Theories versus formalisms

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After the catastrophe of quasi-normality, the modern era of turbulence theory began in the late 1950s, with a series of papers by Kraichnan in the Physical Review, culminating in the formal presentation of his direct-interaction approximation (DIA) in JFM in 1959 [1].

The next step was the paper by Wyld [2], which set out a formal treatment of the turbulence problem based on, and very much in the language of, quantum field theory. Wyld carried out a conventional perturbation theory, based on the viscous response of a fluid to a random stirring force. He showed how simple diagrams could be used with combinatorics to generate all the terms in an infinite series for the two-point correlation function. He also showed that terms could be classified by the topological properties of their corresponding diagrams. In this way, he found that one class of terms could be summed exactly and that another could be re-expressed in terms of partially summed series, thus introducing the idea of renormalization. In other words, the exact correlation could be expressed as an expansion in terms of itself and a renormalized response function (or propagator). In a sense, this could be regarded as a general solution of the problem, but obviously one that by itself does not provide a tractable theory. In short, it is a formalism.

As an aside, I should just mention that Wyld's paper was evidently very much written for theoretical physicists. That is no reason why any competent applied mathematician shouldn't follow it, but one suspects that few did. Also, the work has been subject to a degree of criticism: the current version may be found as the *improved Wyld-Lee* theory in #8 of the list of **My Recent Papers** on this website. But this does not affect anything I will say here and I will return to this topic in a future blog.

In contrast, Kraichnan began by introducing the infinitesimal response function $\hat{G}$, which connected an infinitesimal change in the stirring forces to an infinitesimal change in the velocity field. He made this the basis of what he claimed was an unconventional (superior?) perturbation theory, making use of ideas like *weak dependence, maximal randomness*, and *direct interaction*. Unfortunately these ideas did not attract general agreement, and I suspect that he found the refereeing process with JFM, and the subsequent experience of the Marseille Conference (see the previous blog), rather bruising. Apparently he said. `The optimism of British applied mathematicians is unbounded.' Then after a pause. `From below.' I was told this by Sam Edwards when I was a postgraduate student. Sam obviously appreciated the interplay of wit and cynicism.

Now, in completing his theory, Kraichnan made the substitution $\hat{G}= G \equiv \langle \hat{G} \rangle$, which is in effect a mean-field approximation. So it is important to note that, when the conventional perturbation formalism of Wyld is truncated at second-order in the renormalized expansion, the equations of Kraichnan's DIA are recovered. This is important because it suggests that this particular mean-field approximation is in fact justified. However, we know that Kraichnan came to the conclusion that his theory was wrong, at least in terms of its asymptotic behaviour at high Reynolds numbers: see the previous blog.

This has the immediate implication that Wyld's formalism is also wrong, when truncated at second order. Which is also true of the later functional formalism of Martin, Siggia and Rose [3]. Kraichnan came to the conclusion that his DIA approach should be carried out in a mixed Eulerian-Lagrangian coordinate system; and, if correct, that would presumably also apply to the two formalisms. However, there is also the question of whether or not it is appropriate to treat the system response as one would in dynamical system theory. After all, the stirring forces in a fluid, first have to create the system, and only then do they maintain it against the dissipative effects of viscosity. We will return to this aspect in future blogs.

[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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Marseille (1961): a paradoxical outcome.

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When I was first at Edinburgh, in the early 1970s, a number of samizdat-like documents, of entirely mysterious provenance, were being passed around. One that came my way, was a paper by Lumley which contained some rather interesting ideas for treating the problem of turbulent diffusion. I expect that it is still in my filing system; but, with the Covid-19 lockdown, I am cut off from my university office and unable to refresh my memory. Later on I encountered the paper by Proudman which criticised Kraichnan's theory of turbulence – the Direct-Interaction Approximation – and by that time I presumably had heard about the meeting held in Marseille in 1961. Of course my ignorance is not all that surprising, in that the meeting, which was the source of these papers, took place five years before I began my postgraduate research. In any case, I must have known about it by the late 1980s, as these papers are correctly referenced in my 1990 book on the physics of turbulence.

An interesting and informal account of this meeting is given by Moffatt in his review [1], which is essentially an appreciation of the life and work of G. K. Batchelor, and accordingly the meeting is seen, as it were, through this prism. Having told the story of how Batchelor discovered the work of Kolmogorov, while searching through the literature of turbulence in the library of the Cambridge Philosophical Society; and how he had expanded the short and rather cryptic papers of Kolmogorov into what was to become a seminal work on the subject [2], Moffatt sees the Marseille meeting as a 'watershed' in the study of turbulence. In support of this, he highlights two contributions to the meeting.

First, there is the report by Stewart of experimental measurements of energy spectra carried out in the channel between Vancouver Island and the mainland. This investigation achieved values of the Taylor-Reynolds number up to about 3000, and several decades of power-law behaviour, which appeared to support the Kolmogorov $-5/3$ spectrum. This work was published the following year [3].

Secondly, there was a lecture by Kolmogorov, also published in the following year [4], in which he outlined a refinement (*sic*) of his 1941 theory in response to a criticism by Landau. His conclusion was that the power of $-5/3$ should be subject to a small correction μ; but he was unable to obtain a value for μ.
There is an element of contradiction here, but that could possibly be resolved quite trivially if one were to find out that the two agreed within experimental error. So that in itself is not a paradox. The paradox that I have in mind arises in a different way.

Moffatt discusses the fact that Batchelor essentially gave up turbulence as his main research interest after this meeting. His argument appears to be that Batchelor was already becoming discouraged by the difficulties of the subject. And, given that a major part of his own research had been the interpretation and dissemination of the Kolmogorov (1941) theory, he may have found that Kolmogorov's lecture at this meeting came as the last straw!

Another possibility, that Moffatt doesn't mention, is that Batcheleor may have found the new wave of theoretical physics approaches, as initiated by Kraichnan, not only complicated but also part of an alien culture, to the extent that this too was discouraging. I have a personal note that I can add here. I only met Batchelor once; in 1967 when he examined my Master's thesis. At one point he had some difficulty with the units, where I was giving a quantum physics analogy, and I pointed out that there would be a Planck's constant involved, but that I was working in units where Planck's constant was unity. At another stage *he* pointed out that he was, at the risk of being accused of cynicism, no more optimistic about these new quantum-inspired approaches, than about anything else. And, that was with Sam Edwards, who had published a theory of turbulence in JFM three years earlier, also in the room! I am quite sure that forty (or more) years on, there would be many in turbulence research who would eagerly say that he had proved to be right. But, following one's prejudices, rather than engaging with a subject, is the abnegation of scholarship. Sometimes the truth lies deep.

However, another major discouragement took place at this meeting. Kraichnan was predicting an inertial-range spectrum with an exponent of $-3/2$. Even if the results of Grant *et al.* [3] were compatible with a small correction to $5/3$, they were certainly good enough to convincingly rule out Kraichnan's rival $3/2$ exponent. As a result, Kraichnan had to look at his theory again, and over a period of several years he became convinced that the problem was insoluble in Eulerian coordinates, and that there was a need to change to a mixed coordinate system which he called Lagrangian-History coordinates. The result was an immensely complicated theory, which not only had to be abridged in order to permit computation, but also depended on the way in which the theory was formulated. This has left a legacy of other workers who employ a more conventional Lagrangian system.

This, then, is the paradox that I had in mind. The outcome of the meeting, put in very broad brush terms, is that Batchelor changed his mind because Kolmogorov (1941) was wrong and Kraichnan changed his mind because it was correct. It cannot be said that progress in turbulence is ever smooth.

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Which Navier-Stokes equation do you use?

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In the first half of 1999, a major turbulence programme was held at the Isaac Newton Institute in Cambridge. On those days when there were no lectures or seminars during the morning, a large group of us used to meet for coffee and discussions. In my view these discussions were easily the most enjoyable aspect of the programme. On one particular morning, as a prelude to making some point, I said that I was probably unusual in that I have taught the derivation of the Navier-Stokes equation (NSE) as continuum mechanics to engineering students and by statistical mechanics to physicists and mathematicians. The general reaction was that that I was not merely unusual, but surely unique! I gathered, from comments made, that everyone present saw the NSE as part of continuum mechanics.

Of course the two forms of NSE are apparently identical, otherwise one could not refer to both as the Navier-Stokes equation. Nevertheless, when one comes to consider the infinite Reynolds number limit, it is necessary to become rather more particular. We can start doing this by stating the two forms, as follows.

First, the continuum-mechanical NSE is exact for a continuous fluid which shows Newtonian behaviour under all circumstances of interest.

Secondly, the statistical-mechanical NSE is the *first approximation* to the *exact* statistical mechanical equations of motion. So in principal it should be followed by a statement to the effect that there are higher-order terms.

Now strictly, if we want to consider cases where the continuum approximation breaks down, we should be using the second of these forms. Batchelor argued that in the limit of zero viscosity (at constant dissipation rate) the dissipation would be concentrated at infinity in wavenumber space. Edwards [1] went further and represented this dissipation by a delta-function at $k=\infty$ and matched it with a delta-function input of energy at $k=0$. In this way he could obtain an infinitely long inertial range and assume that the $-5/3$ spectrum applied everywhere, as a test of his closure approximation.

The Edwards procedure is valid, because he was applying it to a closure of the (in effect) continuum-mechanical NSE, as indeed is everyone else who discusses behaviour at large Reynolds numbers; or, for that matter, statistical closures. But the question of the validity of this model arises when people consider the breakdown of the NSE. This actually requires some consideration of the basic physics, which in this case means statistical mechanics; and, essentially this boils down to the following: The general requirement for the continuum limit to be valid is that the smallest length-scale of the fluid motion should be much larger than the mean free path of the fluid's molecules.

The only example of this being looked at quantitatively, that I know of, may be found in Section 1.3 of the book by Leslie [2]. He considered flow in a pipe at a Reynolds number of 10^6, with a pipe diameter of $10mm =10^{-2}m$, which he described as an extreme case. In Section 2.8 of his book, he calculates the minimum eddy size to be greater than $10^{-4}mm =10^{-7}m$. He notes that for a liquid the mean free path is of the order of the atomic dimensions and thus about $10^{-10}m$ and hence the use of a continuum form is very well justified. He further comments: 'It [the continuum limit] is also satisfied, although not by such a comfortable margin, by any gas dense enough to produce a Reynolds number of 10^6 in a passage only $10mm$ in diameter.'

I think that it would be a good idea if those who discuss cases where a theory based on the Navier-Stoke equation is supposed to break down actually put in some numbers to indicate where their revised theory would be applicable and the NSE wouldn't. Or perhaps, it might be salutary to consider in detail the variation of significant quantities with increasing Reynolds number and identify the smooth development of asymptotic behaviour. I will return to this point in future posts.

Anyone who would like an introductory discussion of the derivation of macroscopic balance equations from statistical mechanics should consult Section 7.6 of my book *Study notes for statistical physics*, which may be downloaded free of charge from Bookboon.com.

[1] S. F. Edwards. Turbulence in hydrodynamics and plasma physics. In Proc. Int. Conf. on Plasma Physics, Trieste, page 595. IAEA, 1965.
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Turbulence as a quantum field theory: 2

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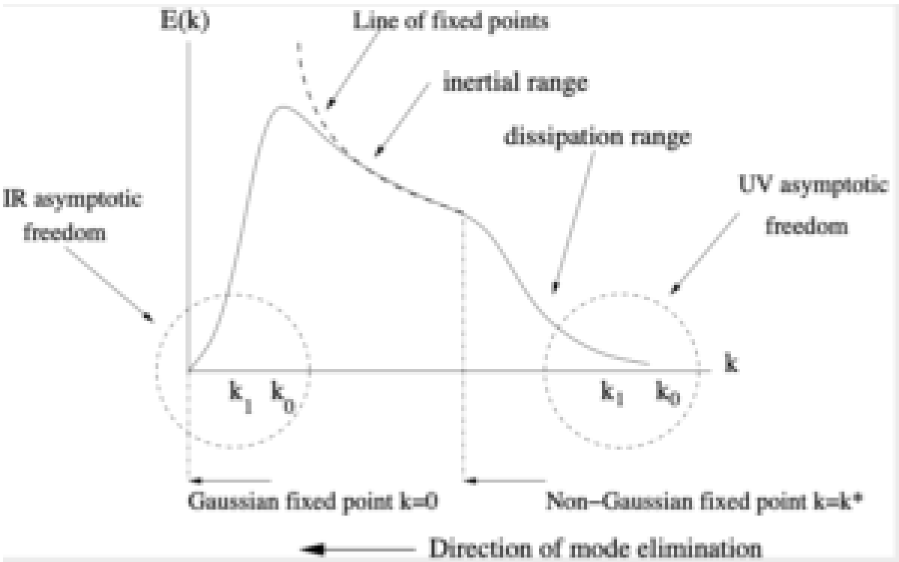
In the previous post, we specified the problem of stationary, isotropic turbulence, and discussed the nature of turbulence phenomenology, insofar as it is relevant to taking our first steps in a field-theoretic approach. Now we will extend that specification in order to allow us to concentrate on renormalization group or RG.

RG originated in quantum field theory in the 1950s, but is best known for its successes in critical phenomena in the 1970s, along with the creation of the new subject of *statistical field theory*. Essentially it began as a method of exploiting *scale invariance*, and ended up as a method of detecting it, and also establishing the conditions under which it would hold. It is most easily understood in the theory of ferromagnetism, where we can envisage a model consisting of lots of little atomic magnets on a lattice. These atomic magnets (or lattice spins) interact with each other and, if we call the interaction energy for any pair J, this energy appears in the partition function as $J/k_B T$, where k_B is the Boltzmann constant, and T is the absolute temperature. This quantity is the coupling constant.

Now RG consists of coarse-graining our microscopic description, and then re-scaling it, to see it we can get back to where we started. If so, that would be a *fixed point*. In practice, we might expect to carry out this transformation a number of times, in order to reach such a fixed point. So in effect we are progressively reducing the number of degrees of freedom. This involves some sort of partial average at each step, in contrast to a full ensemble average, which gets you down from lots of degrees of freedom to just a few numbers being needed to describe a system.

Actually, merely by waving our hands about, we can deduce something about the fixed points of our lattice model of a ferromagnet. If we consider very high temperatures, then the coupling strength will be reduced to zero. The lattice spins will have a Gaussian probability distribution. We can envisage that this will be a fixed point, as no amount of coarse-graining will change it from a purely random distribution. At the other extreme, as the temperature tends to zero, the coupling tends to infinity and there can be no random behaviour: the spins will all line up. Once again, perfect order cannot be changed by coarse graining, and this also is a fixed point. What happens in between these extremes is interesting. As the temperature is reduced from some very large value, clumps of aligned spins will occur as fluctuations. The size of these fluctuations is characterised by the *correlation length*. As the temperature approaches some critical value T_c from above, the correlation length will tend to infinity. When this occurs, it is no longer possible to coarse-grain away the ordering, as it exists on all scales. This fixed point is the *critical point* of the lattice.

So, RG applied to the model identifies the high- and low-temperature fixed points, which are trivial; and the critical fixed point which corresponds to the onset of ferromagnetism. This is known as *real space RG* and I have given a fuller account (with pictures!) elsewhere [1]. For completeness, I should mention that the momentum-space analytical treatment involves Gaussian perturbation theory in order to evaluate parameters associated with the critical point. Also, the temperature in this context is known as a *control parameter*.



Variation of the coupling strength with wavenumber in isotropic turbulence.

In turbulence, the degrees of freedom are the independently excited Fourier modes. The coupling parameter for each mode can be identified with Batchelor's Reynolds number (see my earlier post on 23/04/20) which takes the form $R(k)=[E(k)]^{1/2}/\nu k^{1/2}$. Using the schematic energy spectrum, as given in the preceding post, we can identify the trivial fixed points where the coupling falls to zero. This is because the spectrum is known to go to zero at least as k^4 as $k\rightarrow 0$ and to zero exponentially as $k\rightarrow \infty$. By analogy with quantum field theory, we refer to these points as being asymptotically free in the infra-red and the ultra-violet, respectively. In order to compare with magnetism, we can argue that the $k=0$ fixed point is analogous with the high-temperature, where the low-k motion is random (Gaussian) due to the stirring, whereas at large k, the motion is damped by viscosity and is analogous to the low-temperature fixed point. In the figure we identify another possible, but non-trivial, fixed point where the inertial range is represented by the Kolmogorov $k^{-5/3}$ spectrum. A power law, being scale-free, is likely to be associated with a fixed point of the RG transformations.

In order to carry out calculations, we seek to eliminate modes progressively in bands, first $k_1\leq k\leq k_0$, then $k_2 \leq k \leq k_1$, and so on. At the first stage, the effect of the missing modes results in an increase to the viscosity $\nu_0 \rightarrow \nu_1 = \nu_0 + \delta \nu_0$. We then rescale on the increased viscosity, and repeat the process. Note that we rename the molecular viscosity $\nu = \nu_0$ for this purpose. Also note that it can be a little counter-intuitive associating zero with the maximum value of k, but we want an increasing index as we reduce k, leading on to a recurrence relationship which may reach a fixed point.

In the theory of magnetism, the lattice spacing a is used to define the maximum wavenumber, thus $k_{max} = 2\pi/a$. In turbulence, sometimes the Kolmogorov wavenumber is used for the maximum, but this is likely to be incorrect by at least an order of magnitude. A better definition has been given [2] in terms of the dissipation integral, thus: $\varepsilon = \int_0^\infty 2\nu_0 k^2 E(k) dk \simeq \int_0^{k_{max}}2\nu_0 k^2 E(k) dk$.

I shall highlight two calculations here. Forster *et al* [3] carried out an RG calculation by restricting the wavenumbers considered to a region near the origin. This was very much a Gaussian perturbation theory of the type used in the study of critical phenomena. They did not refer to this as turbulence, and instead considered it as the large scale asymptotic behaviour of randomly stirred fluid motion.

Later, McComb and Watt [4], introduced a form of conditional average which allowed the RG transformation to be formulated as an approximation, valid even at large wavenumbers. They were able to find a non-trivial fixed point which corresponded to the onset of the inertial (power-law) range and gave a good value of the Kolmogorov spectral constant. This work has been carried on and refined but is very largely ignored. In contrast, Forster *et al* seem to have established a new paradigm of Gaussian fluid motion which permits the application of much field theoretic RG which relies on the simplifications of the paradigm. There is, however, one difference. Nowadays people publishing in this field describe it as turbulence! The most up-to-date treatment of the conditional averaging method will be found in [5].

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