Free decay of isotropic turbulence as a test problem.

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When I began my postgraduate research in 1966, I quickly decided that there was one problem that I would never work on. That was the free decay of the kinetic energy of turbulence from some initial value. Although, as the subject of my postgraduate research was the turbulence closure problem, there didn’t seem to be any danger of my being asked to do so.

This particular free decay problem, as widely discussed in the literature, can, if one likes, be regarded as a reduced form of the general closure problem. Instead of trying to calculate the two-point correlation (or, equivalently, the energy spectrum), one is simply trying to calculate the decay curve with time of the total energy. This involves making various assumptions about the nature of the decay process and the most crucial seemed to be that a certain integral was constant with respect to time during the decay: this was generally referred to as the *Loitsyansky invariant.*

We can introduce this by considering the behaviour of the energy spectrum at small values of the wavenumber k. This can be written as a Taylor polynomial \[E(k,t) = E_2(t)k^2 + E_4(t)k^4 + \dots .\] Here the coefficient $E_4(t)$, when Fourier transformed to real space, is known as the Loitsyansky integral, and in general it depends on time. It seemed that this was indeed invariant during decay for the case of isotropic turbulence but it had been shown that this was not necessarily the case for turbulence that was merely homogeneous. The problem was that a correlation of the velocity with the pressure, which is suppressed by symmetry in the isotropic case, existed in the more general case. The difficulty here is that the pressure can be expressed as an integral over the velocity field and so the correlation $\langle u p \rangle$ is long-range in nature, and this invalidates the proof of invariance of E_4 which works for the isotropic case.

So far so good. What puzzled me at the time was that this failure in the more general case somehow seemed to contaminate the isotropic case. People working in this field seemed unwilling to reply on the invariance of E_4 even for isotropic turbulence. However, with the accretion of knowledge over the years (I'd like to claim wisdom as well, but that might be too big a stretch!), I believe that I understand their concerns. At the time, the only practical application of the theory was to grid turbulence; and although this was reckoned to be a good approximation to being isotropic, it might not be perfect; and it might vary to some extent from one experimental apparatus to another. And just to add to the confusion, at about that time (although I didn't know it) Saffman published a theory of grid turbulence in which $E_2(t)$ was an invariant. This led to controversy based on E_2 versus E_4 which is with us to this day.

In more recent years, I have had to weaken my position on this matter, because my students have found it interesting to do free-decay calculations, in order to compare our simulations with those of others. So when I was preparing my recent book on HIT, I decided it would provide a good reason to really look into this topic. As part of this work, I was checking various results and to my astonishment, when I worked out E_2 I found that it was exactly zero. This work has been published and includes a new proof of the invariance of E_4 which is based on conservation of energy [1]. In passing, I should note that the refereeing process for this paper was something that I found educational and I will refer to that in future posts when I get onto the subject of peer review.

Shortly after I published this work, a paper on grid turbulence appeared and it seemed that their results suggested that E_2 was non-zero. I sent a copy of [1] to the author and he replied `evidently grid turbulence is less isotropic than we thought'. This struck me as a crucial point. If we are to make progress and have meaningful discussions on this topic, we need to recognise that free decay of isotropic turbulence and grid turbulence are two different problems. In fact, as things have moved on from the mid-sixties, we also have to consider DNS of free decay as being in principle a different problem. Let us now examine the three problems in turn, as follows:

1. Free decay of the turbulent kinetic energy is a mathematical problem which can be formulated precisely for homogeneous isotropic turbulence.

2. Grid-generated turbulence evolves out of an ensemble of wakes and is stationary with time and inhomogeneous in the streamwise direction. In order to make comparisons with free decay, it is necessary to invoke Taylor's hypothesis of frozen convection.

3. DNS of freely decaying turbulence is based on the Navier-Stokes equations discretised on a lattice. Quite apart from the errors involved (analogous to experimental error in the grid-turbulence case), representation on a lattice is symmetry breaking for all continuous symmetries. The two principal ones in this case are *Galilean invariance* and *isotropy.*

Essentially, these are all three different problems and if we wish to make comparisons we have to at least bear that fact in mind. I have lost count of the many heated arguments that I have heard or taken part in over the years which ran along the lines: A says `The sky is blue!' and B replies: `Oh no, I assure you that grass is green!' In other words they are not talking about the same thing. That may seem rather extreme but supposing one is *momentum conservation* and the other is *energy conservation*. Such a waste of time and energy (and momentum, for that matter).

[1] W. D. McComb. Infrared properties of the energy spectrum in freely decaying isotropic turbulence. Phys. Rev. E, 93:013103, 2016.

Stationary isotropic turbulence as a test problem.

Stationary isotropic turbulence as a test problem.

When I was first publishing, in the early 1970s, referees would often say something like `the author uses the *turbulence in a box concept*' before going on to reveal a degree of incomprehension about what I might be doing, let alone what I actually was doing. A few years later, when direct numerical simulation (DNS) had got under way, that phrase might have had some significance; and indeed its use is now common, albeit qualified by the word `periodic'. Of course, when Fourier methods were introduced by Taylor in the 1930s, it was in the form of Fourier series. But by the 1960s it was becoming usual among theorists to briefly introduce Fourier series and then take the infinite system limit and turn them into Fourier transforms: or, increasingly, just to formulate the problem straightaway in the infinite system. However, it can be worth one's while starting with the finite cubic box of side L, and thinking in terms of the basic physics, as well as the Fourier methods.

In order to represent the velocity field in terms of Fourier series, we introduce the wavevector \[\mathbf{k}=(2\pi/L)\{n_1,n_2,n_3\},\] where the integers n_1,n_2,n_3 all lie in the range from $-\infty$ to ∞. Fourier sums are taken over the discrete values of

$\mathbf{k}$. Then the transition to the continuous, infinite system is made by taking the limit of infinite system size, such that $$\lim_{L\rightarrow \infty}\left(\frac{2\pi}{L}\right)^3\sum_{\mathbf{k}} = \int d^3\,k.$$ As ever in physics, we assume that everything is well-behaved; and that both the field variables and their transforms exist, being independent of system size as we go to this limit.

We do not have to restrict these ideas to the Fourier representation. They are generally true when we make the transition from classical mechanics to continuum mechanics. To do this, we begin with a finite system and replace discrete objects by densities. A continuous (or field) representation is introduced by defining continuous densities in the limit of infinite system size. All physical observables must be expressed in terms of densities or rates. They cannot depend on the size of the system, otherwise we would be unable to take the continuum limit. So, if we formulate turbulence in real space in terms of structure functions in a box, then theoretical expressions for the structure functions (or equivalently, the moments) must not depend on the size of the box. This provides us with a basic first test for any theory; and to our knowledge there have been some surprising failures to recognise this. We will come back to two specific examples presently. First we will look at the general question of how to test theories.

Now, stationary isotropic turbulence can be rigorously formulated as a mathematical problem, where `rigour' is taken to be in the sense of theoretical physics, but it does not occur in nature or indeed in the laboratory. It is true that it may occur to a reasonable approximation in geophysical and astronomical flows, but at the moment it seems that DNS might be our best bet for testing mathematical theories of isotropic turbulence. So it behoves us to examine the question: how representative is DNS of the mathematical problem that we are studying?

Well, of course DNS has been an active field of research for several decades now and this aspect has not been neglected. Nevertheless, one is left with the impression that it is very much a pragmatic activity, governed by `rule of thumb' methods. For instance, when we began DNS at Edinburgh in the 1990s, I asked around for advice on the maximum value of the wavenumber that we should use, as this seemed to vary from less than the Kolmogorov dissipation wavenumber to very much greater. The consensus of advice that I received was to choose $k_{max} = 1.5 k_d$, and this is what we did. Later on, in 2001, we demonstrated a rational procedure for choosing k_{max}: see Figure 2 of reference [1] or Figure 1.6 of reference [2]. One conclusion that emerges from this, is that to resolve the dissipation rate might mean devoting one's entire simulation to the dissipation range of wavenumbers!

In recent years there seems to have been more emphasis on resolving the largest scale of the turbulence, although much of this work has been for the case of free decay. But concerns remain, particularly in the terms of experimental error. It is also necessary to note a fundamental problem. The mere fact of representing the continuum NSE on a discrete lattice is symmetry breaking for Galilean invariance and isotropy, to name but two. I'm not sure how one can take this into account, except by considering a transition towards the continuum limit and looking for asymptotic behaviour. This could involve starting with a `fully resolved' simulation and looking at increasingly finer mesh sizes. To say the least this would be very expensive in terms of computer storage and run time. Naturally, workers in the field always want the highest possible Reynolds number. But, if you begin with low Reynolds numbers, it is cheap and easy to do, and you can learn something from the variation of observables with Reynolds number. There exist some well-known simulations that have employed vast resources to achieve enormous Reynolds numbers and yet provide only a few spot values without any error bars, with no indication of asymptotic behaviour, and I understand suspicions about how well-resolved they are. An awful warning to us all!

Lastly, two more awful warnings. First, as we discussed in the previous post, Kraichnan’s asymptotic solution of DIA depends on the largest scale of the system. That in itself is enough to rule it out as unphysical, whether one accepts Kolmogorov (1941) or not. However, as I pointed out, our computations at Edinburgh do not support this asymptotic form, which was obtained analytically using approximations that Kraichnan found plausible. A critical examination of that analysis is in my opinion long overdue.

Secondly, we have the Kolmogorov (1962) form of the energy spectrum, which also depends on the largest scale of the system. Probably few people now take this work seriously, but its baleful presence influences the turbulence community and lends credence to the increasingly unrealistic idea of intermittency corrections. In fact it has recently been shown that the inclusion of the largest scale destroys the widely observed scaling on Kolmogorov variables [3]. This should have been obvious, without any need to plot the graphs!

[1] W. D. McComb, A. Hunter, and C. Johnston. Conditional mode-elimination and the subgrid-modelling problem for isotropic turbulence. Phys. Fluids, 13:2030, 2001.

[2] W. David McComb. Homogeneous, Isotropic Turbulence: Phenomenology, Renormalization and Statistical Closures. Oxford University Press, 2014.

[3] W. D. McComb and M. Q. May. The effect of Kolmogorov (1962) scaling on the universality of turbulence energy spectra. arXiv:1812.09174[physics.fluid-dyn], 2018.

Asymptotic behaviour of the Direct Interaction Approximation.

Asymptotic behaviour of the Direct Interaction Approximation. As mentioned previously, Kraichnan's asymptotic solution of the DIA, for high Reynolds numbers and large wavenumbers, did not agree with the observed asymptotic behaviour of turbulence. His expression for the spectrum was $E(k)=C'\varepsilon^{1/2}U^{1/2}k^{-3/2}$, where U is the root-mean-square velocity and C' is a constant. In 1964 (see [1] for the reference) he wrote: `Recent experimental evidence gives strong support to [the Kolmogorov `-5'3' form] and rules out [the `-3/2' form above] as a correct asymptotic law.'

However, Kraichnan's result is not actually an asymptotic form. The rms velocity U is in fact part of the solution, not the initial conditions. We may underline this by writing $U= [\int_0^\infty \, E(k)\,dk]^{1/2}$, which allows us to rewrite the Kraichnan result as $E(k)=C' \varepsilon^{1/2}[\int_0^\infty \, E(k)\,dk]^{1/4}\, k^{-3/2}$. So, far from being an asymptotic solution, this appears to be a form of transcendental equation for the energy spectrum.

Now you may object that the dissipation rate is also part of the solution, rather than of the initial conditions, and hence this is also a criticism of the Kolmogorov form. But this is not so. The dissipation only appears because it is equal to the inertial transfer rate. From the simple physics of the inertial range in wavenumber space, the appropriate quantity is the maximum value of the inertial flux of energy through modes, which we will denote by ε_T. Hence the Kolmogorov form should really be $E(k) \sim \varepsilon_T^{2/3}k^{-5/3}$. Of course Kolmogorov worked in real space and derived the `2/3' law. But in 1941 Obukhov recognised that in wavenumber space the relevant quantity was the scale-invariant energy flux, as did Onsager a few years later.

A way of putting the Kraichnan result in a more asymptotic form was given by McComb and Yoffe [1], who made use of the asymptotic Taylor surrogate for the dissipation rate, $\varepsilon = C_{\varepsilon,\infty} U^3/L$, where L is the integral length scale and $ C_{\varepsilon,\infty} = 0.468 \pm 0.006$ [2], to substitute for U in the Kraichnan spectrum, and obtained: $E(k) = C'C_{\varepsilon,\infty}^{-1/3}\varepsilon^{2/3}L^{\beta}k^{-5/3 + \beta}$, where $\beta = 1/6$. Note that we have changed μ in that reference to β in order to avoid any confusion with the so-called intermittency correction, which normally is represented by that symbol.

Kraichnan only computed the Eulerian DIA for free decay at low Reynolds numbers. However, in 1989 McComb, Shanmugasundaram and Hutchinson [3] reported calculations for free decay of both DIA and LET for Taylor-Reynolds numbers in the range $0.5 \leq R_{\lambda}(t_f) \leq 1009$ where t_f is the final time of the computation. These results do not support the asymptotic form of the DIA energy spectrum, as given above. It was found that (for example) at $ R_{\lambda} (t_f) = 533$, the two theories were virtually indistinguishable and both gave the Kolmogorov spectrum to within the accuracy of the numerical methods. It was shown that this result was not an artefact of the initial conditions, by taking $k^{-3/2}$ as the initial spectrum, whereupon it was found that both theories evolved away from this form to once again give $k^{-5/3}$ as the final spectrum.

There is much that remains to be understood about Eulerian turbulence theories and the behaviour of two-time correlations.

[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, A. Berera, S. R. Yoffe, and M. F. Linkmann. Energy transfer and dissipation in forced isotropic turbulence. Phys. Rev. E, 91:043013, 2015.
[3] W. D. McComb, V. Shanmugasundaram, and P. Hutchinson. Velocity derivative skewness and two-time velocity correlations of isotropic turbulence as predicted by the LET theory. J. Fluid Mech., 208:91, 1989.

A brief summary of two-point renormalized perturbation theories.

A brief summary of two-point renormalized perturbation theories.
In the previous post we discussed the introduction of Kraichnan's DIA, based on a combination of a mean-field assumption and a new kind of perturbation theory, and how it was supported by Wyld's formalism, itself based on a conventional perturbation expansion of the NSE. This was not too surprising, as Kraichnan's mean field assumption involved his infinitesimal response function which the Wyld comparison showed was the same as the viscous response function, and hence not a random variable. By 1961 it was known that the asymptotic solution of DIA was incorrect, with implications for both the Wyld formalism (and the MSR formalism later on:

see previous post).

The next step forward was the theory of Edwards [1] in 1964, which was restricted to the more limited single-time covariance and also to the stationary case. This took as its starting point the Liouville equation for P, the probability distribution functional of the velocity field, and went beyond the mean-field case to calculate corrections to it self-consistently. That is, Edwards made the substitution $P\equiv P_0 + (P - P_0)$ and then expanded in powers of the correction term $\Delta P = P - P_0$. Then, taking P_0 to be Gaussian, and exploiting the symmetries of the system, Edwards gave a highly intuitive treatment of the problem, in which he drew strongly on an analogy with the theory of Brownian motion. It turned out that the resulting theory was closely related to the DIA and, like it, did not agree with the Kolmogorov spectrum.

The following year Herring [2], using formal methods of many-body theory, produced a self-consistent field theory which was much more abstract than the Edwards one, but yielded the same energy equation. Then, in 1966 he generalised this theory to the two-time case [3]. All three theories [1-3] led to the same energy equation as DIA, but all differed in the form of the response equation.

Now, it is in the introduction of the response equation that the renormalization takes place, and it is in the form of the response equation that the deviation from Kolmogorov lies, so this difference between these response equations raises fundamental questions about all these theories. Various interpretations were offered at the time, but these were all phenomenological in character. It was much later that a uniform, fundamental diagnosis was offered and I will come on to that presently. But this was the situation when I began post-graduate research with Sam Edwards in October 1966. The exciting developments of the previous decade seemed to be leading to a dead end, and my first task was to choose the response function of the Edwards theory in a new way, such that it maximised the turbulent entropy [4].

On the basis of the Edwards analysis, his theory had failed under the extreme circumstances of an infinite Reynolds number limit, in which the input was modelled by a delta-function at the origin in k-space and the dissipation was represented by a delta-function at $k=\infty$. Edwards argued that under these circumstances the Kolmogorov spectrum would apply at all wavenumbers, and in his original theory this led to an infra-red divergence in the integral for the response function. (Note: Kraichnan used the scale-invariance of the inertial flux Π as his criterion for the inertial range, but the two methods are mathematically equivalent.) The `maximum entropy’ theory [4] certainly achieved the result of eliminating the infra-red divergence, but that was about as much as one could say for it. It became clearer to me later that it was not a very sound approach.

It is a truism in statistical physics that a system is either dominated by entropy or energy. If we consider a system made of many microscopic magnets on a lattice then the entropy will determine the distribution. However if we switch on a powerful external magnetic field, all the little magnets will line up with it and (small fluctuations aside) entropy has no say in the matter! It is just like that in turbulence. The system is dominated by a symmetry breaking current of energy through the modes, running from small to large wavenumbers, where it is dissipated by viscosity. There is no real reason to assume that the entropy determines the turbulence response.

When I was in my first post-doctoral job, I gave a talk to some theorists. I explained my early ideas on how energy transfer might determine the turbulence response. They heard me out politely, and then I made the mistake of mentioning the maximum entropy work. Immediately they became enthusiastic. ‘Tell us about that’, they said. The impression they gave was ‘now that’s a real theory!’ I was in awe of them as they were much older and more experienced than me, and talked so authoritatively about all aspects of theoretical physics. Nevertheless, this was my first inkling of conventional thinking. The implication seemed to be: it was a text-book method, so it must be good.

Over the next few years I developed the local energy transfer (LET) theory [5, 6], and also offered a unified explanation of the failure of first-generation renormalized perturbation theories. The further extension of this work to the two-time case has had a rather chequered history and will be the subject of further posts.

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