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I have written about the problems posed by the different cultures to be found in the turbulence community; and in particular of the difficulties faced by some referees when confronted by Fourier methods. My interest in the matter is of course the difficulties faced by the author who dares to use Fourier transforms when he encounters such an individual. In my post on 20 April 2020, I told of the referee who described Fourier analysis as ‘*the usual wavenumber murder*’. Thinking of this brought back a rather strange incident from the mid-1970s, and it occurs to me that it really underlines my point.

In those days, we used to get visitors from the United States, who would come for a day and ask various people about their work. I seem to recall that they were sponsored by the Office for Naval Research and, as we benefited from a huge flow of NASA reports, stemming from their various programmes, it seemed only fair to send something back.

One particular visitor was a fluid dynamicist who worked on the lubrication of journal bearings. He was known to my colleagues in this area, who told me that he was eminent in that field. So, once he was settled in my office and we had got over the usual preliminaries, he asked me to explain my theoretical research to him. I went to the blackboard and happily began explaining about eliminating the pressure from the Navier-Stokes equation and then how to Fourier transform it.

I hadn’t got very far, when he held up his hand and said. ‘Stop right there! I wouldn’t use Fourier transforms with a nonlinear problem like turbulence.’

I was a little bit taken aback, but my main reaction was that this was a chance for me to learn something, because it was at that time that I was receiving reports from JFM referees which were hostile to the use of Fourier methods.
I didn’t waste time in asking him why. I just asked what he would use instead. His reply astonished me. ‘I would use the Green’s function method.’

In the circumstances I saw no point in continuing and changed the topic to talk about my other work. He seemed quite happy about that. Perhaps it was just a cunning plan to avoid listening to some boring mathematics for an hour or so?
At this stage it will be clear to many people why I did not continue the discussion. But for those who don’t know, there were two points:
A. My visitor was wrong at the most fundamental level. Green’s functions are only applicable to linear problems. For instance, we can eliminate the pressure field from the NSE, because it satisfies a Poisson’s equation, which is of course linear.
B. As a sort of corollary of awfulness, a standard method of evaluating Green’s functions is by the use of Fourier transforms!
These matters are discussed in detail in Appendix D of reference [1] below.

The title of the poem by Alexander Pope has passed into the language as a caution against being too authoritative when one is not really an expert. The question of who does more harm, someone who thinks he knows all about Fourier methods; or someone who is frightened of them and behaves in a childish way, is really a moot point.

[1] W. D. McComb. The Physics of Fluid Turbulence. Oxford

University Press,
1990.

Intermittency, intermittency, intermittency!

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It is well known that those who are concerned with the sale of property say that the three factors determining the value of a house are: location, location, location. In fact I believe that there is a television programme with that as a title. This trope has passed into the general consciousness; so much so, that a recent prime minister declared his principal objectives in government to be: education, education, education. (Incidentally, I wonder how that worked out?)

My use of the title here is not to suggest that I think that intermittency is the dominant feature of the turbulent velocity field, or indeed of any particular importance, so much as to draw attention to the fact that there are three types of turbulent intermittency. Of course in complicated situations such as in turbomachinery, an anemometer signal can be interrupted by the passage of a rotor, say. That would be a form of intermittency. However, by intermittency, what I have in mind is something intrinsic to the turbulent field and not caused by some external behaviour. I believe that is what most people would mean by it.
For convenience, we may list these different types, as follows:

1. **Free surface intermittency.** This form of intermittency occurs in flows like wakes and unconfined jets. It arises from

the irregular nature of the boundary of the flow. An anemometer positioned at the edge of the flow will sometimes register a turbulent signal and sometimes not. There is also a dynamical problem posed by the interaction between the flow of the wake or jet and the ambient fluid, but that is not something that we will pursue here.

2. **The bursting process in pipe flow.** This was discovered in the 1960s, when it was found that a short-sample-time autocorrelation could show a near-sinusoidal variation with time, corresponding to a sequence of events in which turbulent energy was generated locally in both space and time. Measurement of the bursting period was helpful in understanding the mechanism of drag reduction by polymer additives.

3. **Internal intermittency.** This is the apparent inability of the eddying motions of turbulence to fill space, even in isotropic turbulence. Originally it was referred to as the *dissipation intermittency* and then later on as the *fine-structure intermittency*. In recent years it has been established that by means of high-Reynolds number simulations that this inability to fill space is in fact present at all length scales. Thus the growing modern practice is to describe it as *internal* which distinguishes it from the two types of intermittency above.

An account of all three types may be found in Section 3.2 of the book [1], although at that time I used the term *fine-structure intermittency*, in line with other writers at that time. I should also point out that I would no longer give the same prominence to the instantaneous dissipation. I am now clear that the failure to distinguish between this and its mean value; combined with the failure to recognise that the significant quantity in determining the inertial-range spectrum/structure-function is the inertial transfer rate, underpins much of the confusion over the $k^{-5/3}$ (or $r^{2/3}$) result for the inertial range. I have written quite a lot about this matter in recent years and expect to write a great deal more.

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

Does the failure to use spectral methods harm one's understanding of turbulence?

Does the failure to use spectral methods harm one's understanding of turbulence?

Vacation post No. 3: I will be out of the virtual office until Monday 19 April.

As described in the previous post, traditional methods of visualising turbulence involve vaguely specified and ill-defined eddying motions whereas Fourier methods lead to a well-defined problem in many-body physics. This seems to be a perfectly straightforward situation; and one might wonder: in what way do fluid dynamicists feel that the Fourier wavenumber space representation is obscuring the physics? Given that they regard a vortex-based picture, however imprecise, as `the physics', I suspect (a suspicion based on many discussions over the years!) that the problem arises when they try to reconcile the two formulations. Of course, in an intuitive way, one may associate large wavenumbers with small spatial separations. That is, `high k' corresponds to `small r' and *vice versa*. But those attempts, which one sees from time to time, to interpret the \$k\$-space picture in terms of arbitrarily prescribed vortex motions in real space, seem positively designed to cause confusion. It is important to bear in mind that the Fourier representation *reformulates* the problem, and you should study it on its own terms, even if you long for vortices!

Does this matter? I think it does. For example, I can point to the strange situation in which (it seems) most fluid dynamicists believe that there should be intermittency corrections to the exponent of Kolmogorov's $k^{-5/3}$ energy spectrum, whereas it seems that most theoretical physicists (who work in wavenumber space) do not. The hidden point here, is that Kolmogorov worked in real space, and derived the $r^{2/3}$ form of the second-order structure function, for an intermediate range of values of r where the form of the input term and the viscous dissipation could both be neglected, thus introducing the inertial range. His theory was inconsistent; in that he then considered the structure function to depend on the dissipation rate, even although this had been excluded from the inertial range. It is this step which gives some credibility to the possibility of intermittency effects, particularly as there may be some doubt about whether or not the dissipation rate in the theory is the average value or not.

The surprising thing is that, at much the same time, Obukhov worked in k-space, and identified the conservative, inertial flux of energy through the modes as being the key quantity determining the energy spectrum in the inertial range. It follows that, with the production and dissipation being negligible in this range of wavenumbers, the flux must be constant (i.e. independent of wavenumber) in the inertial range This was later recognized by Osager. Later still, this property became widely known and for many years has been referred to by theoretical physicists as *scale invariance*. Scale invariance is a general mathematical property and can refer to various things in turbulence research. It simply means that something which might depend on an independent variable, in either real space or wavenumber space, is in fact constant. It should be emphasised that the inertial flux is an average quantity, as indeed is the energy spectrum, and any intermittency must necessarily be averaged out. In fact a modern analysis leading to the $k^{-5/3}$ spectrum would start from the Lin equation. Therefore it is hard to see how internal intermittency, which is incidentally present on *all* scales can affect this derivation.

Does the use of spectral methods obscure the physics of turbulence?

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Vacation post No. 2: I will be out of the virtual office until Monday 19 April.

Recently, someone who posted a comment on one of my early blogs about spectral methods (see the post on 20 February 2020), commented that a certain person has said `spectral methods obscure the physics of turbulence'. They asked for my opinion on this statement and I gave a fairly robust and concise reply. However, on reflection, I thought that a more nuanced response might be helpful. As the vast majority of turbulence researchers work in real space, it seems probable that many would share that sentiment, or something very like it.

In fact, I will begin by challenging the second part of the statement. What precisely is meant by the phrase `the physics of turbulence'? In order to answer this question, let us begin by examining the concept of the turbulence problem in both real space and Fourier wavenumber space. Note that in what follows, all dependent variables are understood to be per unit mass of fluid, and we restrict our attention to incompressible fluid motion.

In real space, we have the velocity field $\mathbf{u}(\mathbf{x},t)$, which satisfies the Navier-Stokes equation (NSE). This equation expresses conservation of momentum and is local in x. It is also nonlinear and is therefore, in general, insoluble. From it we can derive the Karman-Howarth equation (KHE), which expresses conservation of energy and relates the second-order moment to the third-order moment. This is also local in x, and is also insoluble, as it embodies the statistical closure problem of turbulence. If we wish, we can change from moments to structure functions, but the KHE remains local in r, the distance between the two measuring points. This formulation gives no hint of a turbulence cascade as it is entirely local in nature.

The situation is radically different in Fourier wavenumber (k) space. Here we have a velocity field $\mathbf{u}(\mathbf{k},t)$ which now satisfies the NSE in k-space. This is still insoluble, and when we derive the Lin equation from it (or by Fourier transformation of the KHE), this again expresses conservation of energy, and is again subject to the closure problem. However, there is a major difference. As pointed out by Batchelor [1], Taylor introduced the Fourier representation in order to turn turbulence into a problem in statistical physics, with the $\mathbf{u}(\mathbf{k},t)$ playing the part of the degrees of freedom. The nonlinear term takes the form of a convolution in wavenumber space and this couples each degree of freedom to every other. In the absence of viscosity, this process leads to equipartition, rather as in an ideal gas. However, the viscous term is symmetry-breaking, with its factor of k^2

skewing its effect to high wavenumbers, so that energy must flow through the modes of the system from low wavenumbers to high. We may complete the picture by injecting energy at low wavenumbers. The result is a physical system which has been discussed in many papers and books and has been studied by theoretical physicists over the decades since the 1950s. In short, Fourier transformation reveals a physical system which is not apparent from the equations of motion in real space.

What, then, do those working in real space mean by the physics of turbulence? Presumably they rely on ideas about vortex motion, as established by flow visualisation; and here the difficulty lies. Richardson put forward the concept of a cascade in terms of `'whirls" (not, incidentally, whorls! [2]); and certainly this has gripped the imagination of generations of workers in the field. In a general, qualitative way it is easy to understand; and one can envisage the transfer of eddying motions from large scales to small scales. But when it comes to a quantitative point of view, the resulting picture is very vague and imprecise. Of course attempts have been made to make it more precise and researchers have considered assemblies of well-defined vortex motions. This is a perfectly reasonable way for fluid dynamicists to go about things, but it involves a considerable element of guess work. In contrast, Fourier wavenumber space gives a precise representation of the physical system and essentially formulates the basic problem as a statistical field theory.

So, spectral methods actually expose the underlying physics of turbulence, rather than obscuring it. It is my view that those who are not comfortable with them must necessarily have a very restricted and limited understanding of the subject. I shall illustrate that in my next post.

[1] G. K. Batchelor. The theory of homogeneous turbulence. Cambridge University Press, Cambridge, 2nd edition, 1971.
[2] W. David McComb. Homogeneous, Isotropic Turbulence:

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

Stirring forces and the turbulence response.

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Vacation post No. I: I will be out of the office until Monday 19 April.

In my previous post, I argued that there seems to be really no justification for regarding the stirring forces that we invoke in isotropic turbulence as mysterious, at least in the context of statistical physics. However, when I was thinking about it, I remembered that Kraichnan had introduced stirring forces in quite a different way from Edwards and it occurred to me that this might be worth looking at again. Edwards had introduced them in order to study stationary turbulence, but in Kraichnan's case they were central to the basic idea for his turbulence theory. In that way, Kraichnan's formulation was more in the spirit of dynamical systems theory, rather than statistical physics.

Following Kraichnan, let us consider the case where the Navier-Stokes equation (NSE) is subject to random force $f_{\alpha}(\mathbf{k},t)$, where the Greek indices take the usual values of $1,\,2,\,3$ corresponding to Cartesian tensor notation. If the force undergoes a fluctuation $$f_{\alpha}(\mathbf{k},t) \rightarrow f_{\alpha}(\mathbf{k},t) +\delta f_{\alpha}(\mathbf{k},t),$$ then we may expect the velocity field to undergo a corresponding fluctuation \[u_{\alpha}(\mathbf{k},t)

$$\rightarrow u_{\alpha}(\mathbf{k},t) +\delta u_{\alpha}(\mathbf{k},t).$$ If the increments are small enough, we may neglect the second order of small quantities, then we may introduce the infinitesimal response function $\hat{R}_{\alpha\beta}(\mathbf{k};t,t')$, such that $$\delta u_{\alpha}(\mathbf{k},t) = \int_{-\infty}^t\,\hat{R}_{\alpha\beta}(\mathbf{k};t,t')\delta f_{\beta}(\mathbf{k},t')\,dt'.$$

Kraichnan linearised the NSE in order to derive a governing equation for the infinitesimal response function. Then he introduced the ensemble-averaged form $$\langle\hat{R}_{\alpha\beta}(\mathbf{k};t,t')\rangle =R_{\alpha\beta}(\mathbf{k};t,t'),$$ where $$R_{\alpha\beta}(\mathbf{k};t,t)=1,$$ in order to make a statistical closure. The result was the Direct Interaction Approximation (DIA) and it is worth noting in passing that its derivation contains the step $\langle uu\hat{R} \rangle = \langle uu \rangle \langle \hat{R}\rangle$, which makes the theory a mean-field approximation.

The failure of DIA was attributed by Kraichnan to the use of an Eulerian coordinate system and he responded by generalising DIA to what he called Lagrangian-history coordinates, leading to a much more complicated formulation. This step inspired others to DIA-type methods in more conventional Lagrangian coordinates. However, the fact remains that the purely Eulerian LET (or local energy transfer) does not fail in the same way as DIA. It is worth noting that unsuccessful theories in Eulerian coordinates are invariably Markovian in wavenumber (this should be distinguished from a Markovian property in time).

An alternative explanation for the failure of Markovian theories is that the basic ansatz, in the steps outlined above, may not identify the correct response for turbulence. In dynamical systems the dissipation occurs where the force acts. In turbulence it occurs at a distance in space and time.

When the force acts to stir the fluid, the energy is transferred to higher wavenumbers by a conservative process, until it comes into detailed balance with the viscous dissipation. Arguably the system response needs to include some further effect, connecting one velocity mode to another, as happens in the LET theory [1].

In all theories, the direct action of the stirring force is both to create the modes and then populate them with energy. In DIA, the way in which energy is put into the modes (i.e. the input term) can be calculated exactly by renormalized perturbation theory in terms of the ensemble-averaged response function . However, the general closure of the statistical equations for the velocity moments is equivalent to an assumption that the same procedure will work for it, which is really only an assumption. So it may be that it is the turbulence response which is mysterious, and not the stirring forces as such.

General treatments of these matters will be found in the books [2,3]. It should be noted that I've used a modern notation for the response function (e.g. see [4]).

[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] D. C. Leslie. Developments in the theory of turbulence. Clarendon Press, Oxford, 1973.
[3] W. D. McComb. The Physics of Fluid Turbulence. Oxford University Press, 1990
[4] W. David McComb. Homogeneous, Isotropic Turbulence: Phenomenology, Renormalization and Statistical Closures. Oxford University Press, 2014.