# The exactness of mathematics and the inexactness of physics.

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This post was prompted by something that came up in a previous one (i.e. see my blog on 12 August 2021), where I commented on the fact that an anonymous referee did not know what to make of an asymptotic curve. The obvious conclusion from this curve, for a physicist, was that the system had evolved! There was no point in worrying about the precise value of the Reynolds number. That is a matter of agreeing a criterion if one needs to fix a specific value. But evidently the ratio shown was constant within the resolution limits of the measurements of the system; and this is the key point. Everything in physics comes down to experimental error: the only meaningful comparison possible (i.e. theory with experiment or one experiment with another) is subject to experimental error which is inherent. Strictly one should always quote the error, because it is never zero.

In everyday life, there are of course many practical expedients. For instance, radioactivity takes in principle an infinite amount of time to decay completely, so in practice radioisotopes are characterised by their half-life. So the manufacturers of smoke alarms can tell you when to replace your alarm, as they know the half-life of the radioactive source used in it. In acoustics or diffusion processes or electromagnetism, exponential decays are commonplace, and it is usual to introduce a relaxation time or length, corresponding to when/where the quantity of interest has fallen to $1/e$ of its initial value.

In fluid mechanics, the concept of a viscous boundary layer on a solid surface is of great utility in reconciling the practical consequences of a flow (such as friction drag) with the elegance and solubility of theoretical hydromechanics. The boundary layer builds up in thickness in the stream-wise direction as vorticity created at the solid surface diffuses outwards. But how do we define that thickness? A reasonable criterion is to choose the point where the velocity in the boundary layer is approximately equal to the free-stream velocity. From my dim memory of teaching this subject several decades ago, a criterion of $u_1(x_2) = U_1$, where $U_1$ is the constant free-stream velocity, was adequate for pedagogic purposes.

An interesting partial exception arises in solid state physics, when dealing with crystal lattices. The establishment of the lattice parameters is of course subject to the usual caveats about experimental error, but for statistical physics lattices are *countable* systems. So if one is carrying out renormalization group calculations (e.g see [1]) then one is coarse-graining the description by replacing the unit cell, of side length $a$, by some larger (renormalized) unit cell. In wavenumber (momentum) space, this means we start from a maximum wavenumber $k_{max}=2\pi/a$ and average out a band of wavenumber modes $k_1 \leq k \leq k_0$, where $k_0=k_{max}$. You can see where the countable aspect comes in, and of course the initial wavenumber is precisely defined (although of course its precise *value* is subject to the error made in determining the lattice constant).

When extending these ideas to turbulence, the problem of defining the maximum wavenumber is not solved so easily. Originally people (myself included) used the Kolmogorov dissipation wavenumber, but this is not necessarily the maximum excited wavenumber in turbulence. In 1985 I introduced a criterion which was rather like a boundary-layer thickness, adapting the definition of the dissipation rate, thus: \[\varepsilon = \int^{\infty}_0 \, 2\nu_0 k^2 E(k) dk \simeq

\int^{k_{max}}_0 \, 2\nu_0 k^2 E(k) dk,\] where $\nu_0$ is the molecular viscosity and $E(k)$ is the energy spectrum [2]. When I first started using this, physicists found it odd, because they were used to the more precise lattice case. I should mention for completeness that it is also necessary to use a non-trivial conditional average [3].

Recently there has been growing interest in these matters by those who study the philosophy of maths and science. For instance, van Wierst [4] notes that in the theory of critical phenomena, phase transitions require an infinite system, whereas in real life they take place in finite (and sometimes quite small!) systems. She argues that this paradox can be resolved by the introduction of ‘*constructive mathematics*’, but my view is that it can be adequately resolved by the concept of scale-invariance. Which brings us back to the infinite Reynolds number limit for turbulence. But, for the moment, I have said enough on that topic in previous posts, and will not expand on it here.

[1] W. D. McComb. Renormalization Methods: A Guide for Beginners. Oxford University Press, 2004.
[2] W. D. McComb. Application of Renormalization Group methods to the subgrid modelling problem. In U. Schumann and R. Friedrich, editors, Direct and Large Eddy Simulation of Turbulence, pages 67-81. Vieweg, 1986.
[3] W. D. McComb and A. G. Watt. Conditional averaging procedure for the elimination of the small-scale modes from incompressible uid turbulence at high Reynolds numbers. Phys. Rev. Lett., 65(26):3281-3284, 1990.
[4] Pauline van Wierst. The paradox of phase transitions in the light of constructive mathematics. Synthese, 196:1863, 2019.