The mysterious stirring forces

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In the late 1970s there was an upsurge in interest in the turbulence problem among theoretical physicists. This arose out of the application of renormalization group (RG) methods to the problem of stirred fluid motion. As this problem was restricted to a very low wavenumber cutoff, these approaches had nothing to say about real fluid turbulence. Nevertheless, the work on RG stimulated a lot of speculative discussion, and one paper referred to `the mysterious stirring forces'. I found this rather unsettling, because I had been familiar with the concept of stirring forces from the start of my PhD project in 1966. Why, I wondered, did some people find them mysterious?

As time passed, I came to the conclusion that it was just lack of familiarity on the part of these theorists, although they seemed quite happy to launch into speculation on a subject that they knew very little about. (Well, it was just a conference paper!) So I was left with the feeling that one day it might be worth writing something to debunk this comment. Recently it occurred to me that it would make a good topic for a blog.

The standard form used nowadays for the stirring forces was introduced by Sam Edwards in 1964 and has its roots in the study of Brownian motion, and similar problems involving fluctuations about equilibrium. Let us consider the motion of a colloidal particle under the influence of molecular impacts in a liquid. For simplicity, we specialise to one-dimensional motion with velocity u. The particle will experience Stokes drag with coefficient η, per unit mass. Accordingly, we can use Newton's second law to write its macroscopic equation of motion as: \begin{equation} \partial u/\partial t =-\eta \,

u. \end{equation} At the microscopic level, the particle will experience the individual molecular impacts as a random force $f(t)$, say. So the microscopic equation of motion becomes: \begin{equation}\partial u/\partial t =-\eta \, u + f(t). \end{equation} This equation is known as the Langevin equation. In order to solve it, we need to specify f in terms of a physically plausible model.

We begin by noting that the average effect of the molecular impacts on the colloidal particle must be zero, thus we have: \begin{equation}\langle f(t) \rangle =0. \end{equation} As a result, the average of equation (2) reduces to equation (1), which is consistent. Then in order to represent the irregular nature of the molecular impacts, we assume that $f(t)$ is only correlated with itself at very short times $t\leq t_c$, where t_c is the duration of a collision. We can express this in terms of the autocorrelation function w as: \begin{equation} \langle f(t)f(t’) \rangle = w(t-t’), \end{equation} and \begin{equation} W(t) = \int_0^t\,w(\tau)\,d\tau, \end{equation} where \begin{equation} W(\tau)\rightarrow W = \mbox{constant}.\end{equation}

We can go on to solve the Langevin equation (2) for the short-time and long-time behaviour of the particle velocity $u(t)$, much as in Taylor’s Lagrangian analysis of turbulent diffusion. We can also derive the fluctuation-dissipation relation: see reference [1] for details.

In his self-consistent field theory of turbulence, Edwards drew various analogies with the theory of Brownian motion [2]. In particular, he went further than in equations (4) to (6), and chose the stirring forces to be instantaneously correlated with themselves; or: \begin{equation}w(t-t’) = W \delta(t-t’), \end{equation} where δ is the Dirac delta function. In the study of stochastic dynamical systems, this is known as `white noise forcing’. It allows one to express the rate at which the stirring force does work on the turbulent fluid in terms of the autocorrelation of the stirring forces [3].

It also provides a criterion for the detection of `fake theories'. These are theories which are put out by people with skill in quantum field theory and which purport to be theories of turbulence. Such theories do not engage with the established body of work in the theory of turbulence, nor do they mention how they overcome the problems that have proved to be a stumbling block for legitimate theories. Invariably, they attribute the purpose of the delta function to be to maintain Galiean invariance and clearly do not know what it is actually used for. In fact, the Navier-Stokes equations are trivially Galilean-invariant and adding an external force to them cannot destroy that [4].

[1] W. David McComb. Study Notes for Statistical Physics: A concise, unified overview of the subject. Bookboon, 2014.
[2] S. F. Edwards. The statistical dynamics of homogeneous turbulence. J. Fluid Mech., 18:239, 1964.
[3] W. D. McComb. The Physics of Fluid Turbulence. Oxford University Press, 1990.
[4] W. D. McComb. Galilean invariance and vertex renormalization. Phys. Rev. E, 71:37301, 2005.

Is the entropy of turbulence a maximum?

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In 1969 I published my first paper [1], jointly with my supervisor Sam Edwards, in which we maximised the turbulent entropy, defined in terms of the information content, in order to obtain a prescription for $\omega(k)$, the renormalized decay time for the energy contained in the mode with wavenumber k. Of course, in statistical mechanics, one

associates the maximum of the entropy with thermal equilibrium. So, in the circumstances, we were very frank about possible problems with this approach, having actually stated in the title that our system was ‘far from equilibrium’. Before we examine this aspect further, it may be of interest to look at the background to the work.

By the mid-nineteen sixties, there had been a number of related theories of turbulence, but the most important were probably Kraichnan’s direct-interaction approximation (DIA) in 1959 and the Edwards self-consistent field theory in 1964. At this time there seems to have been a mixture of excitement and frustration. It had become clear from experiment that the Kolmogorov $-5/3$ power law (or something very close to it) was the correct inertial-range form, and none of the various theories was compatible with it. Kraichnan ultimately concluded that he needed to change to a so-called Lagrangian-history coordinatate system, but otherwise could retain all the features of the DIA; whereas Edwards concluded that he needed to find a different way of choosing the response function, which in his case depended on $\omega(k)$. In my view, and irrespective of the merits or otherwise of the ‘maximum entropy’ method, Edwards made the right decision.

When I began my PhD research in 1966, my first job was to work out the turbulent entropy, using Shannon’s definition, in terms of the turbulent probability distribution; and then carry out a functional differentiation with respect to $\omega(k)$, in order to establish the presence of a maximum. What I didn’t know, was that Sam had himself carried out this calculation but had got stuck. In order to take the limit of infinite Reynolds numbers, he had to show that his theory was well behaved at three particular points in wavenumber space: $k=0$, $k=\infty$ and $|\mathbf{k}+\mathbf{j}|=0$, where j is a dummy wavenumber. He had been able to show the first two, but not the third. Not knowing that there was a problem, I soon discovered it, but by means of a trick involving dividing up the range of integration, I managed to show that it was well behaved. However, the prediction of the value of the Kolmogorov constant was not good, and this was not encouraging.

In later years, when I had a lot more experience of both turbulence and statistical physics, I thought more critically about this way of treating turbulence. The maximum entropy method is the canonical way of solving problems in thermal equilibrium where there are only either weak or very local interactions. If we take the para-ferromagnetic transition as an example, we can think of the temperature being reduced and an assembly of molecular magnets (i.e. spins on a lattice) tending to line up as the effective coupling increases. However, this process would be swamped by the imposition of a powerful external magnetic field. Similarly, the molecular diffusion process can be swamped by vigorous stirring. In the case of turbulence, it is possible to study absolute equilibrium ensembles by considering an initially stirred inviscid fluid in a finite system. If we replace the Euler equation by the Navier-Stokes equation, then the effect of the viscosity is symmetry-breaking and the system is dominated by a flow of energy through the modes.

This, of course is a truism of statistical physics: a system is either controlled by entropy or energy conservation. In the case of turbulence, it is always the latter. Turbulence is always a *driven* phenomenon. So while perhaps entropy is actually a maximum with respect to variation of $\omega(k)$, it may be too broad a maximum allow an accurate determination of $\omega(k)$. Also, it is worth bearing in mind, that it is not precisely turbulence but the statistical theory we are approximating it by, which needs to show the requisite behaviour.

In any case, in 1974 I published my local energy transfer theory of turbulence [2], which is in good accord with the basic physics of the turbulent cascade.

[1] S. F. Edwards and W. D. McComb. Statistical mechanics far from equilibrium. J.Phys.A, 2:157, 1969.
[2] W. D. McComb. A local energy transfer theory of isotropic turbulence. J.Phys.A, 7(5):632, 1974.

Analogies between critical phenomena and turbulence: 2

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In the previous post, I discussed the misapplication to turbulence of concepts like the relationship between mean-field theory and Renormalization Group in critical phenomena. This week I have the concept of ‘*anomalous exponents*’ in my sights!

This term appears to be borrowed from the concept of anomalous dimension in the theory of critical phenomena, so we start from a consideration of dimension, bearing in mind that the dimension of the space can be anything from $d=1$ up to $d=\infty$, and is not necessarily an integer. In critical phenomena it is usual to define three different kinds of dimensionality, as follows:

[a] **Scale dimension.** This is defined as the dimension of a physical quantity as established from the effect of a scaling transformation. Confusingly, this is normally just referred to as dimension.

[b] **Normal (canonical) dimension.** This is the (scale) dimension as established by simple dimensional analysis.

[c] **Anomalous dimension.** This is the dimension as established

under RG transformation.

In this context, normal dimension is regarded as the naïve dimension and anomalous dimension is regarded as the actual or correct dimension. In turbulence we don't have dimensionality as a playground, so the merry band of would-be turbulence theorists have extended the concept to the *exponents* of power-law forms of the moments of the velocity field plotted against order. The Kolmogorov forms (dimensional analysis) are seen as canonical and the actual (i.e. measured) exponents are seen as anomalous. The former are seen as wrong and the latter as correct. Naturally, the true believers in intermittency corrections have seized on this nomenclature as adding something to their case. (Also, see my post of 21 January 2021).

Let us actually apply the concept of scale dimension d_s (say) in three-dimensional turbulence (i.e. $d=3$), using the procedures from critical phenomena (see Section 9.3 of [1]) to the energy spectrum $E(k)$. That is, we express the spectrum in terms of the total energy E, thus \[\int\,d^3k\,E(k) = E \quad \mbox{hence} \quad E(k) \sim E\,k^{-3}.\] So, bearing in mind that wavenumber has dimensions of inverse length, it follows that the canonical scale dimension is $d_s = 3$ in $d=3$.

If we now consider the Kolmogorov spectrum based on scale invariance and an inertial transfer rate ε_T, dimensional analysis gives us \[E(k) \sim \varepsilon_T\,k^{-5/3} .\] As this result can also be got from RG transformation, properly formulated for macroscopic fluid turbulence, and employing rational approximations (see [2] – [5]), it follows that K41 corresponds to the anomalous dimension $d_E = 5/3$. So much for inept comparisons with critical phenomena.

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

[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.

Analogies between critical phenomena and turbulence: 1

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In the late 1970s, application of Renormalization Group (RG) to stirred fluid motion led to an upwelling of interest among theoretical physicists in the possibility of solving the notorious turbulence problem. I remember reading a conference paper which included some discussion that was rather naïve in tone. For instance, why did turbulence theorists study the energy spectrum rather than something else? Also, rather unsettlingly, there was a reference to the ‘mysterious stirring forces’ (*sic)*: I shall return to that comment in a future post. However, although no turbulence theory emerged from this activity, a way of thinking did, and this found a receptive audience in those members of the turbulence community who believe in intermittency corrections. In my

view, one set of views is as unjustified as the other, and I shall now explain why I think this.

To understand how these views came about, we need to consider the background in critical phenomena. During the 1960s, theorists in this area began to use concepts like scaling and self-similarity to derive exact relationships between critical exponents. (In passing, I note that in fluid dynamics these tools had already been in active use for more than half a century!) In this way, the six critical exponents of a typical system could be reduced to just two to be determined. At first the gap was bridged by mean-field theory, but then RG came along and the problem was solved.

It is important to know that RG can be viewed, in some respects, as a correction to mean-field theory. As a result, theorists in this field essentially ended up taking the view: ‘mean-field theory, bad! RG good!’, and this had a tendency to spill over into other areas as a sort of judgement. In general this was the attitude during the 1980s/90s, and few paused to reflect that other phenomena might belong to a different universality class. For instance, should the self-consistent field theory of multi-electron atoms be ruled out, because RG is better than mean-field theory at describing the para-ferromagnetic phase transition? Fortunately, this sort of thinking has presumably died out by now, but it has left an unhelpful residue in turbulence theory.

One form of this is the assertion that the Kolmogorov ‘$-5/3$’ energy spectrum is a mean-field theory, and that an RG calculation would lead to an exponent of the form ‘$-5/3+\mu$’; precisely what the ‘intermittency correction’ enthusiasts had been saying all along! The snag with this is that the derivation of the Kolmogorov spectrum does not rely on a mean-field step, nor indeed on the invariable accompaniment of a self-consistent field step. In fact, this can be a problem in critical phenomena. People tend to refer loosely to mean-field theories, without mentioning that they are also self-consistent theories. Actually in turbulence we have various self-consistent field theories which do not predict the Kolmogorov exponent and one which does [1].

In my next post, I will develop this topic further. In the meantime, a general background account of these matters may be found in the book cited below as [2].

[1] 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.

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