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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.