of the flow, we could expand out the dependence on the centroid coordinate in Taylor series. In this way we could separate the wavenumber cascade from spatial effects, such as production and spatial energy transfer.

Ideally one could even use a closure theory: the covariance equation of the DIA has been validated by the LET theory [2] and, although some work has been done on this in the past, a really serious approach would require a lot of bright young people to get involved. Unfortunately, vast numbers of bright young people all over the world are involved in complicated pedagogical exercises in cosmology, particle theory, string theory, quantum gravity and so on, most of which has gone beyond any proper theoretical foundation. Ah well, important but less glamorous problems like turbulence must await their turn.

For completeness, I should emphasise that all flows discussed above are assumed to be incompressible and well-developed.

[1] W. D. McComb. The Physics of Fluid Turbulence. Oxford University Press, 1990.
[2] W. D. McComb and S. R. Yoffe. A formal derivation of the local energy transfer (LET) theory of homogeneous turbulence. J. Phys. A: Math. Theor. 50:375501, 2017.

Can statistical theory help

with turbulence modelling?

Can statistical theory help with turbulence modelling?
When reading the book by Sagaut and Cambon some years ago, I was struck by their balance between fundamentals and applications [1]. This started me thinking, and it appeared to me that I had become ever more concentrated on fundamentals in recent years. In other words, I seemed to epitomize the old saying about scholarship consisting of `learning more and more about less and less'!

It was not always so. I began my career in research and development, which was very practical indeed. Then my employers sent me back to university where I took a degree in theoretical physics, followed by a PhD on the statistical theory of turbulence. Obviously the rot had set in; but, even so, in later years I did quite a lot of experimental work on drag reduction by additives and also turbulent diffusion. At least these topics had a practical orientation. Moreover, I have also used the $k-\varepsilon$ model to carry out calculations on the `jet in crossflow' problem. This might seem surprising, but it arose quite naturally in the following way.

Around about 1980 I had a call from a colleague in the maths department at Edinburgh. The Iran-Iraq war had recently broken out, and one of his MPhil students came from that part of the world. The student had decided that he would rather take a PhD than go home and be involved in the fighting. Very understandable, but the difficulty was that he needed a more substantial project. At present he was studying the jet in crossflow problem, using ideal flow methods. My colleague wondered if I could join in as co-supervisor and introduce some turbulence to the project in order to make it more realistic.

Lacking any experience in this field, I happily agreed to join in, and proposed that we use the $k-\varepsilon$ model, which at the time was the best known of the engineering models. We set out on a programme of studying both the model and associated numerical methods, in the process considering a hierarchy of problems of increasing difficulty, until we reached the jet in crossflow.

This was a long time ago, but two things about this PhD supervision remain in my memory. First, the student was a mathematician and had no prior knowledge of numerical computation. This leaves me with an abiding impression that he initially found it very difficult to realise that we did not need to be able to solve an equation in the mathematical sense. Because of this, we had many discussions which appeared to be going well and then ended in frustration. Secondly, once we managed to encourage him to overcome his reluctance and try to use the computer, he proved to be a natural and worked rapidly through our hierarchy of problems, ending up with useful results in a commendably short time. This happened at a time of upheaval for me, when I was moving from the School of Engineering to the School of Physics, so I have only a rather vague memory of how things turned out. I believe that he got his PhD and then went on somewhere in England as a postdoc. Whether the results were published or not, I don’t recall. But the experience left me with an appreciation of the value of a practical engineering model, where my own fundamental work would have been of little assistance. A short discussion of the $k-\varepsilon$ model can be found in Section 3.3.4 of my book, given as reference [2] below.

When considering how statistical theory might help, we should first recognize that it does give rise to a class of models, beginning with the Eddy Damped Quasi-Normal model (which is cognate to the self-consistent field theory of Edwards) and has a single adjustable constant. It is, however, restricted to homogeneous turbulence. What we could really do with is something like $k-\varepsilon$, which is a single-point theory, but which arises in a systematic way from a two-point statistical theory. The value of the latter is that it takes into account spatially (and temporally) nonlocal effects.

The details of the statistical closure theories are complicated, but the basic idea of how one might try to derive single-point engineering models is quite simple. The key quantity is the covariance of two fluctuating velocities at different points (and times) and a theory consists of a closed set of equations to determine the covariance. In general, the covariance tensor is a matrix of nine covariance functions, although symmetry will often reduce that. We will consider just one such function, which we write as $C(\mathbf{x},\mathbf{x'})$, leaving the time variables out for simplicity. We then make the change of variables to centroid and relative coordinates, thus: $$\mathbf{R} = (\mathbf{x} + \mathbf{x'})/2 \qquad \mbox{and} \qquad \mathbf{r} = (\mathbf{x} - \mathbf{x'}). $$

Now, the statistical theories are studied for the homogeneous case in order to simplify the problem. That is, we assume that there is no dependence on the centroid coordinate; and Fourier transform into wavenumber space, with respect to the relative variable. However, the basic derivation and renormalization are not restricted to this case, and we can write down equations for the general case. Then, recognizing that most turbulent shear flows have a smooth dependence on the centroid coordinate, we can envisage expanding in the centroid coordinate, with coefficients obtained as integrals over wavenumber. Then, setting $x=x'$, we could end up with single-point equations, where coefficients are determined by integrals that arise in the fundamental theory.

This would not be a trivial process but, given the huge importance of turbulence calculations in a variety of applications, it is perhaps surprising that it has been so comprehensively neglected. A recent discussion of statistical two-point closures can be found in reference [3]. For completeness, I should mention that a second edition of [1] has appeared and I understand that a third edition is in the pipeline.

[1] P. Sagaut and C. Cambon. Homogeneous Turbulence Dynamics. Cambridge University Press, Cambridge, 2008.
[2] W. D. McComb. The Physics of Fluid Turbulence. Oxford University Press, 1990.
[3] W. D. McComb and S. R. Yoffe. A formal derivation of the local energy transfer (LET) theory of homogeneous turbulence. J. Phys. A: Math. Theor., 50:375501, 2017.