When is a conjecture not a conjecture?

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Staycation post No 1. I will be out of the virtual office until 30 August.

That sounds like the sort of riddle I used to hear in childhood. For instance, when is a door not a door? The answer was: when it's ajar! [1] Well, at least we all know what a door is, so let us begin with what a conjecture actually is.

According to my dictionary, a conjecture is simply a guess. But in mathematics it is somehow more than that. Essentially, the idea is that a mathematician can be guided by their experience to postulate that something he/she knows to be true under particular circumstances is in fact true under all possible or relevant circumstances. If they can prove it, then their conjecture becomes a theorem.

The question then arises: what is a conjecture in physics? And if you can demonstrate its truth by measurement or reasoned argument, does it become a theory?

Let us take as an example a system such as an electrolyte or plasma containing many charged particles. The particles interact pairwise through the Coulomb potential and as the Coulomb potential is long-range this presents a many-body problem. What happens in practice is that a form of renormalization takes place, and the Coulomb potential due to any one electron is replaced by a potential which falls off more rapidly due to the screening effect of the cloud of particles surrounding it. A very simple introduction to this idea (which is known as the Debye-Huckel theory) can be found in Section 1.2.1 of the book cited as reference [2] below.

If we take the case of the turbulence cascade, the Fourier wavenumber modes provide the degrees of freedom. Then, instead of pairwise interactions, we have the famous triad interactions, each and every one of which conserves energy. If for simplicity we consider a periodic box, then the *mean* flux of energy from low wavenumbers to high can be written as the sum of all the individual mean triadic interactions. As in principle all modes are coupled, this is also a many-body problem and one can expect some form of renormalization to take place. In some simple circumstances this can be interpreted as a renormalized viscosity (*the effective viscosity*) which is very much larger than the molecular viscosity. These ideas date back to the late 19th century and are the earliest example of renormalization (although they did not use this term which came much later on, around the mid-20th century).

Now let us consider what happens as we progressively increase the Reynolds number. For the utmost simplicity we will restrict our attention to forced, stationary isotropic turbulence. Then, if we hold the rate of energy input into the system constant and decrease the viscosity progressively, this increases the Reynolds number at constant dissipation rate. It also increases the size of the largest wavenumbers of the system. The result is a form of scale-invariance in which the flux through wavenumbers is independent of wavenumber and the result is *the dissipation law* that the scaled dissipation law is independent of the viscosity as a rigorous asymptotic result [3]. It should perhaps be emphasised that this asymptotic behaviour is the infinite Reynolds number limit; but, from a practical point of view, we find that subsequent variation becomes too small to detect at Taylor-Reynolds numbers of a few hundred and thereafter may be treated as constant. We will return to this point in the next post, along with an illustration.

Meanwhile, back in real space, velocity gradients are becoming steeper as the Reynolds number increases, and this aspect disturbed Onsager [4] (see also the review of this paper in the context of Onsager's life and work [5]). In fact he concluded that the infinite Reynolds number limit was the same as setting the viscosity equal to zero. In his view, the resulting Euler's equation could still account for the dissipation in terms of singular behaviour. But, it has to be said that, in the absence of viscosity, there is no transfer of macroscopic kinetic energy into heat (i.e. microscopic kinetic energy). I have seen some references to pseudo-dissipation recently, so there is perhaps a growing awareness that Onsager's conjecture needs further critical thought.

Onsager's paper concludes with the sentence: 'The detailed conservation of energy (i.e. the global conservation law of the nonlinear term) does not imply conservation of the total energy if the total number of steps in the cascade is infinite and the double sum … converges only conditionally.' The italicised parenthesis is mine as Onsager referred here to one of his equation numbers. However this is merely an unsupported assertion which is incorrect on physical grounds because:

1. The number of steps is never infinite in a real physical flow.
2. The individual interactions are conservative so it is not clear how mere summation can lead to overall non-conservation.
3. The physical process involves a renormalization which means that there is a well-defined physical infinite Reynolds number limit at quite moderate Reynolds numbers.

It is totally unclear to me what *mathematical* justification there can be for this statement; and discussions of it that I have seen in the literature seem to me to be unsound on physical grounds. I shall return to these points in future blogs.

[1] That is, 'a jar', geddit? Oh dear, I suppose I am getting into holiday mood!

[2] W. D. McComb. Renormalization Methods: A Guide for Beginners. Oxford University Press, 2004.
[3] W. D. McComb, A. Berera, S. R. Yoffe, and M. F. Linkmann.

Energy transfer and dissipation in forced isotropic turbulence. Phys. Rev. E, 91:043013, 2015.
[4] L. Onsager. Statistical Hydrodynamics. Nuovo Cim. Suppl., 6:279, 1949.
[5] G. L. Eyink and K. R. Sreenivasan. Onsager and the Theory of Hydrodynamic Turbulence. Rev. Mod. Phys., 87:78, 2006.