The energy balance equation: or what's in a name?

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Over the last few years I have noticed that the Karman-Howarth equation is sometimes referred to nowadays as the `scale-by-scale energy budget equation'. Having thought about it carefully, I have concluded that I understand that description; but I think the mere fact that one has to think carefully is a disadvantage. To Anglophone speakers of English, the term `budget' suggests some sort of forward planning. Actually I think that in physics the more correct term would be *local energy balance equation.* Let us consider the form of the KHE equation when it is written in terms of the second-order and third-order structure functions, thus:

\[0=-\frac{2}{3}\frac{\dd E}{\dd t} + \frac{1}{2}\frac{\dd S_2}{\dd t} + \frac{1}{6r^4}\frac{\dd}{\dd r}(r^4 S_3) – \frac{\nu}{r^4}\frac{\dd}{\dd r}\left(r^4\frac{\dd S_2}{\dd r}\right). \]

Note that all notation and background for this post will be found in my (2014) book on HIT. Also, I have moved the term involving the total energy (per unit mass) to the right of the equal sign, for a reason which will become obvious.

More recently I have seen exactly the same phrase used to describe the Lin equation, which is just the Fourier transform of the KHE to wavenumber space. This strikes me as even more surprising, but again I don't want to say that it is actually wrong. Indeed in one sense I rather welcome it, because it makes it clear that the concept of scale belongs equally to wavenumber space. It can be all too easy to fall into a usage in which real space is regarded as `scale space' and is distinguished in that way from wavenumber space. But the real problem here is that it is only valid for the simplest form of the Lin equation, and this in itself can be misleading.

Let us now consider the Lin equation in terms of the energy spectrum and the transfer spectrum. We may write this in its well-known form:

$$\left(\ddt + 2\nu k^2\right)E(k,t) = T(k,t).$$

Here, as with the KHE, we assume that there are no forces acting.

However, unlike the KHE, this is not the whole story. We may also express the transfer spectrum in terms of its spectral density, thus:

$$T(k,t) = \int_0^\infty\, dj \,S(k,j;t).$$

When we substitute this in, we obtain the second form of the Lin equation, and this is actually more comparable with the KHE as given above, because the transfer spectrum density contains the Fourier transform of the third-order structure function, which of course occurs explicitly in the KHE.

Now compare the two equations. The KHE holds for any value of the independent variable. If we take some particular value of the independent variable, then each term can be evaluated as a number corresponding to that value of r, and the above equation becomes a set of four numbers adding up to zero. If we consider another value of r, then we have a different four numbers but they must still add up to zero. In short, KHE is *local* in the independent variable.

The Lin equation, if we write it in its full form, tells us that all the Fourier modes are coupled to each other. It is, in the language of physics, an example of the *many body* *problem.* It is in fact highly non-local as in principle it couples every mode to every other mode.

A corollary of this is that the KHE does not predict a cascade. But the Lin equation does. This can be deduced from the nonlinear term which couples all modes together plus the presence of the viscous term which is symmetry-breaking. If the viscous term were set equal to zero, then the coupled but inviscid equation would yield equipartition states.

The well-known question at the head of this post is rhetorical and expects the answer `A rose by any other name would smell as sweet'. But I'm afraid that Juliet's *laissez-faire* attitude to terminology would not be widely applicable. One thinks of the surgeon who fails to distinguish between the liver and the spleen. Or the pilot who thinks west is just as good a name for east. In the turbulence community, I suppose that `locality' for `localness', or `inverse' for `reverse' arise because they seem natural coinages to non-Anglophones. In the wider world, the classic case since the 1960s is Karl Popper's idea that a scientific theory should be *falsifiable.* But in everyday English speech, to falsify means to make false. For instance, to falsify an entry in one's accounts, means, to put it in the demotic, to cook the books!

I shall return to this point in future posts and in particular to the localness of the KHE.

Wavenumber Murder and other grisly tales

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When I was first at Edinburgh, I worked on developing a theory of turbulent drag reduction by additives. But, instead of considering polymers, I studied the much less well-known phenomenon involving macroscopic fibres. This was because it seemed to me that the fibres were probably of a length which was comparable to the size of the smallest turbulent eddies. It also seemed to me that the interaction between fibre and eddy would be two-dimensional and that it might be possible to formulate an explanation of turbulent drag reduction on mainly geometrical grounds. In particular, I had in mind that two-dimensional eddies could have a reverse cascade, with the energy being transferred from high wavenumbers to small. That is, the reverse (but *not* inverse) of the usual process. In this way drag might be reduced.

I derived a simple model for this process, and a letter describing it was published by *Nature Physical Science* in 1974. So far so good. Then I set to work writing the theory up in more detail and submitted it to the *JFM*. The results were not so good this time, and I had three referees' reports to consider. At least, George Batchelor did not feel the need to suppress any of the reports on the grounds of it being too offensive (someone I knew actually had this exeperience). But still, they were pretty bad.

No doubt this was salutary. I didn't dissent from the view that the paper should be rejected. In fact I dismantled it into several much better papers and got them published elsewhere. But what sticks in my mind even yet is the referee who wrote: *`The author commits the usual wavenumber murder. Who knows what unphysical assumptions are being made under the cover of wavenumber space?'*

Well, that's for me to know and you to find out, perhaps! Of course, now that I am older (a lot) and wiser (a little), I realise that I could have played it better. I could have written up the use of Fourier methods, quoted Batchelor’s book extensively, and thus made it very difficult for the referee to respond in that rather childish way. But why would that even occur to me? I was used at that stage to turbulence theorists who moved straight into wavenumber space without seeing any need to justify it. This is a cultural factor. Theoretical physicists are used to operating in momentum space which, give or take Planck’s constant, is just wavenumber space in disguise. Anyway, at the time I was surprised and disappointed that the editor did not at least intervene on this particular point.

I actually found that referee’s reaction quite shocking, but in one form or another I was to encounter it occasionally over the years, until at last it seemed to die out. Partially this could be attributed I would guess to the growth of DNS, with its dependence on spectral methods. Also, I think it could be due to better educated individuals becoming attracted to the study of turbulence.

Anyway, a few years ago, and just when I thought it was safe to mention spectral methods again, I made a big mistake. I had written (with three co-authors) a paper in which we used spectral methods to evaluate the exponents associated with real-space structure functions. It had been increasingly believed that the inertial-range exponents departed from the Kolmogorov (1941) forms, increasingly with both order and with Reynolds number, although it was actually realised that this could be attributed to systematic experimental error. So we had used a standard method of experimental physics to reduce systematic error and found that the exponent for the second-order structure function in fact tended to the Kolmogorov canonical form, as the Reynolds number was increased. This is precisely the sort of result that merits a short communication and accordingly we submitted it as such. One of the referees was contumacious (and I may come back to him in later blog), the other was broadly favourable but seemed rather nervous about various points. However, when we had responded to his various points, he wanted one or two more changes and then he would recommend it for publication. At the same time, he commented that he really did wish that we hadn’t used spectral methods.

This was where I made my big mistake. Overcome by kindly feelings towards this ref, and obeying my pedagogical instincts, I tried to re-assure him. I pointed out that he was quite happy with the pseudo-spectral method of DNS, in which the convolution sums in wavenumber space are evaluated more economically in real space and then transformed back into wavenumber. Now, I said, we are employing the same technique, but the other way round. We are evaluating the convolutions determining the structure function in real space, by going into wavenumber space. The response had a petulant tone. We were, he said, talking nonsense. The structure functions did not involve convolution integrals and he was rejecting the paper as mathematically unsound!

Later on we wrote up a longer version of the work and it was published: see #7 in the list of recent papers on this site. Appendix A is the place to look for the maths which bewildered the poor benighted referee. While accepting that this degree of detail was not given in the short communication, what is one to make of a referee who is unaware that a structure function can be expressed in terms of a correlation function and that the latter is a convolution integral?

Both referees were frightened of Fourier methods and between them almost seem to have bookended my career. But referees who are comprehensively out of their depth have not been a rare phenomenon over the years. The forms which this inadequacy takes have been many and varied and I shall probably be dipping into my extensive rogues’ gallery in future posts. There is also the question of the editor’s role in finding referees who are actually qualified to referee a specific manuscript, and this too seems a fit subject for further enquiry. However, I should finish by pointing out that being on the receiving end of inadequate refereeing is not exclusively my problem.

In the first half of 1999, the Isaac Newton Institute held a workshop on turbulence. During the opening week, we saw famous name after famous name go up to the podium to give a talk, which almost invariably ended with `*and so I sent it off to Physica D instead*'. This last was received with understanding nods and smiles by an audience who were clearly familiar with the idea. This quite cheered me up, it seemed that I was not alone. At the same time, the sheer waste of time and energy involved seemed quite shocking. It prompted the thought: is it the turbulence community that is the problem, rather than the turbulence? That is something to consider further in future posts.

HIT: Do three-letter acronyms always win out?

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In 1997, I visited Delft Technical University and while I was there gave a course of lectures on turbulence theory. During these lectures, I mentioned that nowadays people seemed to refer to homogeneous, isotropic turbulence; whereas, when I started out, it was commonplace to simply say isotropic turbulence. The homogeneity was assumed, as a necessary condition for the isotropy. After the morning session, when we were making our way back for lunch, the postgrads who were attending, said to me `Three-letter acronyms always win out!'. Naturally, I pooh-poohed this, but many years on, I have to confess that I use the three-word name of the subject (it was the title of my 2014 book) and the acronym as well. Sometimes it is just a matter of euphony. But does it do any harm? Well, that's an interesting question, but for the moment let us make a short digression.

In recent years I have been thinking a little about cosmology (well it makes a change from turbulence) and have learned about the *cosmological principle,* which states that the universe is both homogeneous and isotropic.Homogeneous means that its properties are independent of position and isotropic means that its properties are independent of orientation. In everyday life, one might think of a piece of metal or plastic being homogeneous and isotropic, in contrast to wood which has a grain. So naturally when I step out into my back garden in the evening, I can observe this for myself … or rather, I can't. Actually the night sky looks anything but homogeneous, let alone isotropic. Are the cosmologists deluded?

The answer lies in the fact that the cosmological principle applies to *averaged* properties. Apparently it is necessary to take averages over huge volumes of space, each of which contains vast numbers of galaxies, for the concepts of homogeneity and isotropic to apply. Evidently, to paraphrase J. B. S. Haldane (and following in the footsteps of Werner Heisenberg) the universe is not only bigger than we think, it is bigger than we *can* think. So, if I want to behave like an idiot, I should just go about proclaiming: `The cosmologists are mad. You only have to look up at the night sky to see that their claims about the uniformity of the universe are completely unjustified.' In doing so, I would be ignoring the details of what the cosmologists actually said, and surely no one would be so silly as to do that before launching into speech? Well, in turbulence that is exactly what many people do.

In turbulence, for many years we have had flow visualisations based on direct numerical simulation of the equations of fluid motion. These undoubtedly show a spotty distribution of various characteristics of interest, especially the dissipation rate, and this is generally taken as supporting the idea that turbulence intermittency has implications for statistical theories. Indeed, there are those who go further and see results like this as invalidating assumptions of homogeneity and isotropy. What they leave out of the reckoning is; first, *that homogeneity and isotropy are properties of average quantities, in turbulence as in cosmology*. Secondly, the flow visualisations are snapshots or single realisations. If you average over them, the spottiness disappears, as indeed it has to, in order to conform to homogeneity and isotropy, and the field becomes uniform and without structure.

If we go to the fountainhead for this subject, in Batchelor's classic monograph on page 3 we may read: *`The possibility of this further assumption of isotropy exists only when the turbulence is already homogeneous, for certain directions would be preferred by a lack of homogeneity'*. Batchelor also points out that homogeneity and isotropy are average properties of the random variable, and in fact they are defined formally in terms of the probability distribution functional (the pdf, or equivalently its moments).

So this is where I answer my own question. It does matter. It is needed for clear thinking and the best possible understanding that we are careful about the fact that homogeneity is a necessary condition for isotropy. In the process we have to be careful about definitions. In that way one can perhaps avoid the egregious errors which occur in a recent paper, where it is argued that intermittency at the small scales is incompatible with homogeneity and so invalidates the energy-balance equation derived rigorously by averaging the equations of motion. Actually, intermittency is present at all scales and is part of the exact solution of the equations of motion. It is not in any way incompatible with the pdf, which must take a form appropriate to the intermittent (single-realization characteristic) and homogeneous (ensemble-averaged characteristic) nature of the random field. We shall return to a more specific way to this publication in later posts.

The First Post

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Many years ago, early in my career, I learned the hard way that every paper submitted for publication should be ruthlessly pared down to consist solely of factual material and fully justified statements. Any personal opinions, speculations, whimsical thoughts, comments or suchlike, should be eliminated; as, in the words of the poet John Donne, they would offer `hostages to fortune'. That is, there would be at least one referee who would make such an opinion (suitably misinterpreted!) the basis for outright rejection of the manuscript, probably accompanied by gratuitously offensive comments. This of course raises questions about the role of the editor in this increasingly fraught process of peer review, and that is something to which I shall return in future blogs.

In the middle period of my career, I would occasionally receive a referee's report which expressed regret that I had not included more of my own views, and indicated that they would be welcome. My response to this was `No fear', to use an expression from my remote childhood.

Recently I gave in to the temptation to do just that and, in what might well be my last journal submission (rejected by four different journals), I sweepingly dismissed both the Kolmogorov (1962) `revised theory’ and Landau’s criticism of the Kolmogorov (1941) theory, without explaining why. I suppose I was relying on the critique published in my book of 2014. But they were seized upon by one referee to reject the paper, followed by the patronizing comment `Need I say more’. Well, actually what he needed to do was to say less and to think more. That too is something to which I shall return in future blogs.

Evidently my self-imposed constraints are beginning to chafe! So, as a blog (if it is to be of any value as offering clarification or stimulus) should in fact consist very largely of the things that I have omitted from papers, the temptation to blog is clear. As I began my postgraduate research in 1966, I am now in my forty fifth year of turbulence research, so there should be no lack of material. Oh, and it should also be both pithy and hard-hitting. You have been warned.