Does Turbulence need God?

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The questions before us are:

- Are the turbulence models based on physics?
- Do we know all the necessary physics?

My answers in brief will be:

- Certainly the models we have today were (and are) based on the physics as we knew it yesterday.
- But the physics we know today is very different from yesterday.

Let's first look at the basic Reynolds stress model. What 'physics' did does get right?

- Actually I think the RS model is probably *one of the great intellectual triumphs of the second half of the 20th century*.
- I did not contribute directly to this effort.
- But I did have a front row seat to observe how this happened.
- I want to show you how I think it reflects the physics as we knew it 40 years ago.

- It was based upon the basic K41 picture – at least the K41 version put forth by Batchelor in a series of papers in the late 40's and in his Homogeneous Turbulence book(s).
- Actually according to Arkady this spectral version is probably more due to Obukov.

The fundamental K41 picture to which I refer is this one:

- If the Reynolds number is sufficiently high, there exists a local equilibrium range spectral range for which the non-linear spectral transfer and dissipation are essentially in balance.
- In the limit of infinite Reynolds number, then there also exists an inertial subrange where only the non-linear spectral transfer is important. In it the spectral flux, say $\varepsilon_k|_{inertial\ subrange}$ is equal to the rate of dissipation of turbulence energy by viscosity, say ε. The is almost true at very very large Re.
- If this equilibrium range exists, then the spectral flux provided to it from the energetic scales must ALSO be equal to the dissipation.
- From this (and this alone) we can DEFINE a length scale characteristic of the ENERGETIC eddies, say $L_\varepsilon = u^3/\varepsilon_k|_{inertial\ subrange} = u^3/\varepsilon$.
- In the limit of INFINITE REYNOLDS NUMBER we can show L_ε to be proportional to the true integral length scale, L, defined from the correlation (or spectrum).

Now the fun begins – additional hypotheses we have *assumed* to have also be confirmed, but in fact were never more than 'working' hypotheses.

- "The universal equilibrium range has been shown experimentally to be true." Actually it has only been demonstated in *statistically stationary* flows (e.g., 'forced' DNS, laboratory shear flows, 'statistically stationary' portions of the atmosphere). These flows are in fact *statistically stationary* and therefore must be also in *local equilibrium*, quite independent of K41 (or any other hypothesis).
- "Local isotropy of the small scales, and even of the dissipation." Few flows exhibit dissipation isotropy, unless already nearly isotropic.
- "The universal equilibrium range is universal." Remember the famous plot of one-dimensional spectra from all different flows collapsing in Kolmogorov variables – ignoring to point out that in all of these plots the dissipation was determined by collapsing the curves (usually by choosing the dissipation to line up the $k^{-5/3}$ range 'point of tangency').

- "The ratio of integral scale to pseudo-integral scale is constant *and universal*." Contrary to popular opinion (and some oft cited papers), this is demonstrably false from a large number of experiments. In fact in some flows (the statistically stationary ones in fact), L/L_ε appear to reach an asymptote as the Reynolds number increases. Others do not.
- It is obvious how the multipoint models (like LES, EDQMN, etc) depend on these ideas.
- It is not so obvious how RANS does – or at least was believed to in the 1970's.

- No matter the model, there was (is) always a coefficient involving u and L_ε, either as an eddy viscosity, say $\nu_t \propto u\ L_\varepsilon$, or as a time scale, $\tau_L \propto L_\varepsilon/u$

  E.G.

$$u_i u_k u_j \propto \left[\frac{L_\varepsilon}{u}\right]\left\{u_i u_l \frac{\partial u_j u_k}{\partial x_l} + u_j u_l \frac{\partial u_k u_i}{\partial x_l} + u_k u_l \frac{\partial u_i u_j}{\partial x_l}\right\} \quad (1)$$

- Note how fluxes of one quantity can be due to gradients of another an in other directions (e.g. Launder, Reece & Rodi, various Lumley versions).
- All closures must be made to have proper tensorial invariance to change of coordinates, rotation, etc. Many terms needed.
- Direct dependence on local Reynolds number, uL_ε/ν, can also be incorporated – which compensates empirically for the lack of a spectral gap. Many empirical functions and coefficients.
- Also and the coefficients can be written in terms of the invariants of $\overline{u_i u_k}$, etc. More empirical constants needed.

- All models of pressure-strain rate involve this time scale, ; e.g. return to isotropy (another item of ‘faith’, rapid term, elliptic relaxation. Latter two well-founded in homog. theory for pressure fluctuations.
- My own view of URANS is that it is like a low-pass temporally filtered version ensemble average NS; i.e., includes non-stationary effects up to ‘filter frequency’ – which is u/L_ε. Therefore valid for times much longer than this, whether or not there is a temporal scale separation. My reason is that most closure assumptions really are more based on local homogeniety than stationarity.

The so-called 'dissipation' model equation is even more primitive; e.g.,

$$\frac{D\varepsilon}{Dt} = c\frac{\partial}{\partial x_k}\left(\frac{u}{L_\varepsilon}\overline{u_k u_l}\frac{\partial \varepsilon}{\partial x_l}\right) - c'_{\varepsilon 1}\frac{\overline{u_i u_k}\frac{\partial U_i}{\partial x_k}}{L_\varepsilon / u} - c'_{\varepsilon 2}\frac{\varepsilon}{L_\varepsilon / u} \qquad (2)$$

Plus we 'assume' *isotropy* of the dissipation – with corrections proportional to the Reynolds stress for low Reynolds numbers.

These equations and models did not evolve in a vacuum.

- Since they originally were believed to be based on ideas of universality of the small scales and independence of initial (or upstream) conditions, there was a serious attempt to evaluate them against ‘viscometric’ turbulence experiments.
- I was involved in some of these experiments (wake, jet, plumes, contraction, grid).
- It was originally hoped by all that a universal turbulence model would be possible.

- Even from the beginning the turbulence ‘viscometric’ experiments were problematical.
- The models described evolution pretty well (i.e., especially as they became more sophisticated, but with coefficients which varied from one flow to another.
- The favorite ‘fix’ was to decide which experients were ‘correct’, and which were ‘anomalous’; e.g., the famous -1.22 decay for decaying turbulence was good, the axisymmetric jet data bad.
- Free shear flows alway from walls were always a problem, especially round jets vs plane jets, axisymmetric wakes, plane wakes. Separation and APG’s hopeless.
- Lumley’s 1983 comment quoted yesterday was at the end of over a decade of frustration. Almost all modellers gave up hope for any universal models.
- Most now pretend they had never hoped for them. :-)

- Sometimes things only 'appear to work' ... or

RRWR

Right Result, Wrong Reason... Tsinober (many times)

- I think I now understand WHY the RANS models seemed to get the physics right. But didn't work very well – or at least were not universal.
- Two reasons:
  - They only had part of the physics.
  - The whole physics explains both why the models worked – and why they didn't.
- The bad news is that K41 (or even K62) is not the whole story.
- Maybe there are THREE STORIES – for sure at least TWO.

In 1986 I fell (or was pushed there by my students) into an alternative universe – the **BIZZARO WORLD of EQUILIBRIUM SIMILARITY** – but NO LOCAL EQUILBRIUM.

- It begins with the spectral energy equation for isotropic turbulence given by:

$$\frac{\partial E}{\partial t} = T - 2\nu k^2 E \tag{3}$$

- Note that this is often suggested to be the Fourier space counterpart to the von Karman/Howarth equation. It is not, since it is does not require the assumption of isotropic turbulence.
- The equilibrium similarity theory summarized below was presented in George 1989 (Advances in Turbulence, George and Arndt eds. Bacon and Francis (Hemisphere), and in great detail by George 1992 ('The Decay of Homogeneous Isotropic Turbulence,' Physics of Fluids B, 5, 1 - 29.)

Similarity solutions were sought of the form:

$$
\begin{aligned}
E(k,t) &= E_s(t)f(\bar{k}) \qquad (4)\\
T(k,t) &= T_s(t)g(\bar{k}) \qquad (5)
\end{aligned}
$$

where

$$\bar{k} = kl(t) \qquad (6)$$

The functions $E_s(t)$, $T_s(t)$ and $l(t)$ were not assumed *a priori* as in the von Karman/Howarth and Batchelor analyses, but were determined from the equations themselves by an *equilibrium similarity* hypothesis described below.

Substituting these into equation 3 and multiplying by $l^2/\nu E_s(t)$ yields:

$$[\frac{l^2}{\nu E_s}\frac{dE_s}{dt}]f + [\frac{l}{\nu}\frac{dl}{dt}]\overline{k}\frac{df}{d\overline{k}} = [\frac{l^2 T_s}{\nu E_s}]g - [2]\overline{k}^2 f \qquad (7)$$

The *equilibrium similarity* hypothesis simply requires that the flow evolve asymptotically in time in such a manner that all of the terms in equation 7 retain exactly the relative value at the same value of scaled wavenumber $\overline{k}$.

Said another way, all of the terms in square brackets of equation 7 must evolve with time in exactly the same way so the relative balance of the equation is maintained. Since one of them is constant, all must be. There is no reason to believe the constants to be independent of the initial conditions, nor are they.

The requirement for an equilibrium solution is precisely the requirement for any single set of scales to collapse the data over all wavenumbers, since the collapse can be perfect only if the equations admit to such solutions. *Why* the flow might behave this way has been a matter for speculation for nearly a century, but it has been generally observed that when such solutions exist, nature finds them, George 1989, 1999.

No further assumptions are required to determine the following:

- The **energy spectra** collapse at *all* wavenumbers for fixed initial (or upstream conditions) when plotted as $E(k,t)/u^2\lambda$ versus $k\lambda$.
- **The non-linear spectral transfer function** collapses when plotted as $\lambda T(k,t)/\nu u^2$ versus $k\lambda$. This surprising result is the primary difference from the earlier analyses of von Karman/Howarth, Batchelor and Lin who all *assumed* at the outset that T scaled with u^3.
- The **turbulence energy** must decay as a power law:

$$\frac{3}{2}u^2 = B[t - t_o]^n \tag{8}$$

- Note that this implies that the rate of dissipation is given by:

$$\epsilon = -\frac{3}{2}\frac{du^2}{dt} = -nB[t - t_o]^{n-1}. \tag{9}$$

- The **Taylor microscale** is given by:

$$\lambda^2 = \frac{10}{-n}\nu[t - t_o] \tag{10}$$

where

$$\lambda^2 \equiv 15\nu\frac{u^2}{\epsilon} \tag{11}$$

The linear dependence on time follows directly from the power law decay of the energy.

- The product of the velocity derivative skewness, $S_{\partial u_1/\partial x_1}$ and R_λ is constant and dependent only on the initial conditions.

Grid turbulence (at least for normal grids) does seem to produce true power law decay where $t = x/U$, since λ^2 really is linear in t.

Thus if $(U/u')^2$ is proportional to $(t-t_0)^n$ for some part of the decay range, the energy equation—which is an exact deduction from the condition of isotropy—demands for consistency that

$$\frac{d\lambda^2}{dt} = U\frac{d\lambda^2}{dx} = \frac{10\nu}{n}. \tag{4·2}$$

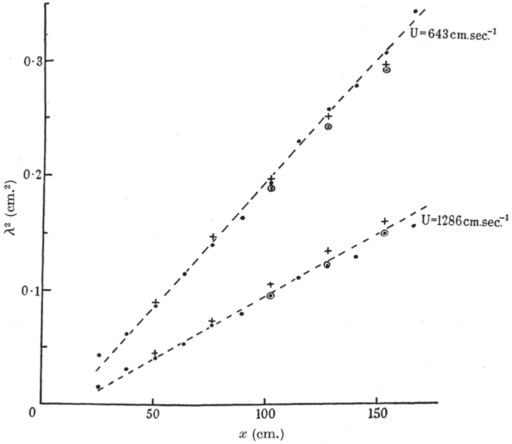
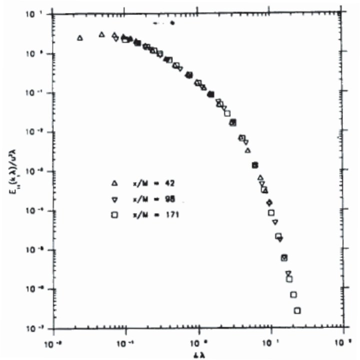


FIGURE 6. Variation of λ during decay.

●, $M = 1·27$ cm.; +, $M = 2·54$ cm.; ⊙, $M = 5·08$ cm.; ---, $\dfrac{d\lambda^2}{dx} = \dfrac{10\nu}{U}$.

The famous and oft-cited Comte-Bellot/Corrsin 1971 JFM experiment provides excellent support

Spectra at grid Reynolds numbers of 34,000 and 17,000 ($72 \geq R_\lambda \geq 61$ and $49 \geq R_\lambda \geq 37$ respectively) collapse remarkably well in Taylor variables at ALL wavenumbers.



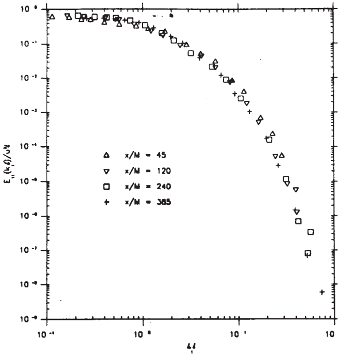


Figure: Comte-Bellot/Corrsin 1971 spectra in Taylor variables from two different grids: left: 2 in. grid at $x/M = 42$, 98, and 171; right: 1 in. grid at $x/M = 45$, 120, 240 and 385 (from George 1992 Phys.Fluids).

... and so do the much higher Reynolds number spectra of Kang et al. 2003 JFM (active grid Reynolds number about $1.1x10^5$, $716 \geq R_\lambda \geq 626$) for four downstream positions in the same wind tunnel.

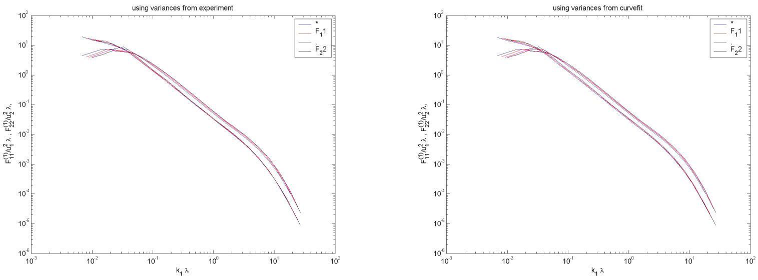
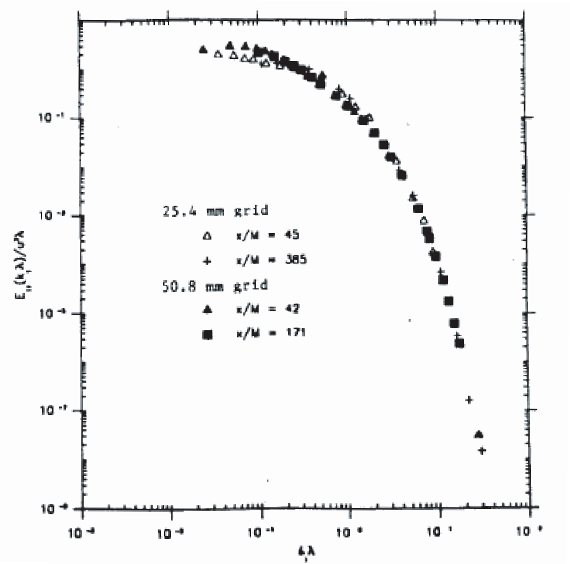


Figure: Longitudinal and transverse component velocity spectra in Taylor variables from high Reynolds number active grid experiment of Kang et al. 2003 JFM in Corrsin tunnel. left: data from paper. right: data from reanalysis by Wänström and George 2008 APS/DFD mtg).

Effect of initial conditions

As predicted by the theory, the spectra are different for different grids.



The theory has been extended (Wänström and George 2008) to show that permanently anisotropic decay is theoretically possible – as the preponderance of the data clearly indicate

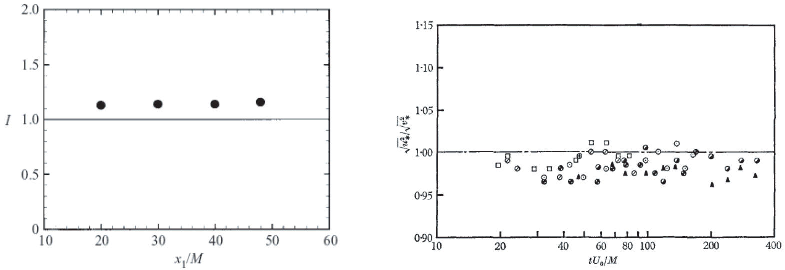


Figure: Anisotropy ratios from Corrsin tunnel (from Wänström and George 2008 APS/DFD mtg). left: high Reynolds number active grid experiment of Kang et al. 2003 JFM. right: Square bar grid after contraction of Comte-Bellot/Corrsin 1966 JFM.

Even the Batchelor/Townsend 1948 Proc. Roy. Soc. paper (which argues for an alternative view) provides at least as strong a support for the G92 theory. Note that in spite of the curves drawn, for each set of data for $x/M \geq 50$, $L \propto (t - t_o)^{1/2}$ (left figure) and L/λ (right figure) is nearly horizontal. Also the coefficients vary with grid Reynolds number (or initial conditions).

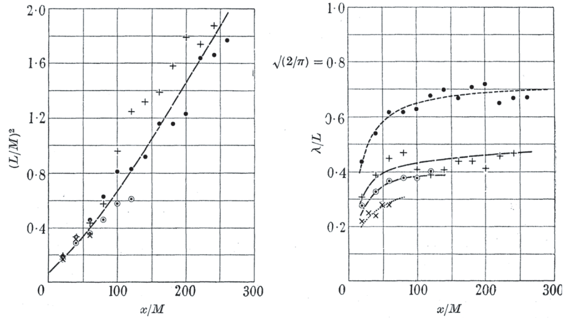


FIGURE 10. Variation of scale during decay.

FIGURE 11. λ/L *vs.* x/M.

● $R_M = 2810$. + $R_M = 5620$. ⊙ $R_M = 11250$. × $R_M = 22500$.

Figure: left: Integral scale squared versus x/M. Right: L/λ versus x/M. from Townsend/Batchelor 1948 Proc.Roy.Soc.

The derivative skewness (for fixed upstream or initial conditions) varies inversely with R_λ during decay. To see this, multiply the spectral energy equation by k^2, integrate and normalize to obtain:

$$G = \frac{30}{7} + \frac{1}{2} S_{\partial u_1/\partial x_1} R_\lambda \tag{12}$$

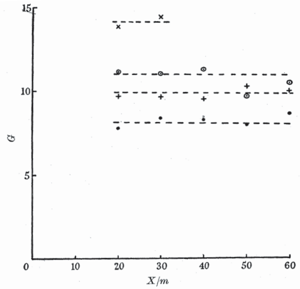


FIGURE 10. Variation of G during decay.
$\times$, $M = 5{\cdot}08$ cm., $U = 909$ cm.sec.$^{-1}$; $\odot$, $M = 2{\cdot}54$ cm., $U = 909$ cm.sec.$^{-1}$; $+$, $M = 2{\cdot}54$ cm., $U = 643$ cm.sec.$^{-1}$; $\bullet$, $M = 1{\cdot}27$ cm., $U = 643$ cm.sec.$^{-1}$.

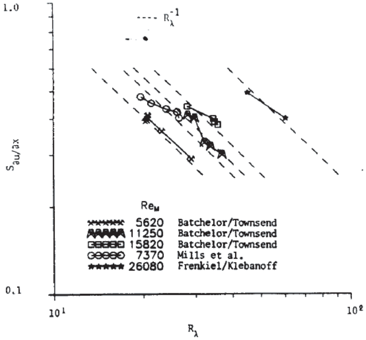


Figure: left: G versus R_λ for Batchelor/Townsend 1948 experiment. right: G92 plot of same data but of $logS$ versus $logR_\lambda$.

How did things go wrong? How could we have missed these solutions before?

Batchelor and Townsend 1948 make the remarkable statement: "In view of the attempts of v.Karman and Howarth (1938) and of Dryden (1943) to deduce the laws of decay from the assumption that the various correlations functions $f(r)$, $k(r)$, etc. are only functions of r/l where l is a length which may change during decay, it is of interest to note that the measurements show G .. to be constant during decay." They then go on to **assume** that $S_{\partial u_1/\partial x_1}$ must therefore be constant during decay with the consequence that the triple correlation independent (and corresponding non-linear transfer function) must be independent of R_λ.

And later they say: “If the result that S is an absolute constant be assumed to hold for indefinitely large values of R_M ...then G approximates to the form $G = 0.2R_\lambda$.”

Recall: This obviously also requires $R_\lambda = \textit{constant}$ which is possible **ONLY IF** $u^2 \propto t^{-1}$. But R_λ is clearly **not** constant, as the subsequent measurements of Corrsin and co-workers showed.

Thus the Batchelor/Townsend theoretical construction collapses.

The turbulence community seems to have recognized that R_λ is not constant during decay, but not the consequences for the theory which demands it be true.

Clearly, even though rejecting the underlying *and necessary* hypotheses, the turbulence field has been willing to keep the consequences of the deductions from assumptions about the flow which are incorrect – namely the idea that the turbulence cannot be described by a single length scale.

In fact, by the 1970's it had become a religion. Not only was it believed that turbulence could never be described by a single length scale, the Taylor microscale was not even believed to be a length scale (c.f. Tennekes and Lumley 1972 A First Course in Turbulence).

Most of these wrong ideas are still promulgated in even recent texts, in spite of rather conclusive evidence to the contrary, some of which we continue to review in this lecture.

This is tantamount to continuing to believe the earth is the center of the universe since one can navigate successfully using only the pre-Copernican (Ptolemaic) view of the universe. (Or for that matter that the earth is flat.)

Summary of G92 equilibrium similarity results

$$E_s(t) = u^2 l \tag{13}$$

$$T_s(t) = \frac{\nu u^2}{l} \tag{14}$$

$$l(t) = \lambda \tag{15}$$

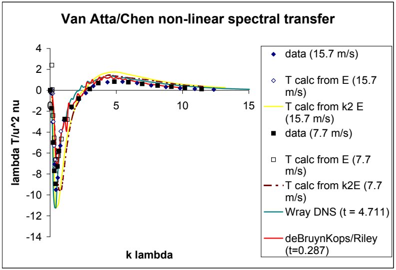
- The energy decays as a power law; i.e., $u^2 \propto t^n$ where n is determined by (or reflected) the initial conditions and was constant during decay).
- The integral scale, L, (determined from the correlation or the spectrum) is proportional to λ. This means L/λ is constant during decay, and is set by the initial conditions.
- Most importantly (and crucially), the scale function for the non-linear transfer, $T_s(t)$ is NOT u^3 (as assumed by von Karman/Howarth (and Batchelor/Townsend) BUT instead u^3/R_λ.

It is easy to show (v. G92) by substitution into the spectral energy equation that the scaled spectral energy function, $g(\overline{k}, *)$ is given by:

$$g = -\frac{5}{n}[f + \overline{k}f'] - 10f + 2\overline{k}^2 f \tag{16}$$

- This provides a direct closure of the equations, which is in excellent agreement with experiments (of which there are only two).
- Note that given the three-dimensional energy spectrum function shape f, there is only one unknown, the exponent of the rate at which the energy decays, n. Both f and n depend on the initial conditions.

Prediction and data are nearly indistinguishable.



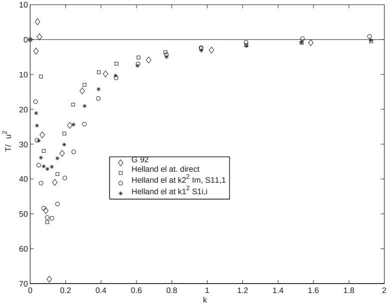


Figure: Nonlinear spectral transfer in Taylor variables from two different square bar grid wind tunnel experiments. left: Chen and Van Atta, JFM 1968 ($R_\lambda = 53$); right: Helland et al. JFM 1977 ($R_\lambda = 237$). Each experiment has a unique spectral shape, reflecting the very different Reynolds numbers and upstream conditions (from George and Wang 2003).

And the DNS is also in excellent agreement with the deduced closure relation (from Wang et al. 2000).

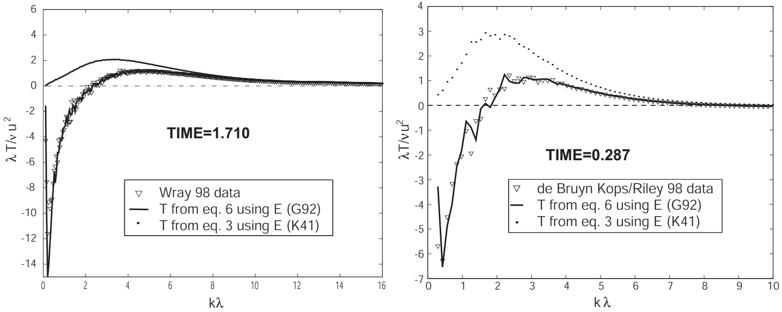


Figure: left: DNS of Wray 1999 right: DNS of DeBruyn-Kops/Riley 1999

Note that the relative values for $k\lambda > 5$ do not change with increasing wavenumber, implying we do NOT approach K41 with increasing wavenumber.

Nonetheless, the DNS was problematical, in large part because of the integral scale. But this was shown by Wang and George (2002 JFM) to be very much influenced by the computational box-size relative to where the spectral peak lies relative to it since $L = (\pi/2u^2)\int_0^\infty E(k)/k dk$, thereby emphasizing the lowest wavenumbers (largest scales).

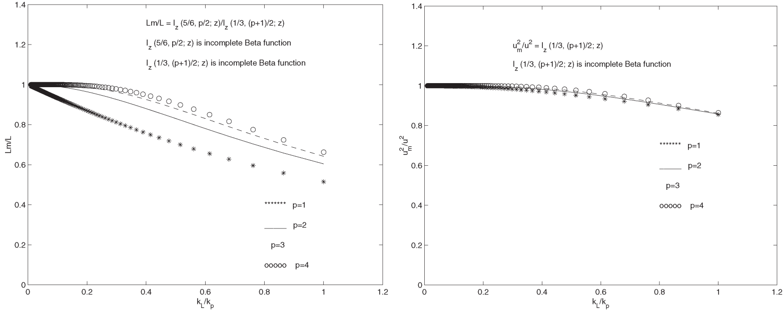


Figure: left: Effect of low wavenumber cutoff (box-size) on integral scale. right: Effect on turbulence intensity. from Wang and George 2002 JFM

It is clear that there is not only a lack of low wavenumbers in the simulations (left), but also a considerable transient at the beginning (right)– plus something going on at the end. Thus there is only a limited range over which a true homogeneous turbulence power law behavior can be approximated. This this can adversely affect judgements about the overall dynamics of such simulations and experiments, at least from a theoretical perspective.

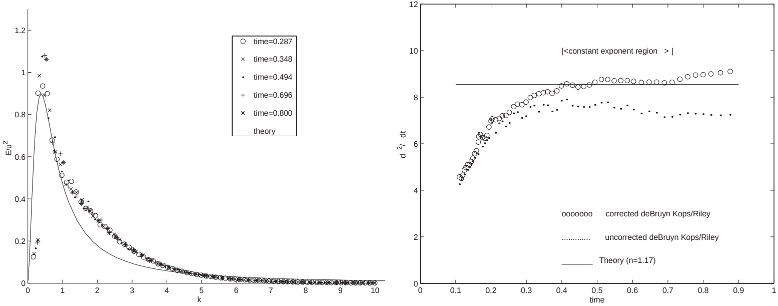


Figure: left: DeBruyn-Kops/Riley 1998 JFM spectra in Taylor variables. right: $d\lambda^2/dt$, DeBruyn-Kops/Riley 1998 JFM. (from Wang and George 2002 JFM)

Finite Box Effects on the Energy Spectrum

We can only approximate homogeneous turbulence by DNS or experiments. Some features are easy; others are more difficult.

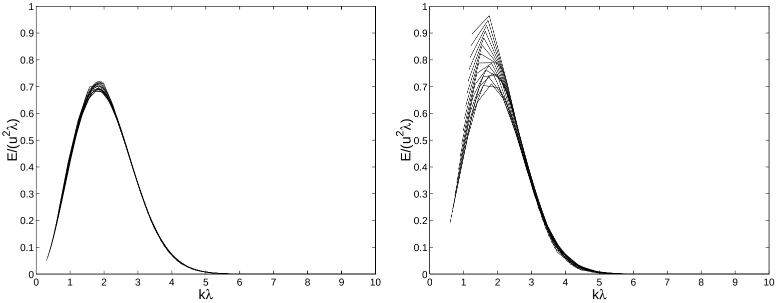


Figure: left: Energy spectra in Taylor variables, 64^3 simulation ($k_p/k_L = 6$, $R_\lambda \approx 35$). Right: Energy spectra in Taylor variables, 32^3 simulation ($k_p/k_L = 3$, $R_\lambda \approx 35$). (from George et al. 2001)

For DNS spectra the peak in the energy needs to be nearly an order of magnitude above the lowest wavenumber.

Finite Box Effects on the Integral Scale Integrand

Two simulations with same resolution (same highest wavenumber and Reynolds number) showing consequences of placing peak too close to lowest wavenumber (George, et al. 2001, "Homogeneous Turbulence and Its Relation to Realizable Flows", see refs at end.) The integral under curves is proportional to the integral scale.

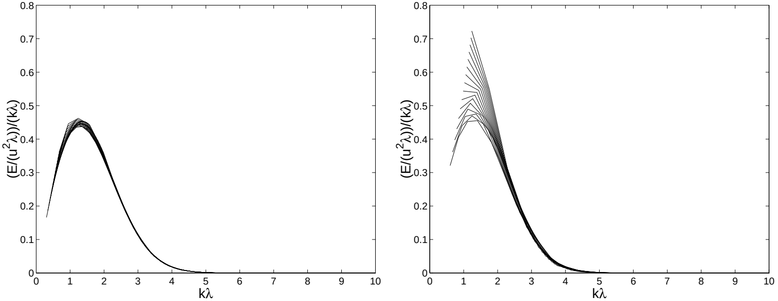


Figure: left: Energy spectra in Taylor variables divided by $k\lambda$ with peak at 6 times lowest wavenumber, 64^3 simulation. right: Energy spectra in Taylor variables divided by $k\lambda$ with peak at 3 times lowest wavenumber,

The derivative skewness remains problematical

Is it constant? Or does it increase during decay as R_λ decreases (for fixed initial conditions)?

Part of the problem is clearly related to our difficulties in resolving or measuring the very highest wavenumbers on which they crucially depend. This is easily seen from its relation to the integral of the enstrophy equation:

$$\frac{\langle[\partial u_1/\partial x_1]^3\rangle}{\langle[\partial u_1/\partial x_1]^2\rangle^{3/2}} = -\frac{3\sqrt{30}}{14}\frac{\int_0^\infty k^2 T(k)dk}{\left[\int_0^\infty k^2 E(k)dk\right]^{3/2}} \tag{17}$$

where from the spectral energy equation the numerator is given by:

$$\int_0^\infty k^2 T(k)dk = \frac{d}{dt}\int_0^\infty k^2 E(k)dk + 2\nu\int_0^\infty k^4 E(k)dk \tag{18}$$

The first term on the right-hand-side is just the dissipation and depends only on $k^2 E$, but the second on $k^4 E$ is much more demanding of a simulation or experiment.

Easy to show that in Taylor variables (George 1992, Phys. Fluids):

$$SR_\lambda = \frac{30}{7}\left(\frac{n-1}{n}\right) - \frac{4}{35}\int_0^\infty \overline{k}^4 f(\overline{k}) d\overline{k} \tag{19}$$

where $f(\overline{k}) = E/[u^2\lambda]$, $\overline{k} = k\lambda$, and $n < 0$ is energy decay exponent (if power law).

Clearly we should be examining what happens to plots of $k^2 T$ or $k^4 E$ versus k.

If data collapse at all wavenumbers when normalized by u^2 and λ, then $SR_\lambda = constant$.

Plotted below are running integrals of SR_λ versus $k\lambda$.

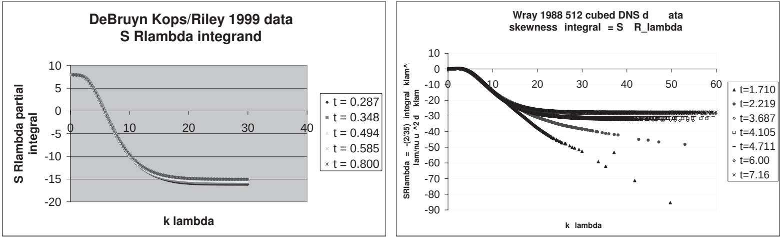


Figure: Integrand of derivative skewness integrals in Taylor variables of 512^3 simulations of decaying turbulence. Left: DeBruyn-Kops/Riley 1998. Right: Wray 1998, (from Wang et al. 2001)

Clearly there is excellent collapse below $k\lambda \approx 10 - 15$.

Is problem at the higher wavenumbers due the different physics than the equilibrium similarity theory? Or is it just due to resolution or box-size effects (Note: the triadic interactions couple the large and small scales.)?

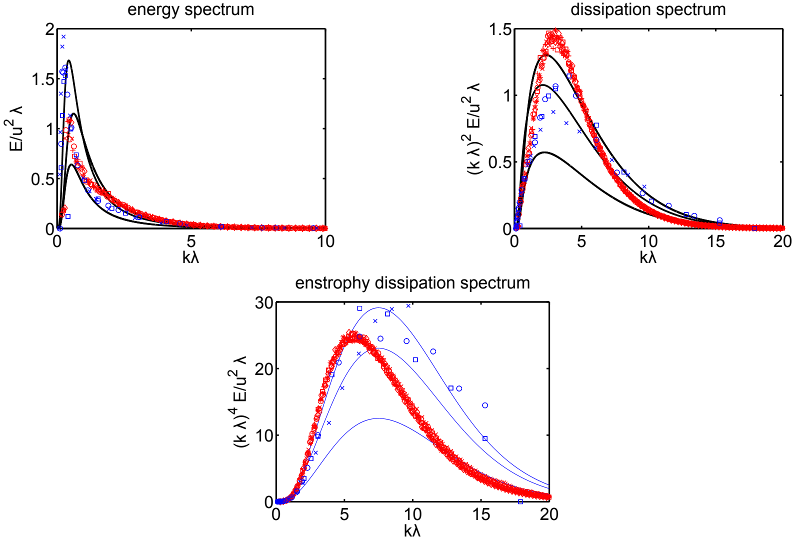


Figure: Effect of different $L/\lambda = constant$ during decay. Blue symbols: Comte-Bellot/Corrsin 1971, $L/\lambda = 5$. Red symbols: DeBruyn-Kops/Riley DNS, $L/\lambda = 3.4$. Solid lines are modified K41 spectral model of Gamard/George 2000 with different parameters.

Integral invariants and initial conditions

The relation between $E(k)$ and the two–point correlations for isotropic turbulence can obtained by substituting the isotropic relations for $B_{i,i}(\vec{r})$ and carrying out the indicated integration over the sphere of radius k. The result after some manipulation is:

$$E(k) = \frac{2}{\pi} \int_0^\infty [B_{LL}(r) + 2B_{NN}(r)][k \sin(kr)] r dr \qquad (20)$$

Lumley (1972) and others have noted that it is possible to Taylor expand the $\sin(kr)$ term in powers of kr and factor the powers of k outside the integrals, so that the leading term is k^2.

The resulting power series in k (even in the limit as k goes to zero) converges only if the integrals exist for all powers of r, which is in turn possible **only if the tails of the correlations roll-off exponentially**. Differentiation by k yields the relation between $E = C_m k^m$ and I_m, the first non–zero integral invariant, **but only for integer values of m**.

If it is assumed that the spectrum near $k = 0$ is proportional to k^p where p is a constant and $1 \leq p \leq 4$ then $n = -(p+1)/2$ since $g = 0$ at $k = 0$. Thus it is the shape of the spectrum near the wavenumber origin that uniquely determines the decay rate, and therefore at least part of the mysterious argument '*'. Note that contrary to frequent assumption (e.g., Chasnov 1993), p is not necessarily integer since there is no reason to assume the spectrum is analytical near $k = 0$ (v. Lumley 1970).

The recent analysis of Gustafsson and George 2008) shows that only the Loitsianskii invariant is viable (or constant) in an infinite domain! There are very interesting consequences for finite domains, however, which cause the Loitsianskii integral to not be constant. This in turn opens up the possibility of decay which depends on the initial conditions, including fractional powers of m or n, as observed experimentally.

What does all of this have to do with turbulence modelling?

- Amazingly MOST of the closure relations fall immediately out of the equilibrium similarity theory as EXACT – e.g. dissipation equation, pressure-strain rate,etc. (George 2001 Australasion Meeting) except for return-to-isotropy.
- Thus even if K41 does not apply to non-equilibrium turbulence, equilibrium similarity does – at least to the flows considered – and this include all of the 'calibration' flows.
- BUT there is one BIG DIFFERENCE: the equilibrium similarity solution coefficients are flow specific; i.e., each flow has its (possibly unique) coefficients which are determined by the initial (or upstream) conditions.
- The more information (structural, etc) you put into the coefficients, the more general the model.
- This is pretty much what the modelling community concluded years ago on empirical grounds.

The real tests of any 'theory' are:

1. Can it account for the data?
    - This is especially important if others have been satisfied with the previous explanations.
    - In turbulence, experiments often have enough uncertainty to be used in support of multiple explanations.
    - Plus the longer a theory has been viewed as acceptable, the more difficult it is to overturn it, no matter the evidence or previously unexplained ambiguities.
2. Can it explain things previously unexplained *without additional hypotheses*?
3. Can it predict things previously unobserved *without additional hypotheses*?

Temperature fluctuations behind a grid

The strong dependence of temperature fluctuations on initial conditions was very much unexplained.

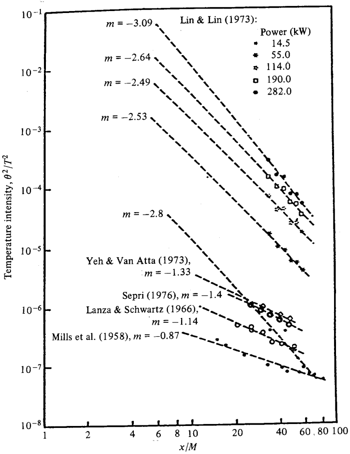


Figure 1. Decay of temperature fluctuations behind heated grid (from Warhaft and Lumley 1978).

Temperature fluctuations behind a grid

The equilibrium similarity solution, like the data, have a strong dependence on initial conditions, but both velocity and temperature spectra collapse for fixed initial conditions with only their respective mean square values and the Taylor microscales (George, W.K. (1990)

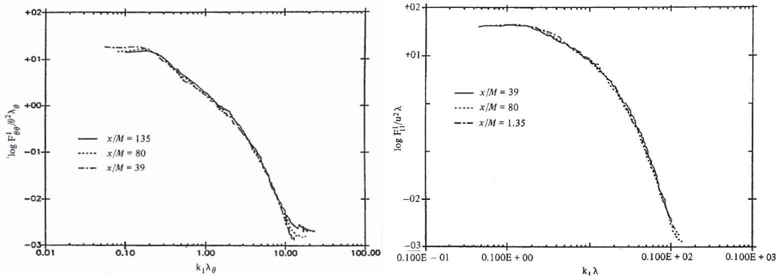


Figure: left: Temperature spectra in Taylor variables. Right Velocity spectra in Taylor variables. Data of Warhaft and Lumley (1988 JFM). from George 1990.

Homogeneous shear flow turbulence

- A curious anomaly of all experiments was the apparent asymptotic constancy of the Taylor microscale.
- The failure of turbulence intensity to reach a constant value was also a mystery, as was the apparent dependence on the upstream conditions.
- George and Gibson (1988,1992 Expts.in Fluids) showed that the turbulence intensities in homogeneous shear flow turbulence grew exponentially with a single length scale.
- L/λ was also constant, as was λ itself (i.e, the length scales defined from the spectrum and correlation function did not grow during decay).

The equilibrium similarity theory explained the hitherto 'anomalous' homogeneous shear flow experiments.

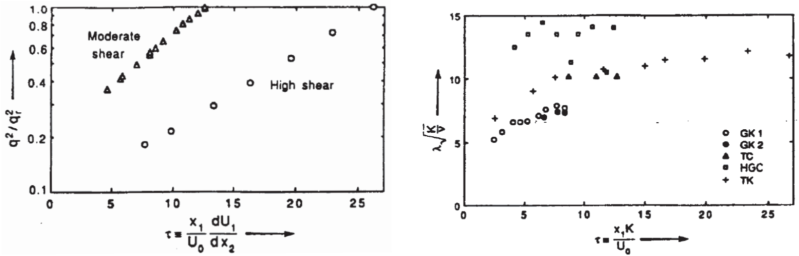


Figure: left: Semi-log plot of turbulence intensities from Tavoularis (1985) showing clearly exponential growth. right: Taylor microscales reach constant asymptote determined by initial (upstream) conditions. Data of Gibson/Kanellopoulos (1988)[GK1,GK2]; Tavoularis/Corrsin (1981)[TC]; Harris, Graham, Corrsin (1977)[HCG], and Tavoularis/Karnik (1989)[TK]. from George and Gibson 1992

And for each homogeneous shear flow experiment the spectra indeed collapsed incredibly well with only the turbulence intensity and Taylor microscale

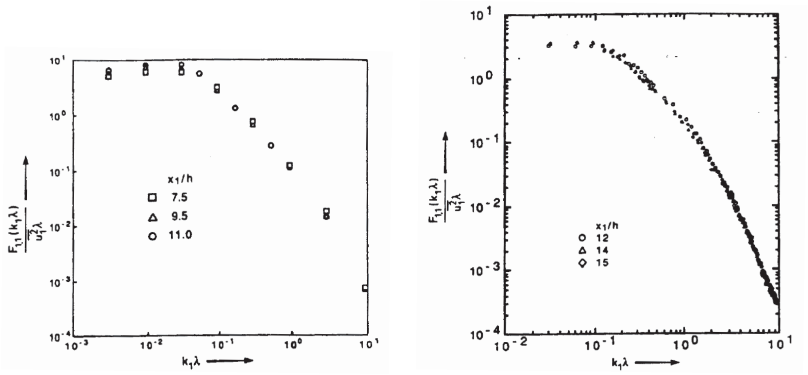


Figure: Spectra from two different homogeneous shear flow experiments collapsed in Taylor variables. left: Tavoularis/Corrsin 1981; right: Kanellopoulous/Gibson 1986. Each experiment has a unique spectral shape, reflecting the different mean shear rates and upstream conditions. from George and Gibson 1992

Homogeneous shear flow spectra of Rohr et al. 1988

These experiments by Rohr, Itsweire, Helland and van Atta at UCSD also showed exponential growth of the kinetic energy.

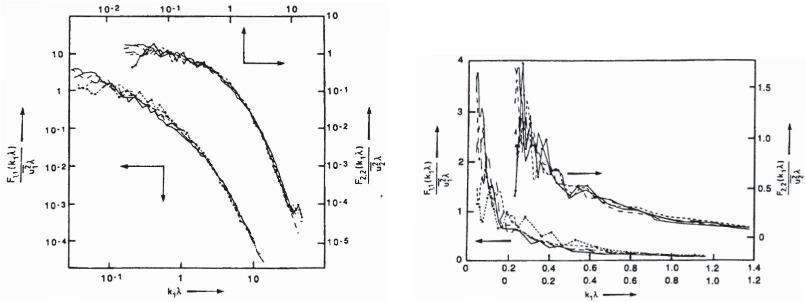


Figure: Spectra homogeneous shear flow experiment of Rohr et al. (1988 JFM) collapsed in Taylor variables. left: log-log; right: lin-lin. from George and Gibson 1992

Note that the spectra are only beginning to asymptote to a constant value as $k \to 0$, thus limiting the determination of the integral scales.

Can equilibrium similarity predict new things?

- 1999 Wang and George submit papars to JFM, Phys. Rev and J of Turbulence) in which they argued using equilibrium similarity that there might exist in nature exponentially decaying solutions which evolved at constant Taylor microscale scale and for which the spectra (and correlation functions) collapsed in Taylor variables.
- 1999-2000 JOT, JFM and Phys Rev Letters all reject theory as being physically impossible, since never observed in nature before.
- 2005 Vassilicos and Hurst observe exponentially decaying solutions which evolved at constant Taylor microscale scale and for which the spectra (and correlation functions) collapsed in Taylor variables.
- 2008 Phys. Fluids publishes 1999 paper with interesting foreword.

Exponentially decaying homogenous turbulence

The predicted (but initially ridiculed) exponentially decaying solutions ...found seven years later at ICL by Vassilicos and co-workers – and of all places, downstream of space-filling fractal grids.

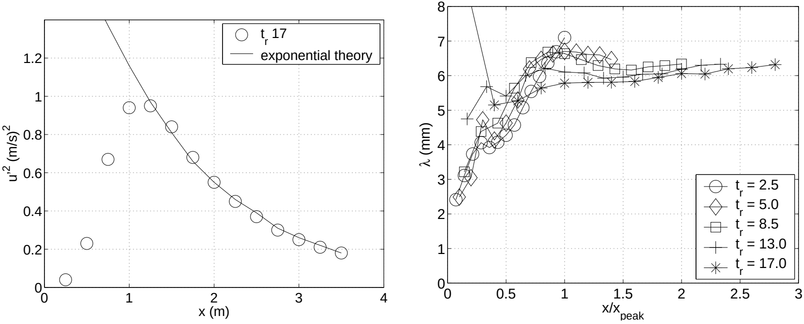


Figure: Results from Seoud, Hurst and Vassilicos experiments with fractal grids. left: Mean square streamwise velocity showing exponential decay. right: Taylor microscale reaching asymptotically constant value. from George and Wang 2008

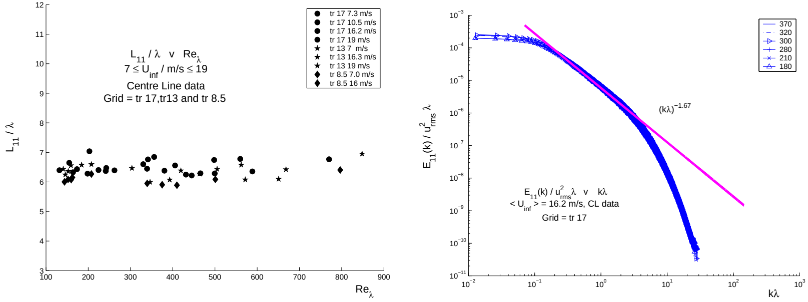


Figure: Results from Seoud, Hurst and Vassilicos experiments with fractal grids. left: Ratio of integral scale to Taylor microscale. right: Spectra scaled in Taylor variables showing collapse at all wavenumbers. from George and Wang 2008

How about Kolmogorov? Or Obukov? Or 'local equilibrium'

- Is there a role for Kolmogorov in these single length scale flows?
- It appears the answer is NO. $\varepsilon \neq u^3/L$ ever.
- These flows march to TWO different *non-equilibrium* drummers - one in which the Taylor microscale (and physical integral scale) are constant. the other in which it evolves.
- Both evolve at constant ratio of L/λ and for both $S_{\partial u/\partial x} \times R_\lambda = constant$ (where constant depends on initial conditions).
- Can these 3 kinds of turbulence co-exist? Or is there an exclusion principal?
- What happens to non-stationary flows for which these equilibrium similarity solutions do not exist? Can they behave as though in 'local equilibrium'? Or do they do something very different?

What does God have to do with all of this?

- Certainly we, like the Astrophysicists, do not need him to set the initial conditions, even they are indisputably important?
- But – if there is a God and creator – then he surely left us with a bigger mystery in turbulence than we thought even a few decades ago.
- And we could certainly use his help figuring out what to do next.

References: homogeneous turbulence papers

These are not all easy to find, so write to me at georgewilliamk@gmail.com if interested and can't find it, or check out library at www.turbulence-online.com.

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