Peer review: some further thoughts.

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Vacation post No 4. I will be out of the virtual office until Monday 31 August.
Peer review continues to cause concern, with widespread perceptions of unfairness. Although most of what I have noticed recently seems to be in the medical/public health communities, where one major gripe appears to be that established researchers have a significantly better chance of getting published. The current favourite response to this is to introduce double-blind refereeing, where you don't know who your referee is and they don't know who you are. Well, I can't see that working in turbulence and I doubt if there is any way that I could conceal my identity. In fact, that goes for anyone who publishes regularly in a field which does not have a lot of participants. So, in STEM subjects in general, that looks like a non-starter.

In any case, why shouldn't a researcher with a good track record of publication in their subject have a better chance of being published? Indeed, I would go further. I think that it should be part of the `rules of the game' that there should be a presumption that a further publication on a topic should be published unless it is wrong in some way, or misleading, or quite definitely does not add anything to previous publications by that particular author. In other words, there should be an onus on the referee to demonstrate such faults.

I would actually go further and argue that, rather than introducing an additional layer of anonymity, we should remove the existing one. In my view, it would be helpful if referees had to put their name to their report. It should improve both fairness and (sometimes) courtesy. I should make it clear that I apply that opinion to everyone who referees and do not exclude myself!

Naturally there will be those who will respond that if we remove anonymous refereeing, then the sky will fall in. I don’t see why this should be. In my early years at Edinburgh, I did some work on turbulent diffusion in aerosol jets and this was published in the Journal of Aerosol Science. Their policy, at least at that time, was to have one referee who was expected to engage constructively with a submission and then to sign their report. My memory of it (rather vague now) was that it was a civilised and effective process. I also remember that the late Bob Kraichnan signed his referee reports and that was my experience on the few occasions that he refereed anything of mine.

And what about me? Well, I have dropped my anonymity on a number of occasions over the years, but only where I felt that it was particularly appropriate, for instance when my own work was being criticised. Apart from that, I have just been part of the flock! However, I seriously believe that the nature of refereeing in turbulence demands reform. My PhD supervisor described it as `cut-throat’ and at times it would be hard to disagree. Partly I think that this is due to the heterogeneous nature of the turbulence community, so that very often people are refereeing work that they are simply not able to understand.

I have yet further thoughts on this subject, which will be the subject of further posts. At the moment I am looking forward to a month’s holiday from turbulence, so this is being written on the 30 July in order to be posted on the 27 August. On the 31 August I shall begin reading my email again.

Is there any place for personal taste in science?

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Vacation post No 3. I will be out of the virtual office until Monday 31 August.

It has long been the case that physicists talk approvingly about a physical theory as being `elegant' or even `beautiful'. Like so much else, this seems to have become commonplace in the 1960s. More recently I have become aware of similar sentiments being expressed in mathematics. In that case one can see that some particular proof, say, might be preferred to another, purely on grounds of economy or clarity or conciseness. However, in the case of physics, one might expect that a comparison of a theory's predictions with experimental results should be the deciding factor.

There is an old adage in engineering design to the effect that `if it looks right, then it is right'. Obviously, there are constraints on this in that your design for a motor car must look as if it is capable of being a motor car. This latter point is an instance of the precept `form follows function' which originated in architectural design in the early part of the last century. But the adage refers to quality, and is supposedly a way of separating a good design from other designs that are merely adequate. So the implication is that a purely aesthetic judgement can lead to a design that satisfies various, perhaps quantitative, criteria which give a universal meaning to the term `good design' in some particular context.

Of course the insertion of the word `probably' into the engineering adage might lead to its justification in practice. That is, if it looks right then it `probably' is right. So the adage could offer a guide as to whether or not one should take a particular design idea further. For this to work there must exist some consensus on what is meant by `looks right'. And this undoubtedly changes with time. A motor car which was at the leading edge of design in the 1960s will look distinctly old-fashioned nowadays.

But there is always some unease about using a personal value judgement to determine a matter which will ultimately be settled on a quantitative basis. And there are other complications too, even when the quantitative aspect is not present, as for example in the arts. An awful warning may be found in the well known crisis in painting at the end of the nineteenth century. This was triggered by the invention of photography, which in turn led to artists becoming experimental in order to avoid producing paintings which were no more than (in effect) photographs. Such attempts were reviled and even the formation of schools of activity (e.g. Fauves, impressionists) did not at first lead to acceptance.

Unfortunately the fact that impressionist paintings are now highly valued appears to have led to the pendulum swinging too far in the other direction of uncritical acceptance. Even so, those who are specialists in the world of art, literature or music can argue that their `informed' eye or ear gives their opinion a special weight. And no doubt that is a tempting argument in science too. Indeed, in the case of string theory or the idea of the multiverse, where testing against experiment is impossible, it is arguable that aesthetic criteria may be all that one has. But, if consensus develops, this can then lead to the creation of schools of opinion and standard models, which in turn can have the perverse effect of shutting down other approaches to the problem. This is not the case in the arts. Indeed, the non-specialist can say `I know what I like', and there is an end to it. One does not have that freedom in science. Or at least, not if one expects to get published in the learned journals.

Therefore, it does seem that there are dangers from importing purely
personal aesthetic considerations into science. It is interesting to
note that the greatest physicist of all had some words to say on this
particular subject. In the preface to his 1916 book, entitled
`Relativity', Einstein stated that he had followed the precepts of that
other great theoretical physicist, Boltzmann, `… according to whom,
matters of elegance ought to be left to the tailor and to the cobbler'.

My list of jobs to do from 17 November 2009.

My list of jobs to do from 17 November 2009.
Vacation post No 2. I will be out of the virtual office until Monday 31 August.
Recently I was tidying up some papers and I came across this list from 2009. At that time I had just entered my fourth year of retirement (now in my fourteenth!) and these were the things I wanted to do. Actually other jobs took priority and none of the following list was ever done!

1. LET: evaluate the Kolmogorov pre-factor as a function of Reynolds number. Does it asymptote?
2. DNS: `Kolmogorov exponent' as a function of Reynolds number. (In fact the inverted commas were because this was shorthand for measure the power-law exponent for the inertial range of wavenumbers and see if it asymptotes to -5/3. I would

also add the pre-factor to this, as in the LET case above.)
3. Calculate LET with the de facto vertex renormalization of omitting modes from the convolution sum: test for universality of the cut-off wavenumber ratios. (Method due to Kadomtsev: see Leslie’s book.)
4. Do the same with DNS.
5. Make a systematic examination of the dependence on initial conditions for both DNS and LET.
6. Use DNS to investigate the vorticity transfer corresponding to the filtered, partitioned energy transfers T^{--}, T^{-+}, T^{+-}, and T^{++}.
7. Use stirring forces which are not `white noise’ to test effect of initial conditions.

Some of these ideas were prompted by the fact that I was studying the variation of the dimensionless dissipation as a function of Reynolds numbers at the time. This only required quite small Reynolds numbers and it was easy to map out the dependence. Our first paper reporting this work was rejected by one of the referees because he had a simulation which could go to much bigger Re, and so our work couldn’t be any good. Fortunately this idiosyncratic view did not prevail.

Seriously, though, I think that the turbulence community as a whole has been influenced by the need to get to large Re in order to resolve questions about universal behaviour, and it is perhaps time to build up a better understanding of the basic physics of turbulence by looking at the low-Re behaviour. Point 6 is relevant to large-eddy simulation, renormalization group and the scale-invariance paradox.

Are there any bright young people out there with access to a code and a computer who would like to take on any of these things? If so, just get in touch and I’ll be happy to advise you.

Can mathematicians solve problems in physics?

Can mathematicians solve problems in physics?
Vacation post No 1. I will be out of the virtual office until Monday 31 August.
When I used to lecture final-year undergraduates in mathematical physics, there were often quite a few mathematicians attending and I would sometimes tease them by pointing out that mathematicians try to prove the ergodic theorem whereas physicists don’t need to. We know it must be true! This was always taken in good part, but it wasn’t really a joke, because I believe it to be literally true. Progress in physics from earliest times has proceeded from experimental observation, which is then codified in mathematical theory. When a new observation arises and does not agree with the existing theory, then so much the worse for the theory. We have to devise a new and better one. (I believe the Hegelian position is the exact opposite of this: so much the worse for the observation!)

The only exception to this that I know of is the work of the great Paul Dirac, who actually started his working life as an electrical engineer and only later qualified in mathematics. He tackled the problem of deducing a relativistic form of the Schrödinger by purely mathematical methods and ended up predicting the existence of antimatter. Nice one Paul!

If one is going to have an exception, what an exception to have. The only thing that I can think of which might be comparable, is the work of Emmy Noether. Her theorem that continuous symmetry of a physical quantity implies its conservation underpins the whole of fundamental theoretical physics. And of course much mathematical work has gone into the development of modern formulations from the original observation-based forms, such as Newton’s laws of motion. However, I don’t know enough about Noether’s theorem to be sure about whether or not it also represents a significant exception. I still intend to rectify this, although I have been intending to do so, for many years.

As regards the relevance of my original question to turbulence, I can come up with a specific example in a related field. A few years before I retired, I had some discussions with a mathematician about problems in soft (condensed) matter. This arose in a social way, in that one of my colleagues had attended a party in the maths department and got talking to a young mathematician who bemoaned the fact that he had no one to discuss his work with. My colleague knew that I had published something in this area [1] and suggested that we make contact. As a result we had a number of discussions (and some games of badminton!) and it was clear that we were poles apart in the way we looked at things. Nevertheless, one specific point emerged. He had reservations about the (at that time) famous KPZ equation for nonlinear deposition. On purely mathematical grounds (something to do with simultaneously working with generalized functions and Fourier transforms, I think) he had concluded that the KPZ equation was mathematically unsound and needed a counter-term to be added to deal with this. Accordingly he was quite surprised to find that my co-author and I had already come to this conclusion on purely physical grounds and that we had identified the requisite term to be added [1].

It seems to me that modern theoretical physics is dominated by this sort of pure mathematical approach which may in fact be sterile without a new physical hypothesis of the kind that physicists can actually understand to be such. In the rather humbler discipline of turbulence theory, I note many papers which seem to be predicated on the assumption that one must take account of singularities. I believe this activity may actually be harmful, as well as unnecessary, because it makes people unsure about things. For example, when a referee insists that I qualify some statement about taking a limit or making an expansion, with the phrase `provided that no singularity occurs' I feel that I am being forced to make use of the mathematician's comfort blanket. Frankly, I would rather rely on the physicist's comfort blanket, which is based on the interlocking physical picture which in turn is based primarily on observation. Just bear it in mind: we physicists know that the ergodic theorem holds.

[1] W. D. McComb and R. V. R. Pandya. Hidden symmetry in a conservative equation for nonlinear growth. J. Phys. A: Math. Gen., 29:L629, 1996.